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# The Place of Principles in Policy Analysis\*

CHARLES W. ANDERSON  
*University of Wisconsin, Madison*

*Any theory of policy evaluation has to address the problem of the choice of criteria for decision making. In most theories of policy rationality, derived from economic theories of the utility-maximizing individual and a positivist conception of valuation, such values are to be regarded as the "preferences" of the policy maker. The stipulation and ordering of standards of judgment is not considered to be part of policy rationality itself. This conception of rationality is not obligatory. Understanding rationality as having good reasons for an action, and policy judgment as a process of argument, enables us to stipulate certain standards at the metapolitical level which any system of policy evaluation must meet. It is possible to identify a logical sense in which such classic principles as authority, justice and efficiency can be understood as necessary considerations in any rationally defensible policy appraisal.*

In order to make a policy decision, one must invoke some criteria of evaluation. We cannot decide whether a proposal for public action is desirable or undesirable, whether the results of a public program are to be adjudged a success or a failure, except in the light of a standard. We have to make a decision on some basis, for some reason. We can favor a specific program for the reason that the benefits seem to outweigh the costs. We can base our decision on the grounds of legality, or on the grounds that one option appears more politically feasible than the rest. We can decide on the basis of majority will or fundamental fairness or because of self-interest, organizational interest or the interest of some group whose cause we are trying to advance. We can decide on instrumental grounds, that this policy would rectify a balance of payments disequilibrium or provide a consistent, low-cost energy supply. Failing all else, we can make a decision because heads came up rather than tails.

All of these are potential criteria for choice, and all identify standards for public judgment. One could incorporate any or all of them into a model of the decision-making process, and each, except perhaps for the last, has been highlighted somewhere in the policy-making literature as constituting the essence of decision. Nonetheless, the essential question, is not how we can decide, or how we in fact do decide, but how we ought to decide. What counts as a good reason for a policy decision and what is an inappropriate basis for political judgment?

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Any theory of policy evaluation<sup>1</sup> has to address the problem of the choice of criteria for decision making. This is as true for those who would propose models to explain the behavior of policy makers as it is for those whose interest is in trying to clarify what "policy rationality" or "good judgment" entails. However, it is probably fair to say that even purely "empirical" theories of policy process cannot escape the normative implications of policy evaluation. The move from the empirical to the prescriptive is easy and natural for the consumers of policy theory, and in the eyes of students and practitioners, current models of policy making readily become standards for the craft of policy analysis. Such has been the fate both of pluralist interest-group theory and incrementalist models of the budgetary process. (It is very easy for those who look upon political science as a repository of practical wisdom to conclude that it is "good practice" to inflate a budget request by approximately the amount of the anticipated legislative cut.) In any event, the "fact-value distinction" is not a profound stumbling block in policy theory. There are few enough students of policy analysis who are not concerned with the implications of different modes of policy choice and many significant works in the field—those of

<sup>1</sup>Recently, the term "policy evaluation" has been appropriated to designate a sub-specialty within the general field of policy analysis, denoting the technical appraisal of the impact of public programs. I want to reserve the concept for a broader, more essential use, to refer to the process of making deliberate judgments on the worth of proposals for public action as well as on the success or failure of projects that have been put into effect.

Dror, Lindblom and Simon, for example—are overt arguments for preferred strategies of decision.

Logically, the choice of criteria is the first element in any theory of policy evaluation. How we perceive a problem depends on how we propose to evaluate it. Problems are not just “out there” waiting to be resolved. The first act of evaluation is to make a distinction between “problems” and “the way things are.” Poverty is not a problem for a society that believes that “the poor are always with us”—or that they get precisely what they deserve. Inflation can be regarded as a “condition” rather than a “problem,” for it means no more than a rise in the price structure. Inflation has to be regarded as a problem for a reason, as, for example, that it affects business confidence, or represents an illegitimate confiscation of some forms of property, or redistributes income in socially undesirable ways. A policy problem, in short, is a political condition that does not meet some standard.

It is not merely in the phase of problem identification that the choice of standards is important. Each step in the process of decision making depends on the initial stipulation of values to be served. We cannot just “weigh” or “compare” policy alternatives. We must weigh and compare them against something. At the end of analysis, we cannot merely make decisions. We also have to justify them. However whimsically or equivocally we *came* to our conclusions, good reasons have to be given for our policy preferences if they are to be taken seriously in the forums of policy deliberation. Ultimately, policy analysis has less to do with problem solving than with the process of argument. The better metaphor for policy analysis may not be the mathematical equation but the legal brief—it is a reasoned case for a preferred course of public action.

### The Stipulation and Ranking of Values

Like the famous recipe for rabbit stew that begins, “Catch the rabbit,” most formulas for rational policy choice begin with the admonition, “Identify and rank your values.”<sup>2</sup> The

problem for any theory of policy evaluation is how this should be done. Contemporary policy theory tends to side-step the issue. Either the selection of criteria is treated in a highly formal and abstract manner or policy rationality is regarded as a kind of economizing activity and a utilitarian conception of political judgment is implicitly or explicitly endorsed. A third alternative is to rely on a “political” conception of evaluation, in which feasibility or social agreement become overriding norms. In any event, in most contemporary formulations of the problem, the stipulation and ranking of values is regarded as more or less arbitrary, as a “given” in the appraisal of policy. The specification of values is not itself part of the process of rational choice. Rationality is defined in a purely instrumental sense, as “goal-directed behavior” (Riker and Ordeshook, 1973, p. 10). As Dahl and Lindblom write (1953, p. 38), “An action is rational to the extent that it is ‘correctly’ designed to maximize goal achievement, given the goal in question and the real world as it exists.”

What virtually all contemporary policy theories have in common is a positivist or “emotivist” theory of evaluation. Values cannot be justified in terms of objective criteria. Hence, they must be regarded as “preferences” on the part of the policy maker. “Technical” or “rational” policy analysis can only begin once relevant values have been stipulated, either by an authoritative decision maker or through the statement of citizen preferences in a democratic political process. It is impossible to specify what standards ought to be taken into account in rendering a policy evaluation.

This, of course, is not the case. To be regarded as “reasonable,” a policy recommendation must be justified as lawful; it must be

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3. He then lists all important possible ways of—policies for—achieving his goals

4. And investigates all the important consequences that would follow from each of his alternative policies,

5. At which point he is in a position to compare consequences of each policy with his goals

6. And so choose the policy with consequences most closely matching his goals.

<sup>2</sup>Charles E. Lindblom's formulation (1968, p. 13) of the idea of policy rationality will do for an example:

1. Faced with a given problem,
2. A rational man first clarifies his goals, values, or objectives, and then ranks or otherwise organizes them in his mind;

For similar formulations, see Dror, 1968, pp. 129–49; Downs, 1957, p. 6, and Zeckhauser and Schaefer, 1968, p. 29. Pragmatic and incrementalist theories differ only in that standards are to be identified and ordered after the fact, in the light of the consequences of policy. See Brecht, 1959, p. 192.

plausibly argued that it is equitable and that it entails an efficient use of resources. Else it is subject to legitimate criticism.

It does appear that there are certain fundamental considerations that must be accounted for in any policy evaluation, a set of problems that can be identified with the classic concerns of political theory and with a repertoire of basic concepts including authority, the public interest, rights, justice, equality and efficiency. As standards of policy evaluation, these are not simply preferences. They are, in some sense, *obligatory* criteria of political judgment. To justify any policy recommendation, one must argue that it is within the legitimate powers of government, that it is, in some sense, "in the public interest," that it is consistent with lawful rights, that it is fair, and efficient in the use of resources.

The view that "comprehensive" policy rationality is impossible, as argued by Braybrooke and Lindblom (1963), March and Simon (1958) and others, then might not be precisely correct. While there may be an "infinite number of evaluations" for any policy question (as Lindblom argues in criticism of the classic rational model), it may be the case that there are only a limited number of values appropriate to address in rendering a policy evaluation. All policy argument leads back, eventually, to a finite and bounded set of classic principles and problems of political evaluation.

Furthermore, it may be that instrumental rationality is not the only conception of rationality pertinent to policy theory. The sense of rationality as "goal-directed behavior" is perfectly formal; it applies to *any* type of decision and not in any specific sense to *policy* decisions. It is an ideal of formal, logical validity that a given conclusion can be shown to derive from a given set of premises. However, formal validity is not the only tenable meaning of rationality in modern theories of logic. In the spirit of Wittgenstein's *Philosophical Investigations*, modern logicians like Stephen Toulmin (1969, p. 188) have argued that the problem of rational assessment—of telling sound arguments from untrustworthy ones—depends on the characteristics of an arena of discourse and the standards that are pertinent to it; or they have argued with Friedrich Waismann (1951, p. 117) that the known relations of logic can only hold between statements which belong to a homogeneous field of discourse. Thus, it may be possible to say more about *policy* rationality than about rational choice in general, and it may be that certain standards of judgment are necessary to rational policy choice because of the very nature of what we do when we make,

and appraise, public decisions.

In the development of contemporary policy theory, what happened was that the ideal of the utility-maximizing individual was simply taken from positive economic theory and applied to the role of the policy maker. Policy rationality then becomes efficiency in the pursuit of any set of utilities. However influential this instrumental conception of rationality may have been in defining the dominant models of contemporary policy analysis, it is by no means the only acceptable use of the term. In modern analytic philosophy, it is also possible to conceive of rationality as performing an action for reasons that an individual correctly or reasonably regards as good reasons (Benn and Mortimore, 1976, p. 2). This conception of rationality as acting for good reasons leads to two related senses in which an action can be called rational. Either we ascribe rationality to individuals who act for reasons which they regard as good, or we call actions rational when we judge that there are reasons to support them, as when we say, "That seemed a rational thing to do." This conception of rationality seems particularly germane to the process of policy discourse and policy argument. And when we consider what would count as a good reason in policy discourse, it would seem that we are inevitably led back to the classic concerns of political evaluation and the classic principles of political thought.

However, merely to invoke the "classic principles" as criteria for policy evaluation is not to have done very much. To talk about "justifying" decisions in the light of such principles seems to lead us down a familiar and none-too-promising path. We begin to ask questions like "What is justice?" and "What is the public interest?" seeking an essentialist definition that would serve as a conclusive test of the propriety or impropriety of public action. It is not my intention to propose an idealist theory of policy evaluation. I want to stay safely within the positivist and pragmatic tradition that has been dominant within this field of political theory, though perhaps to reveal some unexpected implications of what is possible within that tradition. To this end, we must be quite clear about the kind of problem involved in the selection of criteria for policy making and in defining the logical status of the classic principles in any theory of policy evaluation.

### The Pluralism of Policy Languages

We can begin from a consideration of a practical problem of policy appraisal. If, as I have argued, standards of evaluation are em-

bedded in specific fields of discourse, if they arise from specific paradigms of inquiry and analysis, then our sense for what counts as a good reason and as a mistake depends on the ideological, disciplinary, or cultural context within which we are operating. This is a familiar problem, both of the sociology of knowledge and of the philosophy of science, that our "plausibility structures" have much to do with social context and social reinforcement. However, the crucial difficulty is not merely the relativity and contingency of standards that such a view entails. The significant issue is how we should proceed when we are aware that public issues can be perceived and appraised through multiple frameworks of evaluation. Given the diversity of the policy sciences in our times, this has become a characteristic dilemma of policy choice and policy rationality.

The proliferation of "policy professions" in the past generation or so has created a great variety of languages and logics of policy evaluation and justification. In such a field as energy policy, for example, the policy maker or citizen must somehow decide what to make of the various arguments and analyses presented by economists, environmentalists, engineers, scientists, lawyers, and so on, each claiming to be authoritative for a specific aspect of the problem, each justified by its own premises of inquiry and rigorous logic of analysis, each containing imperatives for definitive public action.

In the conventional wisdom of policy making, policy makers specify objectives and policy professionals deal only with means to politically designated ends. However, the conventional wisdom may have the matter almost precisely backwards. The actual role of policy professionalism in contemporary government is probably more prescriptive than instrumental. The setting of standards of good practice is a large part of what professionalism means. Most policy professions are such precisely because they provide standards for public policy. In such diverse fields as forestry, public health, nutrition and welfare, the essential function of the expert is often that of setting criteria for the definition of public objectives and the appraisal of public programs.

Policy evaluation takes place within and between such languages of policy analysis. Like scientific disciplines, policy languages constitute a realm of discourse and of argument. What counts as a "problem," and a "good reason," and as a "mistake" in judgment depends on the normative standards embedded in a specific framework of analysis. The process of policy evaluation and argument is different

in law, macro-economics, diplomacy, environmental planning and civil engineering. In each of these professions, different rules exist for the identification of the problematic; there are different norms of evaluation and different criteria for what would count as a "solution" to a public problem, for how we would distinguish a "successful" public venture from a policy "failure."

This pluralism of policy languages does not pose any ultimate problems of justification in itself. All arguments trail back eventually to certain normative postulates and assumptions widely shared in liberal political discourse (and arguably, by Marxian and Catholic corporatist thought as well). When we *justify* a policy standard (address the question of why it is appropriately invoked as definitive in making judgments about alternative public projects and purposes) we generally appeal to some higher-order principle with which the criterion of choice is deemed to be consistent (Taylor, 1961, pp. 45-46). Thus, the neoclassical economist, when asked why "market failure" is an appropriate (and perhaps exclusive) test of the propriety of government intervention in the economy, will refer back to the values of freedom and efficiency served by the market mechanism and behind this perhaps to some notion of the moral autonomy of the individual and to a nominalist skepticism toward the possibility of objective standards of public judgment apart from individual preference and wants. Similarly, if one inquired of the committed conservationist why it might not be a plausible energy policy alternative to simply use up all fossil fuels in one generation of grand bacchanalian extravagance and let the future fend for itself, appeal would probably be made, in the final analysis, to some notion of justice between generations (Barry, 1977).

It is not then that the diverse policy professions do not rest on a shared set of justificatory principles. Rather, the problem is that the weight, emphasis and significance given to the various fundamental principles is different in each. Because they endorse different standards of argument (what counts as a good reason) and because they are organized to account for different facets of experience, they entail different orderings of relevant values. Thus, the central normative construct of law is rights, while that of economics is efficiency. While it might be possible to argue considerations of efficiency in delimiting the scope of an established right (a court might plausibly argue potential bankruptcy as a ground for denying counsel to indigent defendants), in legal reasoning, the ordinary procedure for establishing the

categories and scope of rights is not that of a calculus of individual utilities. Conversely, while the logic of economic reasoning may rest on certain postulates of rights (such as property rights), the purpose of a calculation of alternative utility functions is not to stipulate a system of rights.

Hence, it makes a great deal of difference whether we choose to adopt the rules of rational discourse of one of these policy professions rather than another in evaluating a policy problem. We may come to very different conclusions if we choose to regard a specific issue as falling in the domain of law rather than economics. As Arthur Okun (1975) suggests, it makes a difference whether we regard a problem like income distribution from the point of view of rights rather than that of efficiency. There is, he suggests, a "big tradeoff" between these two considerations, either of which can be regarded as definitive for policy making.

In such a pluralistic universe of policy discourse and policy rationality, the crucial problem for a theory of policy evaluation becomes that of identifying standards by which we may weigh the claims of various languages of policy analysis. On what grounds should we choose to regard a problem from the point of view of one of the policy professions rather than another? What justifies the choice among the several logics of policy evaluation?

This is to put the "metapolitical" problem in applied form. Consider the following situation. A labor leader is deciding whether to declare an illegal strike. If the decision is made to strike on the grounds that the probable benefits would be greater than the probable costs, we would say that the decision was based on economic reasoning. If the strike is not called because of its illegality, we would say that the decision was based on legal grounds. But what guidance can be given as to whether the leader ought to decide for economic or legal reasons?

Hence, the problem that must be faced in constructing a theory of policy evaluation is also a characteristic practical problem of policy making in our times. How do we judge a judgment? What standards must any system of policy evaluation meet? The problem concerns the justification of the procedures of policy analysis, but it also addresses the concrete question of how we weigh the rival claims and considerations adduced by different "rationalities" of policy appraisal.

### The Metapolitics of Policy Evaluation

A few political scientists have begun the

process of trying to extend the idea of policy rationality, asking what is logically required in any stipulation of criteria for policy evaluation. For example, Brian Barry and Douglas Rae (1975, pp. 337-401) set forth seven requirements that any system of political evaluation must meet. Their standards derive largely from formal decision theory. They are rules that must be followed if we are to judge rationally among alternatives in the light of standards.

For Barry and Rae, the formal tests of adequacy are these. The standards by which a policy is judged must be *internally consistent*, that is, transitive. It is logically impossible to come to conclusions about preferred policy unless the criteria are clear and hierarchically ranked. Standards must be *interpretable*: we must be able to tell what a standard means in policy discourse, to judge when it has been met or fails to be met. It must be possible to *aggregate criteria*, to rank them, for a variety of standards might be relevant to choice; yet we cannot simply say, for example, that we favor both, say, equity and efficiency, for no alternative may satisfy both criteria equally.

Any theory of policy evaluation, for Barry and Rae, must also account for the problem of forced choice, that not to decide is in fact a policy choice, and there is no a priori reason why the status quo should have a privileged position in policy appraisal. A system of political evaluation must acknowledge *risk and uncertainty* (we cannot say with confidence, that a given choice will actually meet standards) and it must deal with *time* (policies will have both proximate and distant consequences). Finally, choices must be logically justified in terms of some conception of *individual welfare*.<sup>3</sup>

Duncan MacRae (1976) has attempted to develop a "metaethics" for normative policy discourse on the basis of an analogy between the logic of policy evaluation and the rules of scientific inquiry. MacRae suggests that normative discourse can be as rigorous as empirical investigation in politics, that it is not mere assertion, but can be seen as disciplined inquiry to be judged by "higher-order" standards of appropriate procedure. MacRae envisions normative discourse as debate between the proponents of rival evaluative systems. The stan-

<sup>3</sup>Barry and Rae also take up a rigorous critique of certain specific criteria of decision, including utilitarianism, equality, Pareto optimality, majority, minimax and dominance and they do consider the place of principles in political evaluation, about which more further on.

dards that any system must meet, the grounds of appropriate criticism of any normative judgment, include (1) *generality*, if the proposed system fails to apply to a choice about which both discussants have moral convictions and to which the critic's system does apply; (2) *internal consistency*, in which the proponent's system yields contradictory recommendations where the critic's does not; (3) *inconsistency with shared convictions*, that the proposed system leads to conclusions at odds with presumably shared evaluations.

I offer no critique of the position of either Barry and Rae or MacRae. Both have extended and clarified the domain of rationality in policy evaluation. Yet, I think the logic of their analysis can be carried further. They emphasize procedural norms, formal rules of consistency and logical rules of validity in rational choice and ethical judgment. However, it seems possible to continue this line of logic to assimilate certain substantive principles that distinguish policy judgment from the more general language regions of rational choice or normative discourse. Barry and Rae, to be sure, do take up the place of the classic principles in policy analysis. However, they view such concepts as justice, rights, the public interest, etc., primarily as "simplifying devices," culturally derived constructs that somehow pull together and focus attention on certain facets of our shared political experiences. The logical status of such principles is somehow different from that of the formal norms of rational decision making. My own view is that something more can be said for and about the place of such principles as "metapolitical" standards, as criteria for judging the worth of any system of policy evaluation.

#### The Place of Principles in Policy Analysis

It is not immediately apparent that any systematic relationship exists among the classic principles of political thought. Words like *right*, *the public interest*, *justice*, *freedom* and *community* can seem to be no more than an array of discrete "good things." We are used to the idea that political argument can fasten arbitrarily on one or a few of these concepts and that they can be arranged in different patterns in ideological thinking, invested with a variety of meanings and given different degrees of emphasis.

To make the point that the criteria which enter into a policy evaluation are not merely preferences, but that certain substantive standards need be accounted for in any rationally

defensible policy judgment, one must show that certain principles do form, in some sense, a finite and bounded set and one must establish in what specific way such criteria are mandatory in the activity of policy evaluation. It is also necessary to show that these principles can be stated in such a way as to be free from any partisan or ideological quality. They must reflect no more than the logical characteristics of any form of political judgment.

The essence of my argument is this: a defensible policy judgment must meet certain formal standards of rational choice—it must have regard for such problems as the transitivity of values and forced choice—but it must have other qualities as well. By virtue of the fact that these are policy judgments, certain political values must be taken into account. It is not that these values can be justified in any ultimate sense. I will not argue the Kantian position that there are certain normative propositions that any rational thinker must endorse to remain consistent, nor will I argue, with Rawls, that there are certain principles that self-interested individuals would be led to accept if placed in the "original position" and called upon to formulate basic norms of political life. Rather, the argument is that certain criteria of choice are inherent in the activity of politics itself, that they are part of what we mean by "making a political judgment" or as Wittgenstein might have put it, that they are part of politics as a "form of life."

My case is that a specific set of principles is *obligatory* in political evaluation in a very special sense. It is important to be very clear about the logical status of such principles as criteria of "metapolitics"—standards for judging the adequacy of any system of political judgment. There is a difference between claiming that certain *concepts* are indispensable in political evaluation and that certain *propositions* are. To say that a comprehensive policy analysis must account for the problem of authority is not to say that a binding policy recommendation must be shown to "derive from the will of the people." As Stephen Toulmin says (1972, p. 418),

it is one thing to argue that a certain concept is something that a rational thinker or agent cannot very well get along without; it is quite another to establish the necessary truth of certain substantive ethical or scientific principles.

As standards at the "second level," as criteria for evaluating a policy evaluation itself, the classic principles have a particular standing. They are not norms of conduct; rather, they

identify those aspects of a policy recommendation that require justification. The status of such principles in normative policy theory is not, unlike the standing of a hypothesis in scientific inquiry. As a hypothesis *requires* experimentation to test its validity, so "principles," in the sense I shall be using the term, require justification to demonstrate the rational plausibility of a policy recommendation.

Taken in this sense, it is possible to identify three of the classic principles that appear to be rationally requisite to the construction of a defensible policy evaluation. These are authority, justice and efficiency. The relationship of these to other fundamental political ideas can also be specified. Nonetheless, this statement is to be taken as exploratory and provisional. While I think it is possible to demonstrate that these considerations, *at least*, have to be accounted for in the process of policy evaluation, it is possible that a similar case can be made for other principles as well.

**Authority.** In the most general sense, authority means that an exercise of power is rightful, which is to say, justifiable. This implies that good reasons can be given for the act of policy making, that an entitlement can be established to decide on the public behalf and a concomitant obligation to abide by such decisions. Thus understood, authority is a necessary characteristic of any legitimate policy decision and a requirement of any system of policy evaluation. A decision which cannot be justified in terms of this standard is simply an act of domination or coercion.

Put another way, as a logical characteristic of any system of policy evaluation, authority functions to structure the terms of the public debate. It locates the burden of proof in political argument. The logic of public policy making requires that the burden of proof rest always with the state (Neuman, 1953, p. 904). It is up to government to justify its actions; it is not up to citizens to demonstrate why policy makers should not do precisely as they please. It is hard to imagine how the presumption could be reversed and retain any meaningful sense of political discourse. If we were to accept that public action might just as well be whimsical and arbitrary, no problem of political evaluation really arises.

To justify a policy evaluation, then, reasons have to be given for regarding a problem as properly resolved through public action. In liberal political thought, the problem of authority is systematically related to such principles as individual freedom and consent. In

liberal discourse, the problem of justification takes the form of giving good reasons for overriding the presumption in favor of individual autonomy. Any legitimate public action must be shown to derive, in some sense, from consent or individual wants and interests. However, the problem is not at all peculiar to liberalism. "Divine right" was, after all, a formally rationalized justification for the exercise of authority. Sixteenth- and seventeenth-century doctrines of sovereignty were an attempt to "give reasons" for unlimited authority. Marxism is an elaborate critique of the consistency of the liberal notion of authority and provides an alternative system of criteria for regarding public action as appropriate or inappropriate. (In fact, Marx, like Rousseau, can be regarded as doing no more than making the liberal presumption in favor of freedom a more stringent test of the legitimacy of public action.)

Even the so-called "irrationalist" political theories, those that *argue* that rationality is far from a definitive force in human affairs, rest paradoxically on a reasoned justification for a certain pattern of authority. Thus, A. James Gregor writes (1969, p. 19):

Adolf Hitler's *Mein Kampf* can, in no sense, be conceived as *simply* a statement of the author's personal emotional preferences. Suggestions, admonitions and imperatives are *argued*. The arguments may be elliptical—vital premises may be suppressed—or they may be invalid and their factual premises erroneous, but they are not simply assertions of personal preference. Hitler advocates, for example, the fulfillment of one of mankind's highest aspirations; man's continued evolutionary progress. . . . We raise objections when Hitler contends that the fulfillment of such an ideal demands the caste superordination of a specific biological race. . . . We demand evidence, for example, that would confirm that a given race is the sole repository of man's foremost creative capacities. But such objections raise questions of fact and definition and as such are subject to cognitive appraisal.

In any rational policy evaluation, then, good reasons have to be given for regarding a problem or project as appropriately the subject of public action. In the contemporary languages of the policy professions, this can be and is done in a variety of ways. The neoclassical political economist may argue that "market failure" or bona fide "public goods" are the only grounds for state intervention that legitimately override the presumption in favor of autonomous, individual choice. The utilitarian may appeal to some aggregate social welfare function as an appropriate test that "authorizes" a public action. The social democrat

may argue from some need or desert-regarding conception of distributive justice. The legal positivist may simply hold that a policy option is legitimate if it is apt to be upheld by the courts, while the constitutionalist will admit the propriety of considering a policy option if it squares with the formal powers of government. Whatever the particular reasons offered, the essential point is that *some* justification on the grounds of authority has to be provided. Authority is not simply a "preference" of the policy maker that may be introduced as a criterion for the evaluation of public policy or not as one likes. One simply cannot say, in justifying a policy appraisal, "I don't care if it's blatantly illegal and rides roughshod over established rights; it's still the most efficient way of getting the job done."

There is an intimate connection between the principle of authority, thus understood, and such concepts as freedom, rights and justice. These are grounds of argument, criteria that can be introduced to make claims about authority, or to test propositions about authority. Different conceptions of freedom—the distinction between the "freedom from" imperatives of the classic liberal and the more activist sense of "freedom to" (Berlin, 1969, pp. 118–72)—give rise to different images of legitimate authority. It might be argued that freedom is as much an obligatory consideration in any political judgment as is the concept of authority itself. (Certainly, the very notion of political argument implies a notion of the individual capable of judging the worth of rival claims.) Yet, it is such only in relation to the problem of authority, or justice. We make propositions about human freedom in order to arrive at a conception of legitimate authority. We do not make cases about authority so as to arrive at a conception of human freedom.

The concept of "the public interest" also bears a close relationship to the problem of authority. It is easy enough to demonstrate that there is no definition of the public interest to which all reasonable persons would necessarily repair. It is equally easy to show that any rational policy evaluation must give reasons for regarding a policy proposal as, in some sense, in the public interest. One cannot justify a policy recommendation on the grounds that "it would make me and my friends richer." However refreshing the candor of such an argument might be, it does not and cannot stand as a legitimate warrant for a public action. (When self-interest is argued as a legitimate basis for decision it is always in connection with some theory of how the public interest will arise from the competition or aggregation of indi-

vidual interests.)

The concept of the public interest is itself logically dependent on other fundamental criteria of political evaluation, among them concepts of individual welfare and community. The connection between individual and collective welfare must somehow be accounted for in any comprehensive policy evaluation and it is necessary to specify precisely which "public's" interest is to be taken into account. Any policy appraisal entails a decision about who is properly regarded as "us" and "the others," whose interests are to be promoted and whose thwarted or ignored. It makes a considerable difference if the relevant community in whose "interest" policy is to be made excludes the adjoining class, county, nation, the rest of humanity or the people who will live five centuries hence.

Thus, a rigorous and comprehensive policy analysis must touch a number of bases to establish the propriety of public action. This does not mean that every policy argument must contain a full-fledged case about authority going back to first principles and premises. To think of policy analysis and debate in this way would be tedious indeed. Authority may not be problematic in all exercises in policy evaluation. It may simply be assumed that the available array of alternatives passes the test of legitimacy. There are short cuts and simplifying devices in the ordinary language of any policy debate. What is certain, however, is that if a policy is controversial, it will be so in relation to one of the fundamental dimensions of evaluation. Implicitly or explicitly, a policy recommendation must be regarded as plausible in terms of each of these considerations.

**Justice.** John Rawls (1971, p. 3) states that "justice is the first virtue of social institutions, as truth is of systems of thought." It may or may not be the case that justice must rank first in any ordering of criteria for policy evaluation. Justice might be taken as the paramount criterion for policy evaluation, as a test, say, of rival conceptions of authority. This is the thrust of Marxian analysis and modern theories of civil disobedience, which suggest that a system of government is not rightful if it can be shown that prevailing standards of authority do not permit the consideration of the more equitable policy alternatives. On the other hand, one can argue, with Jimmy Carter, that "the world is unfair," that government should deal only with those cases of inequity that meet the tests of authority and efficiency (and therefore, that welfare funds need not be made available to pay for abortions). It does not seem possible to



specify a *necessary* priority between the principles of authority and justice. However, this does not mean that the ranking of these standards of policy evaluation is simply a matter of the policy maker's preference. Good reasons must be given to justify assigning priority to one or the other of these values. Furthermore, it would seem possible to show that justice is not an optional but a necessary consideration in any system of policy evaluation.

This is the case not only because any public decision will have distributional consequences but also because it is inherent in the logic of policy making that rules must either be stated universally or so as to apply to particular cases and categories. The maxim of justice, "Treat like cases alike and different cases differently," stands then not as an ethical imperative but as a statement of a problem that requires justification: it raises the question of what counts as a good reason for regarding cases as alike or dissimilar. As Benn and Peters (1959, p. 99) put it: "If, for example, we believe in democratic rights for white men, we must show good grounds for denying them to black, and the simple criterion of skin color alone is not an obviously relevant ground of distinction. There may be other and better grounds—but the onus of proof rests on those who would limit the right, not on those who would give it universal scope."<sup>2</sup>

The concept of equality then is probably best regarded not as an independent consideration in policy evaluation, but as a proposition that stands in a logical relationship to the problem of justice (Perelman, 1963, pp. 1–60; Bedau, 1971; Benn and Peters, 1959, pp. 107–34). In liberal argument, equality locates the burden of proof in making cases about justice, as freedom does in relation to the problem of authority. All individuals are to be regarded as in the same position with regard to policy unless good reasons can be given for treating them differently. Grounds for differentiation may include need or desert, merit, compensation, or some public interest criterion (as in the familiar appeal to the need for incentives as a spur to productivity and economic growth, which stands as a proxy for aggregate social utility in most modern economic analysis). To my knowledge, there has never been a pure egalitarian argument that cases are to be treated alike regardless of circumstances. Most contemporary egalitarian arguments rest on the premise that existing inequalities are unjust, and that failing a reasonable justification for differential treatment, cases should be treated alike. Thus it seems

appropriate to regard justice and not equality as the necessary consideration in any system of policy evaluation.

The point then is that any policy evaluation must include a justification of the categories of universal or differential treatment to be established (though liberals might argue that equality requires no justification) (Feinberg, 1973, pp. 98–102). Once again, it does not make sense to regard justice simply as a preference of policy makers, a consideration which they are free to incorporate into a policy evaluation or not as they wish. One cannot really vindicate a policy recommendation by asserting, "I know this program is unfair, but it is efficient, and we clearly have the authority to enact it."

**Efficiency.** It is easy enough to show that efficiency is a necessary consideration in any system of policy evaluation. Means must be appropriate to the ends chosen, and it is a legitimate criticism of any policy recommendation to demonstrate that there are better alternatives for achieving stipulated values. What is more important is to demonstrate that efficiency cannot be the exclusive consideration in rendering a policy appraisal.

In many models of policy evaluation, efficiency is regarded as tantamount to rationality. The problem of policy is "solved" when it can be demonstrated that one alternative yields an optimum level of benefits over costs. As we have noted, rationality is often defined in an instrumental sense, as "goal-directed behavior" in policy theory. Yet, some conceptions of policy rationality carry the matter even further. They argue that the efficient solution to a policy problem is also the solution that is necessarily in the public interest. In fact, latent in the logic of much of contemporary political economy, whether founded on neoclassical or utilitarian premises, is the notion that to solve for the problem of efficiency is simultaneously to solve for the problems of authority and of justice.

The cost-benefit analysis is a fundamental paradigm of much contemporary policy analysis. In the most general sense, any cost-benefit analysis rests on a utilitarian foundation. The only appropriate (or possible) criterion of policy is individual wants and interests, somehow aggregated to yield a social utility function. Any policy which reduces social costs or increases net social welfare is "beneficial." The standard of policy evaluation may be cast in terms of Pareto optimality: a desirable policy is one which makes someone better off without making anyone worse off (or in terms of the looser formulation, a desirable policy is one

where winners could conceivably compensate losers through costless transfers). In either case, a solution to the problem of efficiency also yields a solution to the problem of the public interest.

Thus, it can be argued, as it has been, that cost-benefit analysis can be justified as a mode of policy evaluation, that it accounts for those considerations that are necessarily incorporated in any rational system of policy appraisal. However, this is not a necessary conclusion, and in fact most economists do not understand the force of cost-benefit analysis in this way. Efficiency is better identified as one element, but not the exclusive element in policy discourse. As E. J. Mishan says (1973, p. 13),

It cannot be too strongly stressed, however, that even the result of an ideally conducted cost-benefit analysis does not, of itself, constitute a prescription for society. Since it simulates the effects of an ideal price-system, the ideal cost-benefit analysis is also subject to its limitations. Any adopted criterion of a cost-benefit analysis, that is, requires *inter alia* that all benefits exceed costs, and therefore can be vindicated by a social judgment that an economic rearrangement that *can* make everyone better off is an economic improvement. The reader's attention is drawn to the fact that such a judgment does not require that everyone is actually made better off, or even that nobody is actually worse off. The likelihood . . . that some people, occasionally most people, will be worse off . . . is tacitly acknowledged. A project that is adjudged feasible by reference to a cost-benefit analysis is, therefore, consistent with an economic arrangement that makes the rich richer and the poor poorer. It is also consistent with manifest inequity, for an enterprise that is an attractive proposition by the lights of a cost-benefit calculation may be one that offers opportunities for greater profits and pleasure by one group, in the pursuit of which substantial damages and suffering may be endured by other groups.

In order, then, for a mooted enterprise to be socially approved, it is not enough that the outcome of an ideal cost-benefit analysis is positive. It must also be shown that the resulting distributional changes are not regressive, and no gross inequities are perpetrated.

Even an ideal cost-benefit analysis, then, is normatively incomplete; it cannot escape criticism on the grounds that are pertinent to any policy appraisal. However, this is not the only difficulty with the kind of utilitarian calculation that cost-benefit analysis represents. Since it is impossible in practice to know and to aggregate individual utilities, any concrete exercise in cost-benefit analysis requires a stipulation of the costs and benefits to be taken into

account. The values postulated, and their ranking, has to be defended on *some* basis. Alasdair MacIntyre (1977, p. 226) points out that "the use of a cost-benefit analysis clearly presupposes a prior decision as to what is a cost and what a benefit" and that this decision has to rest on some ground other than utilitarianism itself.

It is not possible to derive a complete policy evaluation from any form of purely economic analysis. Efficiency is properly regarded as one ground of policy evaluation, a necessary one to be sure, but not as the exclusive consideration in policy appraisal. Efficiency is best regarded as an instrumental value, a tool for comparing policy options in terms of other values. In fact, in any cost-benefit analysis, we do not compare all alternatives but only those that have survived scrutiny on other evaluative grounds. In making an economic analysis of different road-building techniques, we do not draw up a neat comparison of the costs and benefits of *corvée* and slave labor in relation to more conventional methods. In terms of our standards of authority and justice, these options are simply out of bounds.

If efficiency is properly regarded in this instrumental sense, then it is a lower-order criterion of political judgment, basically a "tie-breaker" between policy options that have passed minimum tests of acceptability on grounds of authority and justice. If this is in fact the case, then it is possible to specify not only the values that have to be taken into account in any comprehensive policy evaluation but to advance certain propositions about their ranking as well. Though it is difficult to stipulate a logical priority between authority and justice (some would argue that justice is the supreme test of the propriety of any exercise of public authority, others that government should only act with regard to those forms of injustice that fall within its legitimate powers), it is possible to say that efficiency can never be assigned a higher priority than authority or justice in any plausible value ordering. Again, we simply cannot argue, "I realize that this policy is unjust; nonetheless, it is the most efficient way of achieving our objectives."

The propositions I have outlined would seem to raise problems of democratic theory. If individual wants, somehow aggregated into "public wants," are not to be regarded as a definitive test of policy, then neither, it would seem, is "majority will." To be sure, in any democratic polity majority will, expressed through prescribed constitutional mechanisms, is accepted as the definitive test of authority. A policy is legitimate insofar as it reflects the

"will of the people." However, unless we are very dogmatic "absolute majoritarians" we do recognize that to regard majority will as the exclusive standard for the evaluation of policy is both paradoxical and unsatisfactory. Within most versions of democratic theory, it is an appropriate criticism of majority will to argue that what the majority wants is unjust (as, for example, if the majority wills genocide against a minority) or inefficient (if the policy preferred by the majority would lead to national bankruptcy). The puzzles and quandaries that arise in relation to any simplistic conception of democratic theory suggest that the majoritarian principle cannot be the exclusive ground of justification. In fact, of course, within any democratic polity, we do judge the worth of majoritarian policies on other grounds.

### Conclusion

The problem for a theory of policy evaluation is not to discover the right policies but to establish the appropriate grounds for decision making. Not all reasons are good reasons for a policy decision. One cannot justify a public decision simply by invoking a schedule of personal preferences. A justifiable policy evaluation must meet certain standards; it must address specific problems that are characteristic of the activity and enterprise of politics itself.

To say that a policy is justifiable is not the same as saying that one will endorse it. Rather, it means that we regard the basis on which it was made to be plausible, or rationally defensible. A plausible policy argument is one worth taking seriously in the public debate. An impartial observer would weigh its claims and a critic would feel obliged to respond with careful counter-argument.

There are in fact only a finite number of kinds of reasons that can and must be given in justification of a policy recommendation, a logically delimited set of grounds that are appropriate to the appraisal of public policy. It is possible to set standards of comprehensiveness in policy evaluation, to identify those questions that must at least be addressed if a policy appraisal is to appear "worthy of consideration" in our eyes.

It is not simply the stipulation of values to be served that requires reasoned justification but their ordering as well. One cannot state a transitive ranking of values simply as a matter of preference. If efficiency is to be regarded as more than an instrumental value, reasons will have to be given for regarding the most efficient solution as also within the range of "just" alternatives, and as in some sense "in the public

interest."

It might be argued that this conception of policy rationality does not transcend our ordinary sense for what is at issue when we face dilemmas of public choice or when we criticize prevailing notions of how public issues ought to be perceived or analyzed. I admit that my discussion of the limitations of instrumental rationality is hardly novel and my conception of the critical political principles unexceptional. And that is precisely the point. There is something wrong when our dominant models of policy rationality are so obviously at odds with what we in fact take to be fundamental considerations in the deliberation of policy choices. My object has simply been to make explicit what we do regard as legitimate grounds of criticism of any framework for policy choice, and to suggest that our "ordinary language" is probably not deceptive, that certain standards are inherent in the logic of policy evaluation itself.

It can also be argued that this analysis has not "solved" the problem of policy choice, that it provides no prescriptive tests which the practitioner might apply in making decisions about desirable and undesirable public purposes or options for action. That, of course, was not in the cards. In order to reduce policy analysis to a routine, rather than an object of deliberation, one must state criteria of judgment in the form of rules. As Barry and Rae suggest, it is a requirement of any "first-order" system of evaluation that standards be interpretable, that we be able to gauge clearly when a policy meets the mark or fails to do so. The sense of policy rationality described here does not pertain to decisions of this type. Rather, it comes into play at the second, or "metapolitical" level, when we are trying to decide how to decide, when we must face the decision of which system of standards to apply to a given situation.

We face a genuine dilemma of decision only when we are aware that public purposes can be perceived and appraised in more than one way. As I noted, given the proliferation of policy languages in our time, we increasingly face quandaries of this type. So long as we are operating within a bounded framework of policy evaluation, we face no such perplexity. We can "justify" first-order decision rules according to the higher-order value propositions with which they are deemed to be consistent. It is when we understand that different frameworks of evaluation could be applied to a given situation of policy choice that we face a decision of a different kind. The standards that apply to such a judgment have a different

logical character from those that apply to first-order policy choices. We must invoke criteria that test the adequacy of any system of policy evaluation. The requirements of formal decision theory, as described by Barry and Rae, pertain to decisions of this type. So do the rules of normative consistency proposed by Duncan MacRae. My point has been that a certain conception of the classic political principles, understood as necessary considerations rather than as explicit normative propositions, is essential to complete this expanded sense of policy rationality.

The classic principles, then, have a place in policy theory that is very much like that of the norms of inquiry in scientific investigation. Thus, the sense of the concept "validity" in scientific discourse is not unlike that of "justification" in policy evaluation. Validity has no clear, incontestable meaning as a standard of science and different disciplines endorse different rules concerning what counts as a valid scientific argument and what does not. By the same token, different policy languages and professions endorse different notions of what counts as a justifiable policy proposal.

One basic norm of scientific inquiry is that propositions be stated in "operational" form, that one can at least imagine an objective procedure that would identify the presence or absence of a phenomenon. Again, different scientific disciplines endorse different standards regarding the kind of scientific evidence that is appropriate or obligatory, what kinds of reasons can or must be given to establish the plausibility of a scientific hypothesis. In the same way, the maxim of justice that "like cases be treated alike and different cases differently" establishes no necessary test of the justice of a public project. Different policy languages endorse different standards regarding the equity considerations that would establish the plausibility of a public proposal. Yet just as *any* scientific argument must account for the problem of experimental demonstration, so also *any* given policy argument must account for the problem of the propriety of universal or differential treatment of cases.

There is no final justification for the norms of scientific inquiry or for those of policy evaluation. There is no way of showing that a policy *must* be equitable any more than it can be shown that a proposition *must* be verifiable. What can be said instead is that just as there is nothing binding about the norms of scientific inquiry unless we are doing science, there is also nothing binding about the norms of political discourse unless we are engaged in policy evaluation.

Inevitably, the policy sciences will endorse some conception of how one ought to proceed when faced with a problem of public choice. The function of policy theory is not merely to explain why we get the kind of public policies that we do, but to provide guidance as to what is involved in thinking through a public problem to a reasonable conclusion. The very notion of policy evaluation implies that we will make judgments about desirable and undesirable public purposes and projects on some grounds.

The models of instrumental rationality that have provided our dominant strategies of decision do not adequately represent what we acknowledge to be at issue when we confront public decisions. Models of decision which place the stipulation and ordering of the values which will serve as objectives and constraints on choice outside the realm of policy rationality provide a truncated and unsatisfactory foundation for policy theory. They place the policy analyst in the position of an agent to an authoritative policy maker. The mission of policy analysis becomes that of maximizing the evaluative preferences of a particular client. Either that, or one invokes some equally problematic norm of "public responsiveness" which similarly evades the fact that in policy making we do debate the grounds on which public issues ought to be decided. However, it has generally been assumed that no other moves were possible without introducing some absolute criteria of decision, which would violate the fundamental norms of scientific neutrality and rational generality on which any comprehensive theory for the policy sciences would have to rest.

This analysis suggests that it is possible to treat the question of specifying the substantive standards that are properly introduced in any comprehensive policy evaluation as well as the rules that appropriately guide choice for any stipulated set of values along lines that are perfectly compatible with the fundamental norms of modern policy theory. In doing so, it opens up the possibility of an alternative model of policy rationality, one in which policy making is understood as a process of reasoned deliberation, argument and criticism rather than a pragmatic calculus. However, this more comprehensive vision of policy choice does not displace but rather complements the formal requirements of instrumental rationality. Furthermore, it does not imply that institutional and sociological factors are unimportant elements in the calculus of choice. Rather, it is simply explicitly recognized that to follow dictates of situational or strategic necessity is not a sufficient criterion of good political

judgment. Finally, by suggesting that normative political argument has its own strenuous rules and standards, one defines a rather precise relationship between the perennial concerns of political philosophy and the paradigms of practical reason which the policy sciences aspire to develop.

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# The Parties Come to Order! Dimensions of Preferential Choice in the West German Electorate, 1961-1976\*

HELMUT NORPOTH  
State University of New York, Stony Brook

*Spatial analyses of party systems typically construe the dimensions of party configurations in terms of policy-defined issues and ideologies such as the left-right framework. But as far as the mass public is concerned, perceptions and evaluations of parties in those terms require a degree of political sophistication beyond the grasp of the average voter. That is why this article explores the impact of alternative criteria. Its main thesis is that in multiparty systems with coalition governments the party alignments in government—and possibly in opposition—provide a cue to the public of great consequence for its preferential choice. Party preference orders of the West German electorate are examined by way of unfolding analysis. The data come from a series of surveys collected during the period 1961-1976. The results of the analysis reveal traces of the two dominant cleavages of German politics, namely social class and religion, but fail to show any significant influence of the left-right framework. What is most conspicuous is the susceptibility of preferential choice to cues emanating from the coalition behavior of party leaders. Given a context of changing coalition alignments among parties in government, that susceptibility prevents any fixed configuration of parties from governing voters' preferential choice.*

Few areas of political inquiry have witnessed such a rapid flowering in recent years as the area of spatial analysis of party systems. The stimulus of Downs' (1957) work combined with the spread of multidimensional scaling techniques has spawned a rich body of configurational analyses, depicting the location of parties, groups, issues, candidates and, sometimes, voters in joint geometric spaces (e.g., Weisberg and Rusk, 1970, and Budge et al., 1976). Such analyses would seem especially useful for the analysis of party distances and cleavage dimensions in multiparty systems. They provide a map of party locations in the political space and, using that map, we might be able to explain both the coalition building among parties in government and electoral choice among voters.

The Downsean theory, like most spatial analysis, assumes that the dimensions defining the political space are ideological in nature, with the left-right continuum being the most prominent dimension; at least such analyses refer to policy issues of high and lasting significance. Voters are supposed to perceive

parties (and candidates) as located along those policy-defined dimensions and to select whatever party (or candidate) is least distant from their own locations. Stokes (1966) has questioned the key premises of this notion, and Converse (1966) has called attention to the intricacies of the "party distance" notion. Few, if any, spatial analyses have actually tried to test how well *individual* preferential choices fit the "ideological distance" model and none has demonstrated such a fit. The typical procedures used in multidimensional analysis are not suitable for such a test, anyway.

It is the goal of this article to explore the contribution of nonideological criteria to voters' preferential choice among parties. As Stokes (1966, pp. 170-71) in his critique of the Downsean model suggested, "position issues" may affect partisan choice far less decisively than "valence issues," which link "parties with some condition that is positively or negatively valued by the electorate." However, the parties' perceived competence to govern and to achieve goals deemed important by the public, or the personal appeal of party leaders, have rarely been introduced as *explanatory* variables into spatial analysis.<sup>1</sup> In multiparty

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<sup>1</sup>Rabinowitz (1978, p. 809), in a spatial analysis of presidential candidates, makes the interesting point that the personal appeal of candidates may be taken into account without adding another dimension to a configuration defined in terms of political issues. He observes that "the more appealing the candidate, the

systems, as Lehmbruch has suggested (1969, p. 299), the definitions of party distances as provided by the elite members of the various parties may greatly shape voter evaluations of parties. Alignments of parties in government, or in opposition, supply perhaps the most visible clue to the voters for judging similarities and distances between parties (Pappi, 1973, p. 202). Even where no party coalition governs, voters may take important cues from party leaders' pronouncements regarding possible alignments with other parties.

This article pursues this suggestion in analyzing the party preference orders of West German voters. The German Federal Republic has been governed by a succession of coalition governments. Since 1961, the pattern of a coalition composed of two parties and opposed—within the Bundestag—by a single party has not been disturbed, although coalition partners have changed. The data to be analyzed consist of preference rankings of the West German parties. The series of surveys of the West German electorate available for analysis makes it possible to extend the proposed inquiry over a considerable span of time (1961–1976).<sup>2</sup> These surveys contain a rich body of comparable preference data on the German parties, providing a good opportunity to address questions of change.

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more he is located toward the population center; and the less appealing, the farther he is located from the population center." For an attempt at projecting such dimensions as personal appeal or competence into a spatial configuration, see the author's scaling analysis of West German party leaders (Norpoth, 1977; an English version can be obtained from the author).

<sup>2</sup>The data used in this analysis were made available by the *Zentralarchiv für empirische Sozialforschung* in Cologne, Germany, through a joint project with the *Zentrum für Umfragen, Methoden und Analysen* in Mannheim and the ICPSR in Ann Arbor, Michigan. The 1961 survey (ZA no. 056, ICPSR no. 7100) was conducted by Gerhart Baumert, Erwin K. Scheuch and Rudolf Wildenmann; the 1965 survey (ZA no. 556, ICPSR no. 7103) by Max Kaase and Rudolf Wildenmann; the 1969 survey (ZA no. 426/427, ICPSR no. 7098) by Hans D. Klingemann and Franz U. Pappi; the 1972 survey (ZA no. 635/636/637, ICPSR no. 7102) by Manfred Berger, Wolfgang Gibowski, Max Kaase, Dieter Roth, Uwe Schleth and Rudolf Wildenmann; and the 1976 survey (ZA no. 823/824/825, ICPSR no. 7513) by the *Forschungsgruppe Wahlen*, Mannheim. Neither the principal investigators nor the institutions involved in the data preparation bear any responsibility for the analyses or interpretations presented in this article.

### Models of Spatial Analysis: A Brief Comparison

Most attempts at spatial analysis of party systems rely on the "similarities" model (Coombs, 1964, pp. 6, 25–28). What is given in such an analysis is judgments of similarity between stimuli such as parties or candidates, and what emerges from the non-metric analysis of those similarities is distances between those stimuli (Shepard, 1962; Kruskal, 1964). These distances can then be displayed by way of an  $n$ -dimensional plot, depending on the number of axes the analyst decides to select. The judgments of similarity providing the input to the analysis, it should be noted, are averages formed across respondents doing the judging. The individual element is averaged out of the picture, literally speaking. Such a step, to be sure, filters out a great deal of "noise" like personal idiosyncrasy or even frivolity, which can be safely considered to be random in nature. But that step also suppresses meaningful differences in the judgments of stimuli, obscures asymmetrical relations between them and produces a solution that need not fit the judgments of a large number of individuals. That individuals rely on a particular dimension for their judgments cannot be established with these kinds of solutions.

The typical spatial analysis, moreover, uses as its point of departure some form of affective rating of stimuli; e.g., how much do you like or dislike a particular party? The judgment of similarity is then inferred by comparing the rating of one party with that of another one, the overall similarity between any two parties being expressed by correlation coefficients or averaged absolute differences between the two ratings. What is submitted for analysis, therefore, consists not of similarities in perception, but of correlations of affect. The resulting configuration reveals the structuring of affect: the larger the distance between any two stimuli, the lower the affect correlation.

Affective ratings, it would seem, are far more akin to preference orders, into which they can easily be translated, than to judgments of similarity. It is one thing to perceive two stimuli as similar or dissimilar and another to prefer or rate one higher than the other. One of the most elegant and intuitively appealing models for the analysis of this type of "data" is furnished by Coombs' "unfolding" model (1964, pp. 80–121). The essence of this model is best expressed in Coombs' own words (1964, p. 80):

Each individual and each stimulus [i.e., party] may be represented by a point on a common

dimension called a *J Scale*, and each individual's preference ordering of the stimuli from most to least preferred corresponds to the rank order of the absolute distances of the stimulus points from the ideal point, the nearest being the most preferred. The individual's preference ordering is called an *I Scale* and may be thought of as the *J Scale* folded at the ideal point. . . (italics in original).

This formulation, of course, applies only to the one-dimensional case, but the notion expressed here can also be extended to the multidimensional case.

The unfolding model, as developed by Coombs, is deterministic. There is no provision for error or goodness of fit. A particular preference order (*I scale*) either fits a given ordering of the stimuli (*J scale*) or it does not. For the success of application it does not matter how many individuals select the various *I scales*. At the same time, the fact that just two individuals choose the stimuli in exactly the reverse order is sufficient to postulate the existence of an ordering of stimuli in that fashion. While very much hinges on the choices of very few individuals, little weight is attached to the choices of many. It is difficult, if not impossible, without side information, to screen out the significant from the idiosyncratic choices. The recovery of the metric configuration of both individuals and stimuli in the same space is an arduous task.

These drawbacks notwithstanding, this article takes advantage of the unfolding model for casting light on the *ordering* of stimuli which underlies individual preferential choice. The model is well suited for determining the fit of a given ordering of parties such as the left-right framework. It permits us to separate preference orders which are compatible with a given party ordering from those which are not.

#### Party Preference Orders: The Three Major Parties

The first part of the analysis is devoted to preferences involving the major parties, that is, the parties represented in the West German federal parliament. Since the same three parties, and no others, have held the seats of the Bundestag from 1961 onwards, it would seem justifiable to examine preferences for them in their own right. To provide a sketch of their identities, let us take note of the social cleavages which spawned these parties (Pappi, 1973, 1976; Kaack, 1971). The oldest among them, the Social Democratic party (SPD), is closely identified with the industrial working class and espouses, in terms of practical policy, a labor

union ideology. The chief partisan opponent of the SPD, the Christian Democratic/Social Union (CDU/CSU), is the heir to a party generated by the conflict over the role of the Catholic Church in society. The CDU/CSU continues to articulate policy positions agreeable with the values, if not always the stated positions, of the Catholic Church. It draws its most loyal electoral support from the Catholic part of the public. Perhaps surprisingly, neither the SPD nor the CDU/CSU, as Pappi has pointed out (1976, p. 6), confronts the other one as its chief policy adversary in the domain in which it has staked out its distinctive claim. Instead, it is the Free Democratic party (F.D.P.), heir to the various streams of German liberalism, which performs that role. The F.D.P. and its predecessors have traditionally advocated limited government interference in economic and social matters as well as a reduction of church influence. Its electoral clientele is concentrated among the non-Catholic middle class.

The sharp edges of these policy confrontations have undoubtedly been blunted ever since the current parties or their predecessors began mobilizing electoral support. To many voters, these distinctions have become all but invisible. Nonetheless, the rapport struck between parties and certain groups of voters at an earlier time constitutes a bond of continuing strength and vitality (Pappi, 1976). First preferences for parties, and thus voting choices, are firmly anchored in social affiliations which bear witness to economic and religious cleavages. But what about the *order* of party preferences? How profoundly are second and third preferences shaped by the party locations along the traditional cleavages?

Table 1 presents respondents' preference orders involving the CDU/CSU, SPD, and F.D.P. for the 1961-1976 period. Since the preference rankings were not obtained the same way each year, a brief review of the measures may be in order. In 1961 and 1965 respondents were asked to rate each of the three parties separately on an 11-point scale ranging from -5 (least favorable) to +5 (most favorable). The preference orders were then constructed by comparing the three ratings; ties, consequently, were unavoidable. The 1969 survey affords the analyst the rare opportunity to derive party preference rankings from paired comparisons. Respondents were presented with each pair of parties and asked which of the two they preferred. A respondent had to make ten such comparisons since there were five parties, and from this set of comparisons a respondent's preference order was obtained. Surprisingly few



Table 1. Party Preference Orders of the German Electorate, 1961–1976, Pre-Election Surveys (Percent)

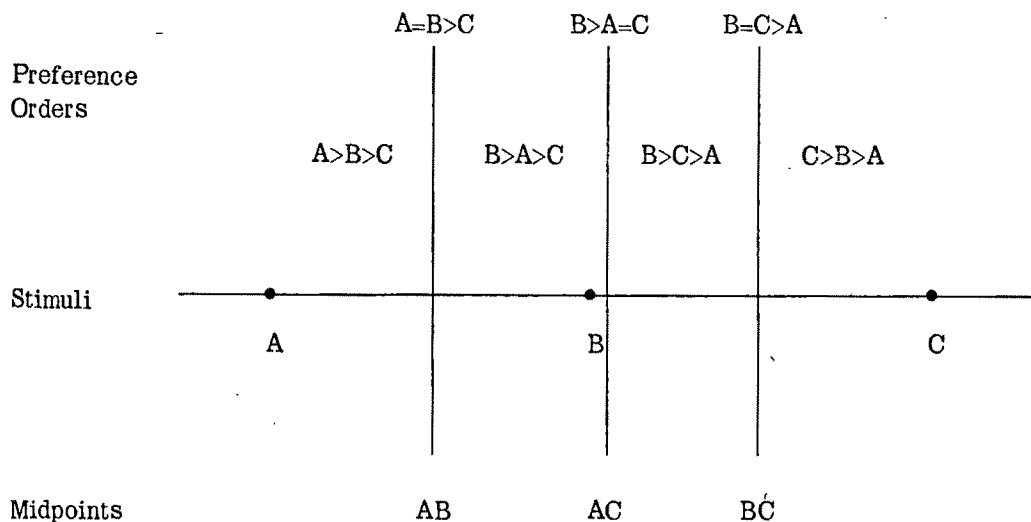
Preference Order	July 1961	September 1965	September 1969	September 1972	June 1976
S>C>F	16	20	32	18½	8
S>F>C	8	8	14	33	37
S>C=F	7	8	—	—	—
S=C>F	8	7	—	—	—
C>S>F	19	17	33	28	19
C>F>S	21	18	15	14	28
C>S=F	12	13	—	—	—
C=F>S	4	4	—	—	—
F>S>C	1	1	3	4	6
F>C>S	3	3	3	2½	2
F>C=S	1	1	—	—	—
F=S>C	1	1	—	—	—
Total N=	101 1463	101 1275	100 818	100 1595	100 1872

Source: Zentralarchiv, Cologne, West Germany, study nos. 056, 556, 426/427, 653/636/637, and 823/824/825; also ICPSR, Ann Arbor, Michigan, study nos. 7100, 7103, 7098, 7102, 7513.

individuals were caught in intransitive rankings—not more than three percent of those making all ten paired comparisons. In 1972 and 1976 respondents were asked to indicate their preference orders for the five parties in a single step, with ties between two or more parties not permitted. At this stage of the analysis, however, only preferences for the major three parties are considered.

Three parties—let us denote them as *A*, *B*, and *C*—can be placed in three different ways: *A-B-C*, *B-A-C*, and *A-C-B* as well as their

mirror images; but *C-B-A*, for example, would be considered identical with *A-B-C*. Each of those orderings forms a unidimensional configuration of parties that might underlie the preferential choice of individuals. This notion is illustrated in Figure 1, using the ordering *A-B-C*. The three parties are located on a single axis at the points indicated. Midway between any two points right through the “midpoints” run vertical lines which delineate zones of preference orders. Provided that individuals rank their party preferences by folding



Source: Compiled by the author.

Figure 1. Preference Orders Compatible with One-Dimensional Configuration of Three Stimuli

the party continuum at the point of their own location ("ideal point"), four distinct preference orders are possible, given the party ordering shown in Figure 1:  $A > B > C$  for individuals located to the left of midpoint  $AB$ ,  $B > A > C$  for those located between the midpoints  $AB$  and  $AC$ ,  $B > C > A$  for those located between the midpoints  $AC$  and  $BC$ , and  $C > B > A$  for those located to the right of midpoint  $BC$ . As can be quickly verified, the preference orders  $C > A > B$  and  $A > C > B$  are not permitted by the party ordering in Figure 1. The location of  $B$  in the middle blocks these two orders. Once ties in rankings are allowed, three further preference orders qualify for the above party ordering:  $A = B > C$  for individuals located exactly at the midpoint  $AB$ ,  $B > A = C$  for those at midpoint  $AC$ , and  $B = A > C$  for those at midpoint  $BC$ .

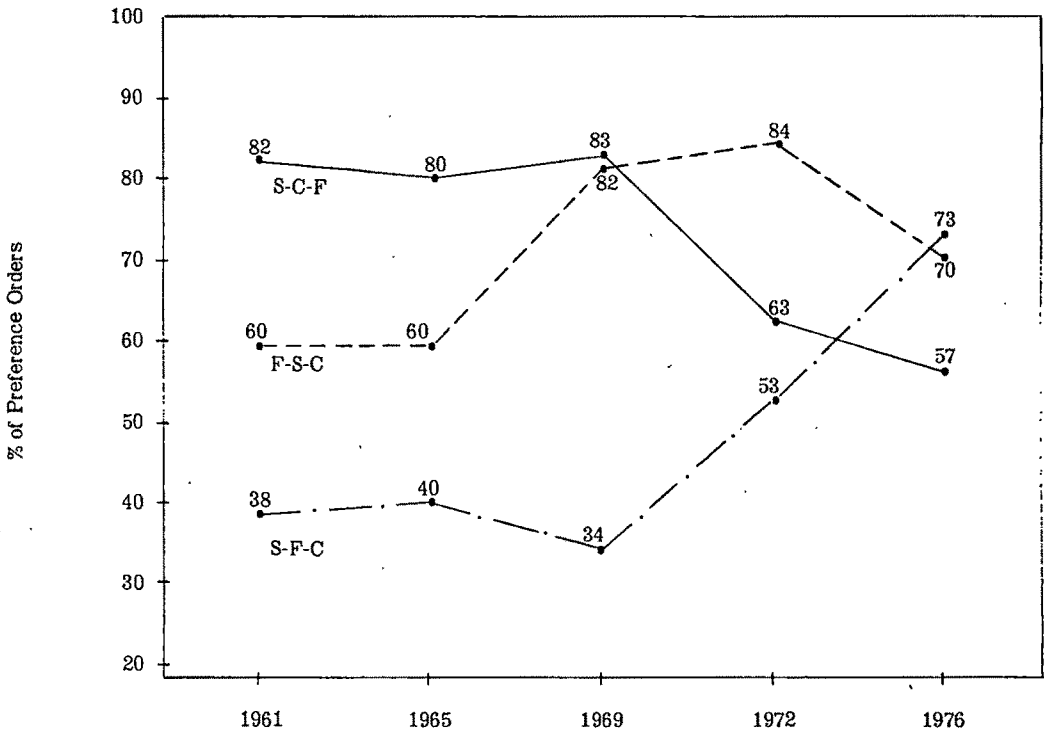
#### Dimensions of Party Preference Orders

The share of individual preference orders compatible with a given ordering of the three

main German parties for the 1961–1976 period is charted in Figure 2. It should be stressed that none of the preference orders listed in Table 1 belongs exclusively to one ordering of the parties. In fact, each preference order is compatible with two of the three orderings. This fact, unfortunately, narrows sharply the leeway for choosing between the competing orderings on the basis of the preference frequencies.

Above all, it is worth noting that no single ordering of parties dominates throughout the whole period. The early 1960s witnessed the dominance of the S–C–F ordering, whereas in the late 1960s the F–S–C ordering rose to prominence. On two occasions, that is in 1969 and 1976, two orderings tied for the lead. What is the message conveyed by this change, and what light does it cast on the meaning of the various party orderings?

Viewed in the light of the above discussion of the German party system, the S–C–F ordering may be construed as the legacy of the economic cleavage, pitting the "socialist" SPD against the "free enterprise" F.D.P. Given the



Source: Zentralarchiv, Cologne, West Germany, study nos. 056, 556, 426/427, 653/636/637, and 823/824/825; also ICPSR, Ann Arbor, Michigan, study nos. 7100, 7103, 7098, 7102, 7513.

Figure 2. Percent of Individual Party Preference Orders Compatible with Different Party Orderings, 1961–1976

prominence of status polarization—some would say, class struggle—in any industrial society, one should not be surprised to find its trace in political orientations and preferences of the mass public. But if so, Figure 2 suggests a marked decline of this dimension in the years since 1969. To be sure, the SPD's decision to abandon socialist ideology and rhetoric helped close the gap between this party and the F.D.P. Yet, this step was taken in the late 1950s whereas, according to Figure 2, the S-C-F curve declined most sharply between 1969 and 1972, that is, a full decade after the SPD had come forward with its major policy reorientation. The magnitude as well as the timing of this decline cast some doubts on the claim that the S-C-F ordering bears out the economic cleavage to the extent suggested by its share of preference orders.

The F-S-C ordering recalls the religious cleavage pitting the "secular" F.D.P. against the "clerical" CDU/CSU, to exaggerate a bit. As Figure 2 indicates, the F-S-C curve rises sharply in 1969 from a moderate level and clearly dominates the other two in 1972. This would suggest intense and exacerbated religious tensions during the late 1960s and early 1970s. Several new issues indeed arose at that time which were fought in the shadow of the religious cleavage, such as the easing of divorce and abortion laws as well as the phasing out of denominational schools. But the overwhelming share claimed by this dimension nonetheless strains credulity. Religious issues have rarely if ever topped the priority list of the public in recent years.

The third, and final, possible ordering of the three German parties produces a pattern that is quickly recognized as the left-right continuum. Klingemann (1972) and Inglehart and Klingemann (1976) have demonstrated that German voters in large numbers place SPD, F.D.P. and CDU/CSU in that order on a left-right continuum. There can be no doubt that the public perceives significant distances between these three parties on such a continuum, at least since 1969; for lack of data, no such claim can be made with the same certainty for the time before that date.

Turning from perception to preference and, again, to Figure 2, one can quickly note that the left-right ordering in all but one of the years studied accounts for the lowest share of individual preference orders. It is also quite noticeable that its share varies the most over those years. While it is not implausible to see the left-right ordering reach its peak in 1976—what with the CDU/CSU's campaign theme of "freedom versus socialism"—it is certainly puzzling

to see this ordering decline during the late 1960s. If indeed an ideological orientation possesses the average voter and governs his or her reaction to politics, it manifests itself in a highly erratic fashion. That alone can be safely taken as evidence that left-right ideology does not greatly shape the average voter's party preferences in Germany. Negative findings of this sort were also reported by Butler and Stokes (1969, pp. 200–05) for British voters, whose party system closely resembles the German system.

At this point, the reader may be puzzled by the lack of congruence between the left-right ordering of parties (S-F-C) and their supposed ordering on the economic dimension (S-C-F), which is typically construed in left-right terms. One way of resolving this puzzle, if it is one, would be to argue that these two dimensions are the same, but that the F.D.P. changed its location in the party system, moving from the right of the CDU/CSU to the middle position between SPD and CDU/CSU. Between 1966 and 1969, the F.D.P. indeed underwent a sweeping transformation including a leadership shake-up and the adoption of a platform advocating wide-ranging political reform. Both of these moves were widely regarded as a shift of the F.D.P. (until then simply the FDP) to the left of the CDU/CSU, if not the SPD (Kaltenleiter, 1970; Zülch, 1973). Granted that such a shift occurred, the public nonetheless had yet to take note of it by 1969. Judging from respondents' preferential choice, the S-F-C ordering, which features the new location of the F.D.P. in the middle, fails to increase its share in 1969. That share, in fact, is lower than ever, whereas the alternative S-C-F claims a share in that year that is barely diminished compared to 1965. Voters' preferential choices, then, registered little, if any, movement of the F.D.P. to the left (of the CDU/CSU) before the 1969 election.

#### Coalition Alignments and Party Preference Orders

The efforts to decode the meaning of the three competing party orderings in terms of social cleavages or left-right ideology have not proven very successful. It is highly unlikely that these dimensions shifted so frequently and so strongly during the 16 years from 1961 and 1976 as to account for the changing shares of the three party orderings. It is even less likely that throughout that period one and the same dimension was underlying most preference orders, but that the ordering of parties on that dimension changed. What, then, accounts for

the changing shares of preference orders? While party positions and the salience of the various cleavages may have changed somewhat over time, far more visible change has occurred in the coalition alignments among the parties—from a CDU/CSU–F.D.P. coalition until 1966, through a CDU/CSU–SPD coalition between 1966 and 1969, to a SPD–F.D.P. coalition since 1969. What impact did these realignments exert on the voters' party preference orders?

The best opportunity for such a test is provided by the 1969 panel survey. In 1969, immediately following the federal election, the SPD entered into a coalition with the F.D.P., thereby dissolving the coalition it had maintained until then with the CDU/CSU. While the pre-election interviews took place at a time when the CDU/CSU–SPD coalition was still in office,<sup>3</sup> the post-election interviews were held

right after the new coalition had been initialled. Table 2—a documents the massive reshuffling of second and third preferences among SPD partisans during the short six weeks which elapsed between the two survey waves. Whereas before the election 65 percent of the Social Democrats favored the CDU/CSU secondmost, only 45 percent did so afterwards with the majority now favoring the F.D.P. Almost half of the 65 percent forsook the CDU/CSU for the F.D.P., and exactly half of the post-election S>F>C champions adopted their preference order in the wake of the party leadership switch of coalition partners.

The net shift attributable to this switch in the aftermath of the 1969 election amounts to 20 percent. By 1972 it had increased to 30 percent and by 1976 had reached the 50 percent mark. While the later shifts cannot be fully attributed to the coalition's existence and persistence, the near-instantaneous shift occurring in 1969 is surely an impressive tribute to the impact of elite coalition decisions on the preference orders of the electorate. A shift of similar magnitude can also be found among CDU/CSU partisans during the time between

<sup>3</sup>It must be noted, however, that the coalition signals issued by party leaders throughout 1969 were somewhat confusing. It was an open secret that CDU/CSU as well as SPD, even though they were governing together, were each prepared to enter into a small coalition with the F.D.P. after the election. The "coalition" between SPD and F.D.P. during the election of Heinemann (SPD) as federal president in early 1969 offered an unmistakable clue to the prospect of a government coalition between these two parties. Yet, despite such clues, the supporters of

CDU/CSU and SPD in the electorate maintained a preference for this coalition and close affection for each other's party.

Table 2. Change in Party Preference Orders among SPD Partisans in 1969 and CDU/CSU Partisans in 1965–1969

a. Social Democrats, 1969				
Pre-Election 1969	Post-Election 1969			
	S>C>F	S>C>F	Total	
S>C>F	37	28	65	
S>F>C	8	27	35	
Total	45	55	100%	(n=223)
b. Christian Democrats, 1965–1969				
1965	1969			
	C>F>S	C>S>F	Total	
C>F>S	— <sup>a</sup>	—	52	
C>S>F	—	—	48	
Total	31	69	100%	

Source: Zentralarchiv, Cologne, West Germany, study nos. 556 and 426/427; also ICPSR, Ann Arbor, Michigan, study nos. 7103 and 7098.

<sup>a</sup>Internal frequencies not available for lack of panel data.

1965 and 1969 when their party switched coalition partners (see Table 2-b).

If coalition realignments change party preference orders, these alignments themselves must be considered as among the forces, along with the social cleavages, which *shape* party preference orders. The finding from the 1969 survey might warrant the estimate that *at least* half of those individuals whose preferences orders are compatible with a given coalition alignment are formed in response to that alignment. Given the short time interval in 1969 and the evidence of more change subsequently, this estimate may be viewed as conservative. Using the 50 percent mark, I have recalculated the shares of the three party orderings in 1969, subtracting from each of them (see Figure 2) half of the frequency of those preference orders which were compatible with the prevailing coalition alignment.

This adjustment for the coalition effect in 1969 cuts the shares of the two top orderings S-C-F and F-S-C from about 80 down to 50 percent, while leaving the one-third share of the left-right ordering S-F-C undisturbed. Even now, because of the large overlap between their shares, the orderings S-C-F and F-S-C make inflated claims. While it is impossible with the available data to estimate the exact shares of the two dimensions suggested by these two orderings, namely class and religion, each of them has a close, and perhaps superior, rival in the cues emanating from the coalition decisions of party elites.

Coalition realignments go a long way toward accounting for the fluctuations of the preference shares observed in Figure 2. The sharp decline, for example, of the S-C-F ordering after 1969 coincides with the establishment of a party coalition (SPD and F.D.P.) which violates that ordering. Likewise, the ordering which surges during the same time, that is, S-F-C, features the new partners side by side; so does the alternative F-S-C, whose share remains stable. As a rule, the dominant party ordering at any given time invariably witnesses the two coalition partners in adjacent positions.

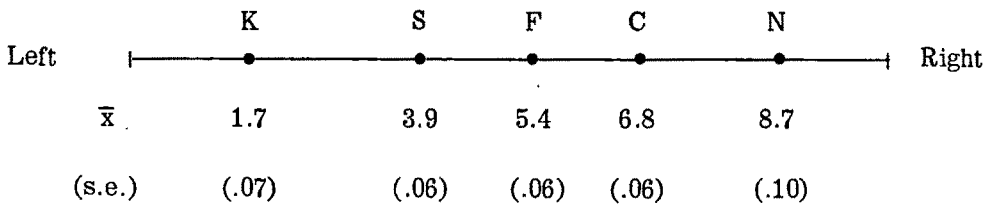
It cannot be overlooked, however, that on several occasions the least popular party ordering also features the coalition partners side by side. That holds true for the ordering S-F-C in 1961 and 1965 when it was compatible with the CDU/CSU-FDP coalition and in 1972 with respect to the SPD-F.D.P. coalition. The low share of this ordering may be viewed as further testimony to the small significance, until recently, of the left-right continuum, with which it is consistent. At the same time, coalition politics offers an important clue which may help resolve the paradox noted above. That clue

refers to the coalition strategy of the opposition party. Although opposition parties are often ambiguous about their choice of a most-preferred coalition partner, it is safe to say that the SPD during the early 1960s aimed at a coalition with the CDU/CSU, which ultimately was concluded in 1966. While the leading ordering (S-C-F) happens to accommodate both the coalition between CDU/CSU and F.D.P. and the affinity of the SPD to the CDU/CSU, the S-F-C ordering does not. Likewise the surge to prominence of the S-F-C ordering between 1972 and 1976 coincides with a move, however cautious and grudging, on the part of CDU leaders toward the F.D.P. as their preferred coalition partner in the future. That move would well account for the massive shift among CDU/CSU voters from the SPD to the F.D.P., as far as second preferences are concerned. On the whole, the dominant party ordering which underlies preferential choice in West Germany turns out to be the one which features the coalition partners side by side and the opposition party alongside that party which is targeted by the leaders of the opposition party as future coalition partner.

#### Party Preference Orders: The Five-Party Case (1969-1972)

The three parties treated in this analysis so far have dominated electoral and legislative politics in the Federal Republic. But below the five-percent level of electoral support, a few minor parties continue to exist and occasionally threaten the tranquil alignments in the Bundestag. The near-miss of the National Democratic Party (NPD) coupled with the bare success of the F.D.P. in 1969 brought the parties close to the brink of a major governmental crisis. While less potent at the polls, the German Communist party (DKP) remains visible largely as the result of its vocal support on university campuses and because of the issue of public service employment of left-wing radicals. These two parties have added color, contrast and unrest, however unwelcome, to the mainstream politics offered by the three major parties.

By far the most familiar continuum on which each of the five parties can be assigned a singular location is the left-right framework. Voters in Germany, as is also the case in Italy and France (Barnes and Pierce, 1971), are capable of viewing their parties in left-right terms. They also agree to a great extent on the placement of the parties from left to right. Figure 3 reproduces Klingemann's (1972) findings concerning the five parties' mean locations



Source: Hans Klingemann (1972), "Testing the Left-Right Continuum on a Sample of German Voters," *Comparative Political Studies* 5:96.

Figure 3. Left-Right Locations of German Parties as Perceived by Mass Public, 1970

in the eye of the mass public.

Provided that a voter arrives at a preference order by folding the left-right continuum at the point of his or her own location on it and by picking the parties in the order from least to most distant, 11 distinct preference orders involving 5 parties are possible (Coombs, 1964, p. 87). That assumes that voters are unanimous in their positioning of each party. If we relax this unrealistic assumption and allow for some disagreement so long as the left-to-right *ordering* of the parties is not disturbed, 5 more preference orders are permissible for a total of 16.<sup>4</sup> How large a share of all individual preference orders, weighted by the number of respondents picking them, do these 16 orders claim?

Table 3 lists the respondents' preference orders involving all five parties for 1969 and 1972, for which such data were available. As can be verified with the information in Table 3, 34 percent of respondents in 1969 and 53 percent in 1972 preferred the parties in one of those 16 orders permitted by the left-right ordering. The two figures are virtually identical with those found for the left-right framework in the three-party analysis. It appears, then, that the addition of two parties located at one or the other end of the left-right continuum has neither diminished nor enhanced the share of that framework. But let us take a closer look at the preference orders in the five-party case.

It is quite apparent from a quick scan of the listing in Table 3 that preference orders are distributed in a highly skewed fashion. The top

ten orders, in terms of frequency of choice, account for 88 percent of individual choices in 1969 and 1972. These ten preference orders invariably feature the three main parties in the first three ranks with the NPD and the DKP (ADF in 1969) alternating between the last two ranks.<sup>5</sup> One could suppose, therefore, that the fit of the left-right continuum, limited as it is already, is rather fortuitous.

An ordering of parties featuring the three main parties in the left-right fashion but reversing the locations of NPD and DKP happens to claim 32 percent of individual preferences in 1969 and 49 percent in 1972. In other words, the rather absurd ordering, N-S-F-C-K, as Table 4 bears out, does just about as well as the much-heralded left-right ordering, K-S-F-C-N. And that holds for both 1969 and 1972. These findings severely challenge the significance of the left-right framework as a guide to preferential choice in the German mass public.

Most respondents appear to treat NPD and DKP as if they were interchangeable, albeit undesirable, parties. The 1969 panel survey consisting of a pre- and post-election wave reveals that one of every three respondents moved between NPD and DKP, as far as ranks four and five, that is last and next to last, were concerned, not to mention the 20 percent of respondents who would not assign separate ranks to them. The fact that about as many respondents shift from NPD to DKP as do the other way around suggests that change was largely of a random nature, like the flip of coin. As a result, as many as two-thirds of the respondents might be inclined to treat these

<sup>4</sup>The 16 preference orders, with the most-preferred party listed first each time, are: KSFCN, SKFCN, SFCKN, SFCKN, SFCNK, FSKCN, FSCKN, FSCNK, FCSKN, FCSNK, FCNSK, CFSKN, CFSNK, CFNSK, CNFSK, NCFSK. Note that in this section of the article tied ranks are not considered. Respondents in the 1969 and 1972 surveys were not given the option to tie two or more parties for a particular rank.

<sup>5</sup>From now on, the symbol "DKP" or simply "K" will be used to refer to the Action Democratic Progress (ADF) in 1969 as well as to the DKP in 1972. The ADF was an electoral alliance between the DKP and the German Peace Union (DFU).

Table 3. Party Preference Orders of West German Electorate, 1969 and 1972

Party Preference Order	Frequency		Party Preference Order (continued)	Frequency	
	1969	1972		1969	1972
S F C N K	29	315	C S K N F	—	2
S F C K N	85	210	C N K S' F	—	1
S F N C K	1	13	C F N K S	1	—
S F K C N	3	45	C F K S N	2	—
S F N K C	—	25	F S C N K	6	35
S F K N C	1	9	F S C K N	16	33
S C F N K	122	201	F S K C N	1	3
S C F K N	141	94	F S K N C	—	2
S K F C N	3	13	F C S N K	19	31
S N F K C	—	1	F C S K N	5	10
S K F N C	—	1	F N S C K	—	2
S C N F K	6	7	F C N S K	—	1
S N C F K	2	1	F C K S N	2	—
S C K N F	—	1	F K N S C	1	—
S N C K F	—	1	F S N C K	1	—
S N K C F	—	1	N K C F S	—	1
S C K F N	6	—	N F S C K	—	1
S K N F C	1	—	N S F C K	—	2
S K C F N	3	—	N C F S K	6	1
S K C N F	1	—	N S C F K	—	1
C F S K N	56	40	N C S F K	1	1
C F S N K	66	178	N S F K C	1	—
C F N S K	10	17	N F S K C	1	—
C F K N S	2	2	N K S C F	1	—
C S F K N	135	97	N K F S C	4	—
C S F N K	138	351	K N F C S	—	1
C N F S K	3	9	K S F N C	—	1
C N F K S	—	2	K S F C N	2	4
C S K F N	2	2	K C S F N	1	—
C S N F K	11	6	K S C F N	1	—
C K S F N	1	1	K F S C N	1	—
C N S F K	5	7	K N F S C	1	—
C N K F S	—	2			
			Total	907	1785

Source: Zentralarchiv, Cologne, West Germany, study nos. 426/427 and 635/636/637; also ICPSR, Ann Arbor, Michigan, study nos. 7098 and 7102.

Key: C = Christian Democratic Union/Christian Social Union  
 S = Social Democratic party  
 F = Free Democratic party  
 N = National Democratic party  
 K = Communist party (1972), Action Democratic Progress (1969)

two parties as interchangeable.

Neither in 1969 nor in 1972 does the left-right ordering account for most individual preferences involving all five parties; nor does it even come close to such a standing. Table 5 lists the four party orderings which account for the highest number of preferences. To be sure, two of them closely approximate the left-right ordering. The K-S-C-F-N ordering simply has C and F reversed, that is, the CDU/CSU and the F.D.P. Likewise, the K-F-S-C-N ordering has S and F reversed. These two party scales might be construed as slightly impure versions of the left-right continuum. One could indeed

make a case for the F.D.P. being located to the right of the CDU/CSU up until about 1969 when the party moved leftwards. It is conceivable that left-right considerations govern voters' preferential choices except for the fact that not all voters link parties with those terms in exactly the same fashion.

Even without probing people's understanding of the left-right concept, we can soundly reject that hypothesis. It is just too apparent from Table 5 that the slightly impure left-right orderings (K-S-C-F-N and K-F-S-C-N) happen to account for no larger a share of preferences than do two alternative orderings

Table 4. Left-Right Party Ordering and Alternative: Share of Individual Preference Orders

Party Ordering	Percent of Individuals Whose Preference Orders Are Compatible with Party Ordering	
	1969	1972
K-S-F-C-N	34	53
N-S-F-C-K <sup>a</sup>	32	49
N =	907	1785

Source: Zentralarchiv, Cologne, West Germany, study nos. 426/427 and 635/636/637; also ICPSR, Ann Arbor, Michigan, study nos. 7098 and 7102.

<sup>a</sup>On this party ordering, the positions of K and N as featured on the left-right ordering are reversed.

where the locations of NPD and DKP are reversed. It simply does not make a difference whether it is the NPD or the DKP which is placed at the right (or the left) end of the party continuum. These two parties seem to surround the three-party field like two planets circling around the sun. They lack fixed locations in a configuration featuring all five parties. The left-right ordering is no more than a transient constellation of the five parties which may be occasionally observed.

These results suggest that preferential choice involving all five parties is mostly guided by the consideration<sup>1</sup> of the *political* status of the parties, with the established and legislatively represented parties on one hand and the parties outside the Bundestag and despised by the parties inside, on the other. What matters most to the average voter in making a preferential choice, it seems, is not so much the policy position(s) or ideology of the minor parties but the fact that they are rejected without qualification as possible allies by each of the parties which the voter might most prefer. SPD leaders find the ideologically more related DKP no more acceptable as an ally than the ideologically more distant NPD. And the same holds true

for the CDU/CSU leaders as well as the F.D.P. leaders. It is clues of this sort, rather than perceptions of policy locations, which shape preferential choice among the German public.<sup>6</sup>

### Conclusions

This article has been mainly concerned with the question of preferential choice. Which considerations govern the order in which German voters prefer their parties? In particular,

<sup>6</sup>An alternative view based on "rational choice" premises has been suggested to me by G. Robert Boynton. This view maintains that since both NPD and DKP are so unlikely to receive enough votes, that is, more than five percent, to win seats in the Bundestag, votes for these parties would be "wasted" and, therefore, are not cast. While this may account for the low support for these parties at the polls, it is not clear why this should also prevent voters from expressing their true preference for these parties. In any event, the NPD during the years 1966-69 mustered sufficient strength in state elections to enter state legislatures and came close enough in the 1969 federal election, with 4.3 percent of the vote, to make a vote for it worthwhile.

Table 5. Other Party Orderings Featuring the Minor Parties at Endpoints: Share of Individual Preference Orders

Party Ordering	Percent of Individuals Whose Preference Orders Are Compatible with Party Ordering	
	1969	1972
K-S-C-F-N	78	57
N-S-C-F-K	78	57
—	—	—
K-F-S-C-N	77	75
N-F-S-C-K	75	76
N =	907	1785

Source: Zentralarchiv, Cologne, West Germany, study nos. 426/427 and 635/636/637; also ICPSR, Ann Arbor, Michigan, study nos. 7098 and 7102.



does an issue-defined configuration of parties underlie preferential choice? Most spatial analyses of voter evaluations (and preferences) of parties assume a common political space whose dimensions are typically construed in terms of policy issues or ideology.

The analysis above has tried to isolate the dimensions relevant to party conflict in Germany and to estimate their shares of preference orders. The results of the analysis dealing with mass preferences for the three parties represented in the Bundestag reveal traces of the two classical cleavages, class and religion, which constitute the main long-term determinants of electoral choice. What is noteworthy, however, is the fact that throughout the 16 years covered (1961–1976) the apparent contributions of these dimensions vary greatly. These fluctuations, in fact, closely parallel changes in the coalition alignments of the parties. The ordering of party preferences is highly susceptible to the coalition cues provided by party elites.

This conclusion is not challenged by an analysis of preferences involving the two minor parties NPD and DKP along with the three major parties. Any major party ordering flanked by the minor parties does about equally well accounting for preferential choice, irrespective of the side on which the minor parties are placed. Since this holds true for each of the three party orderings involving the major parties, including the left-right ordering, it is highly unlikely that a spatial configuration defined in terms of position issues or ideology underlies the preferential choice of German voters. Rather, it appears that a sharp dichotomy separates the major parties from the minor ones and that this dichotomy is defined by the political status of the parties.

The preferential choice of voters, to a large extent, is based on cues taken from party leaders. Through actions or doctrine, these leaders define the relationship between their own party and the other parties. Some of the other parties are summarily ruled out of contention as possible allies by being declared "unconstitutional" or "extreme." And from among the others, one or more are selected as the most preferred ally or allies. The policy dimensions which may shape those elite decisions are not at issue here. They do not greatly affect the preferential choice of average German voters. What matters more to voters are those decisions themselves and the cues they provide.

How relevant are these conclusions to preferential choice in other multiparty polities? The German case may seem of limited value since

only three parties are represented in the elected assembly. However, several studies dealing with more extended multiparty systems suggest mass susceptibility to elite cues regarding preferential choice there as well. In their study of party distances in Norway, for example, Converse and Valen report (1971, p. 146) that between 1965 and 1969 three parties converged on a single point on the perceptual map of the public as reconstructed by multidimensional scaling. This shift is attributed to the formation of a government coalition involving those three parties. Likewise, Damgaard and Rusk (1976), in their analysis of perceived party distances in Denmark, point to informational cues such as the parties' coalition behavior, rather than to "sophisticated belief systems," in accounting for the close fit between mass evaluations of parties and the left-right framework (Damgaard and Rusk, 1976, p. 196).

The fact that in West Germany and elsewhere voters' preferential choices are considerably affected by party elite cues raises the question of popular representation in multiparty systems. In most of those systems, the government consists of a coalition of several parties. Popular influence may be facilitated by parties forming that kind of coalition most preferred by the electorate. But what if popular coalition preferences—like party preference orders—are guided by the preferences and actions of the very party elites forming the coalitions? How much influence can the electorate exercise under these circumstances? These are challenging questions worthy of closer scrutiny.

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# Political Periods and Political Participation\*

PAUL ALLEN BECK  
Florida State University

M. KENT JENNINGS  
University of Michigan

*Analysis of complementary data sets, a 1965–1973 panel study of young adults and their parents and the 1956–1976 Michigan presidential election series, shows that the late 1960s and early 1970s were a deviant period where participation in American politics was concerned. During this time, the young were more active politically than their elders, substantially increasing their participation from previous years, and Americans on the ideological left participated more than those at other positions along the ideological continuum. While this surge of left-wing activism was not restricted to the young, it probably accounts for the relative participation advantage enjoyed by the young. These findings challenge the “conventional wisdom” about patterns of participation in America. They are best explained by recognizing that the opportunities for political action among the American citizenry are not fixed, but instead vary with changes in the political stimuli across different periods.*

The late 1960s and early 1970s were years of exceptional turmoil in American politics. Prominent amid this turmoil—in the anti-Vietnam War protests, the civil rights movement, and the political campaigns of McCarthy, McGovern, and others—were the young, accused by many of being the instigators of the turmoil, if not its underlying cause. The allegedly exceptional activity of the young suggests that the “normal” relationship between age and rate of participation may not have prevailed during these years. The apparent leftward bias of youthful participation seems an exception to the “normal” conservative bias in participation. Careful examination of the participation of this young generation is, therefore, more than a heuristic exercise. It promises to illuminate and possibly explain a potentially deviant case of participation. It can as well provide a hard test of current theories of participation.

For this inquiry we draw initially upon data from the second wave of a two-wave panel

study of young Americans and their parents.<sup>1</sup> The first wave was a representative cross-section sample of 1,669 high school seniors and their parents, personally interviewed in the spring of 1965. The second wave, conducted in early 1973, consisted of personal interviews with 1,119 of the youths (now 25 and 26 years of age) and 1,118 of their parents as well as responses to mail questionnaires from 229 youths and 61 parents.<sup>2</sup>

These data are particularly well suited for examining the participation of young adults during the late 1960s and early 1970s. To begin with, the data provide information on the political participation of emergent adults, precisely the group located in the center of the turmoil in the years prior to 1973. Another advantage of these materials is that they embrace a wide range of political activities, both electoral and non-electoral. They enable us to chart the participation of young adults from before their entry into the adult electorate to their mid-twenties. Finally, the parent data

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<sup>1</sup>The study was conducted by the Center for Political Studies at the University of Michigan. For earlier reports using the same data base, see Jennings and Niemi (1975, 1978).

<sup>2</sup>The total number of personal and mail re-interviews yields an unusually high re-interview rate for each generation, given the eight-year interval between waves: 81 percent for the younger generation and 75 percent for the parents. Detailed comparisons, based on 1965 characteristics, between those re-interviewed and those not re-interviewed indicated very little bias in the 1973 subset.

provide a convenient benchmark against which to gauge the political activity of the young adults. The participation of the parents was charted for the same period and may be compared directly with that of the young. While parents are not the only useful benchmark for comparison and are at a different stage of the life cycle, comparisons across these biologically linked generations have the advantages of holding constant many important factors contributing to participation and of dealing directly with differences between two generations widely regarded as lying at the core of the generation gap during this time.<sup>3</sup>

The two-generation data are the most comprehensive data on participation for the period of interest because they tap a wide range of political activities. Yet they cannot give us a picture of participation at other age levels in these years or for all ages in preceding and subsequent years—two other useful benchmarks for comparison. To provide this perspective, we shall turn subsequently to information on participation from the presidential election series of the University of Michigan's Survey Research Center/Center for Political Studies. These materials enable us to assess the representativeness of the youth and parent samples for their respective age groups, to determine the age-participation relationships across all age groups in the late 1960s and early 1970s, and to extend our analyses to other time periods. They are limited to campaign-related activities, however, thus restricting the range of our comparisons.

Taken together, the two data sources will allow us to say a good deal about political participation, particularly among the young, during a turbulent period in American politics. Comparing the participation of young adults with that of their parents and of other age cohorts at the same time, and with that of the same age cohort in other times, will triangulate upon what was normal and what exceptional about these young. Identifying deviance in this

generation, though, is not the same as explaining it. While our data are less amenable to the task of explanation, we shall nonetheless elaborate the proposition that strong period forces affected participation during the late 1960s and early 1970s.<sup>4</sup>

#### Youth-Parent Differences in Rates of Participation

Our initial comparison between the youths and their parents focuses on their rates of participation—that is, how politically active they were between 1965 and 1973. All previous studies have found the same general relationship between age and participation. Participation is lowest for young adults, peaks in the middle-aged years, and then falls off for the old (Milbrath and Goel, 1977, pp. 114–15). Verba and Nie (1972, p. 141) have shown, further, that lower rates among the old may be attributed to their lower socioeconomic status and that, once status differences are removed, participation rises gradually with age through age 65, and then levels off afterwards. These results give us every reason to expect substantially greater political participation in the parental generation than in the youth generation.

The political activity of the parents and youths in our sample was gauged by answers to nine questions, covering a variety of participative acts. Five questions dealt with involvement in election campaigns since 1965, including referenda and contests for public office at any level. They elicited information regarding the following: persuading others to vote a certain way, attending meetings or rallies, displaying buttons or bumper stickers, donating money, and doing other kinds of campaign work. The four remaining questions covered non-electoral activities performed at any previous time: writing a letter to the editor, contacting a public official, engaging in protests or demonstrations, and working with others to solve some community problem.<sup>5</sup>

<sup>3</sup>Our use of the term *generation* refers primarily to the lineage concept rather than the age or birth cohort concept. Actually, the youth sample is, aside from dropouts, representative of the 1947–1948 birth cohort eligible for the first grade in 1953. The parent sample is, strictly speaking, only representative of parents of high school seniors, although comparisons between the parents and similarly-aged respondents in cross-section samples reveal strong political similarities. For a useful explication of the differences between the lineage and age cohort models see Cutler (1976, 1977).

<sup>4</sup>An extensive discussion of period effects, as well as other sources of discontinuity between age cohorts, may be found in Riley, Johnson, and Foner (1972), especially Ch. 2, and in shorter form in Riley (1973). See Jennings and Niemi (1975) for an examination of other types of period effects in the parent-youth pair data set.

<sup>5</sup>For all specific forms of electoral and non-electoral activities, precise information was elicited concerning the timing and nature of the activity. Although the time span specified for the non-electoral acts was not restricted to the 1965–1973 period, in

Separate factor analyses of the activity intercorrelations for the youth and parent samples justify using the same unidimensional index of participation for each generation, in which participation is measured simply by the number of activities performed. The principal components solutions from these analyses are presented in Table 1. The solutions are strikingly similar. For the youth only one factor emerges. This factor explains 26 percent of the *total* variance in the activities, and all activities are substantially correlated ( $r \geq .35$ ) with it. For the parents too the principal components analysis yields a single factor. This factor accounts for 29 percent of the *total* variance in parent activities, and all activities but protest are substantially correlated ( $r \geq .43$ ) with it. Even though protest does not load highly on this dimension for the parents, it is associated with the other activities. Thus, the underlying structure of participation is virtually the same for the two generations. Lowering the threshold for inclusion of additional factors and oblique rotations of the resulting reference axes distinguish slightly different patterns of participation for the two generations, but we shall use the results at the more general level of partici-

pation where a single overall dimension is uncovered.<sup>6</sup>

When the means of the participation index for youths and parents are compared, the young are found to have been more active by a considerable margin. They performed an average of 2.1 activities, compared with 1.7 for their parents. That the youth enjoy this edge is contrary to the findings from previous studies of participation. Part of their edge results from the markedly higher level of protest activity among the young. Six times as many youths as parents were involved in protests or demonstrations from 1965 to 1973. One in six members of this early adult cohort engaged in such direct action, impressive evidence indeed for the wide

<sup>6</sup>The results of our principal components analyses are remarkably similar to those reported by Verba and Nie (1972, p. 68) in their 1967 study of participation, in spite of the differences in activities used, populations sampled, and time frames adopted for performance of the activities. Their first factor explains 27 percent of the total variance and correlates substantially with all activities (including voting activities which were omitted from our analysis) except the two reflecting personalized contact. Verba and Nie used their results to justify construction of a summary index of participation from all the items, just as we have done. Unlike us, though, they have moved beyond this first general factor by rotating the reference axes to establish four basic subtypes of participation—voting, campaigning, communal, and particularized contacting—for use in subsequent analysis along with the overall measure.

practice virtually all of the youth participation fell into that time span as did over three-fourths of the parental activities cited. Given the age of the filial respondents, the bulk of their electoral activities clustered in the 1972 races.

Table 1. Factor Analysis of Participation Variables\*

Variable	Youth Generation		Parent Generation	
	Factor One	Percent Explained of Item Variance	Factor One	Percent Explained of Item Variance
INFLUENCE	.51	26	.64	41
RALLIES	.63	40	.68	46
OTHER	.58	34	.54	29
BUTTONS	.52	27	.50	25
DONATIONS	.55	30	.59	35
CONTACTS	.53	28	.59	35
LETTERS	.44	19	.43	18
COMMUNITY	.35	12	.53	28
PROTEST	.44	19	.24	6
Percent Explained of Total Variance		26		29

Source: 1973 Parent-Child Socialization Study, Center for Political Studies, University of Michigan

\*These are the principal components solutions in which the number of factors allowed to emerge was restricted to those which explained at least the equivalent of the variance in a single item (1/9 or 11.1 percent of the total variance).

dispersion of non-conventional behavior among the young during the period. Not surprisingly, the frequency of these actions varied directly with education—rising from 5 percent among those not attending college, to 15 percent among those with some college, and to 29 percent among those with a college degree.

But the participation advantage of the young has a broader base than confrontation politics. The young also enjoyed a substantial advantage on two other activities—attempts to influence the votes of others (47 to 31 percent) and displaying campaign stickers and buttons (30 to 21 percent). These behaviors are directed towards influencing elections, central institutions of conventional democratic politics. That the younger generation has an edge here demonstrates that it was also quite adept at playing the normal political game.

Generational differences in participation rates were minor across the other specific activities. The young outperformed their parents slightly on attendance at rallies (22 to 19 percent), other campaign activities (13 to 11 percent), contacting officials (31 to 30 percent), and writing letters to the editor (6 to 5 percent). Parents enjoyed an edge on only two, donating money for use in campaigns (21 to 17 percent) and engaging in cooperative community service activities (33 to 28 percent). Parental advantages here should hardly be surprising, even in the face of parental lags elsewhere. The parental generation is at the peak of its earning power and in a much better financial situation to contribute to campaigns. Similarly, the parents are more likely to have entered the mainstream of community life and, as a result, to have had greater opportunities to engage in community-betterment activities. Finally, since performance of the noncampaign activities is measured over the respondent's lifetime, the parents have had far more opportunity for participation than their offspring. Given all of these life-stage facilitators, it is surprising that the parental edge is so meager for these activities.

Before interpreting our findings further, however, we need to consider two plausible reasons for the surprisingly greater participation by the youth. One is that previous studies of participation, because they typically included voting with the other forms of activity, may have overstated (or even reversed) age cohort differences. The overall index of participation used by Verba and Nie (1972, p. 141) in their analysis of age effects, for example, contains four voting items and thus builds in a sizable advantage for the older cohorts who vote at much higher rates than recent entrants to the

electorate (Hout and Knoke, 1975). Our analysis of the data from the Verba and Nie study, though, demonstrates that a pronounced older adult advantage persists even when voting activities are removed from the participation index. On each of the nine specific activities which parallel those used in our analysis (influence, rallies, donations, membership in a political group, other campaign work, two community activities, and two kinds of contacts), respondents in the age cohorts encompassing the parents (age 45 to 65) outperformed those in the young adult cohort (under age 26) in the years up to 1967. The youthful edge found in our data for the 1965–1973 period deviates markedly from this pattern.

A second plausible explanation for the relatively greater participation by the young adults in our sample is that our findings are an artifact of the restricted populations we have sampled. The high school seniors of 1965 and their parents were not representative of their respective age cohorts. The high school senior population comprised about three-quarters of all people at that age and the top three-quarters where education was concerned. Likewise, the parents were better educated than all parents whose children were of high school senior age in 1965. But this explanation too is discounted when subjected to examination. We compared the *election campaign* activity of young adults (21–29 years of age) with that of parent-aged adults (44–64 years of age, the age group which included three-quarters of the parents), using the Center for Political Studies national election surveys in 1968 and 1972, the two presidential election years in the 1965–1973 period. In both years the young participated more than the parental-age cohort by a noticeable margin. The average activity edge for the youth over the five indicators used (influence, rallies, donations, buttons, and other activity) was .58 to .49 in 1968 and .68 to .54 in 1972. Even though the election studies data did not include the full range of political activities present in our youth-parent data, the results of this analysis add strong support to our findings.

There should by now be little doubt that the surprising advantage in political participation enjoyed by the young adults in the late 1960s and early 1970s over their parents was real. The generation that came of age in the late 1960s was indeed exceptional in its political activity. The political events and turmoil of the period most likely drew these young adults into political activism at rates much higher than normal for young people.

### Youth-Parent Differences in Bias of Participation

Typically, only a minority of citizens perform any one of the political activities considered here. That more people are politically inactive than active underscores the extensive slack in use of political resources in America (Dahl, 1961, pp. 271-301). It also enhances greatly the potential for disproportionate participation by certain groups that has the effect of biasing the nature of political representation (Verba and Nie, 1972; Nie and Verba, 1975). The direction of bias may be the same over time, as the differential usage of resources remains constant across various groups. Such is undoubtedly the case in the United States where socioeconomic status is concerned. Alternatively, the direction of bias may vary over time, as groups change their levels of activity. The long history of white activity advantage in the South, now reduced significantly by upsurges in black participation, illustrates the temporal variability of bias.

The most important biases are those that emerge when groups richer in political resources hold policy preferences different from groups with fewer resources. In their 1967 survey of political participation in the United States, Verba and Nie (1972, Ch. 15) found that political activists were noticeably more conservative than the general public. Turning the causal ordering of the relationship around, they also discovered indirect evidence of a tendency for conservatives to be more active (pp. 224-28). In short, the Verba and Nie study shows conservatism and activism to be positively correlated at the beginning of the period under consideration in our study. By implication, this is the usual pattern.

The events of the late 1960s and early 1970s provide some reason for challenging the constancy of this relationship, at least with respect to young adults. Political activity of the young during this period appeared to be focused heavily on left-wing causes and candidates. So pronounced did the tendency towards left-oriented activism among the young seem to be that careful students of public opinion warned us not to assume that the liberal activists were necessarily representative of their more quiescent peers (Converse and Schuman, 1970). Middle-aged cohort political energies, by contrast, did not appear to be directed disproportionately towards left-wing causes and candidates. Thus, the prevailing portrait of the period was one in which more liberal youths confronted their more conservative parents.

Of course, there is one way in which the

apparent liberal tendencies of the young activists during this period could be made to square with findings from previous research. Our data show that the younger generation was considerably more liberal than the parental generation (see the marginals for Figure 1 below). It is conceivable that left-oriented participation among the young was so conspicuous because the young were unusually liberal as an age cohort, even while the relatively more conservative youth among them held the "normal" activity edge over their peers. This would not be the first time that a genuine compositional change was mistaken for a change in the functional relationship between two variables.

We have tested for the ideological bias in the political participation of each generation by comparing the participation levels of those who took different positions on major domestic policy issues of the period. Bias refers to the differences in participation rates across various ideological positions, treating political activity as if it is dependent upon ideology. If those at each position participate to the same extent, there is no participation bias to ideology, and the participants are genuinely representative of the entire sample from an ideological perspective. As the levels of participation diverge by ideological position, on the other hand, bias increases. Measured in this way, the bias of participation can be compared across groups with different ideological compositions. (See the appendix for the difference between this measure and the one used by Verba and Nie.)

Assessment of participation bias requires measurement of two concepts—policy orientation and participation. Our measure of participation is the index of political participation described earlier. To measure ideology, we have constructed an index of liberalism-conservatism based upon the positions of respondents in 1973 on a series of important domestic issues: government guarantees of jobs, protection of rights of the accused, government aid to minorities, legalization of marijuana, school busing, equal rights for women, and changes in the form of government.<sup>7</sup> Positions on each of these issues were initially recorded on a seven-point scale, which we classified as liberal (1-3),

<sup>7</sup>Measuring issue positions at the end of the period of participation assumes, of course, that these positions were held by respondents as they were participating. As tenuous as this assumption might be, it is far preferable to measuring issue positions at the beginning of the period of participation in 1965—the only other time point for which we have data.



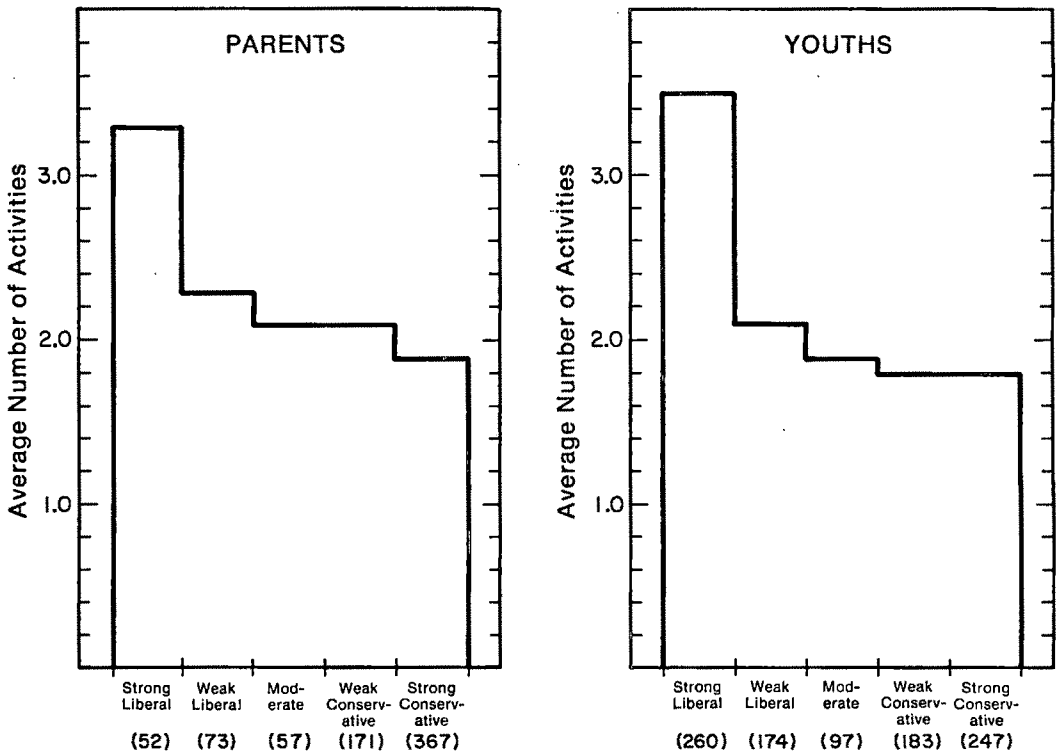
moderate (4), and conservative (5-7) for purposes of our analysis. An index was formed across the issues by simply subtracting the number of conservative positions from the number of liberal positions. For this presentation, the resulting index score has been collapsed into five categories: strong liberal (+7 to +3), weak liberal (+2 to +1), moderate (0), weak conservative (-1 to -2), and strong conservative (-3 to -7).<sup>8</sup> We could not include foreign policy issues in this index because too few of them were covered in the survey instrument and none was asked in terms of the seven-point scale. Using actual positions on issues is, of course, only one of several legitimate ways to measure ideology (Converse,

1964). This measure is sensitive to constraint among issue positions, but it is indifferent to whether the sources of that constraint lie in some overarching belief system.

The rates of parent and youth participation within each category of the ideology index are presented in Figure 1.<sup>9</sup> The most striking characteristics of these data are the distinct *liberal biases* within *both* groups. If there were no participation bias to ideology, the bars in Figure 1 would rise to uniform heights across the horizon. Instead, there is a spectacular, then

<sup>8</sup>Treating the issues in this manner assumes that they fall along a single continuum, which may be labeled liberal/conservative. This assumption is validated by the principal components solution to a factor analysis of the issue correlations, in which all items had substantial positive loadings on the first factor.

<sup>9</sup>Respondents who failed to take a position on one or more of the issues were eliminated from this analysis. Missing data automatically force respondents towards the middle categories of the index. These same respondents tend to have lower rates of participation. The combination of the two factors artificially sharpens the liberal participation advantage in both generations. Even if the missing data are retained, though, the substantive conclusion is the same—the two generations are alike in their liberal bias.



Source: 1973 Parent-Child Socialization Study, Center for Political Studies, University of Michigan.

Figure 1. Participation Bias among Parents and Youths



halting decline in political activity as the figure is read from left to right for each group. Activity levels among the strong liberals are almost twice as great as those among strong conservatives. This enormous advantage assumes added significance for the young adults in view of the large size and near-numerical equality of the two most polar groups. Both the disproportionate size of the youthful liberal contingent and its heightened participation have created the impression (which now seems very realistic) of a leftward bias in the political activity of youth during the late 1960s and early 1970s. Although parents exhibit an ideology-participation relationship which is virtually a carbon copy of that for their children, their ideological distribution is radically different. The much higher participation rates of parent liberals are counterbalanced by the enormously larger number of parent conservatives. In this contrast to the filial generation, purely a compositional one, may lie the prevailing view of a generation "gap" in the bias of participation.

These data suggest another, perhaps more surprising, deviation from expected patterns of participation. In the late 1960s and early 1970s, not only did the young participate more than their parents, but both groups also exhibited a liberal participation bias. Later we shall propose that this behavior was in response to the opening up of political alternatives on the left during the period under examination, especially in the 1972 general election. Whatever its cause, though, the remarkable finding here is that both parents and youth deviated from the expected conservative bias in patterns of participation.<sup>10</sup>

#### Participation and Bias in Broader Perspective

Analysis of these youths and their parents uncovered generational differences in rates of participation but similarities in liberal participation biases. That the findings in both cases were

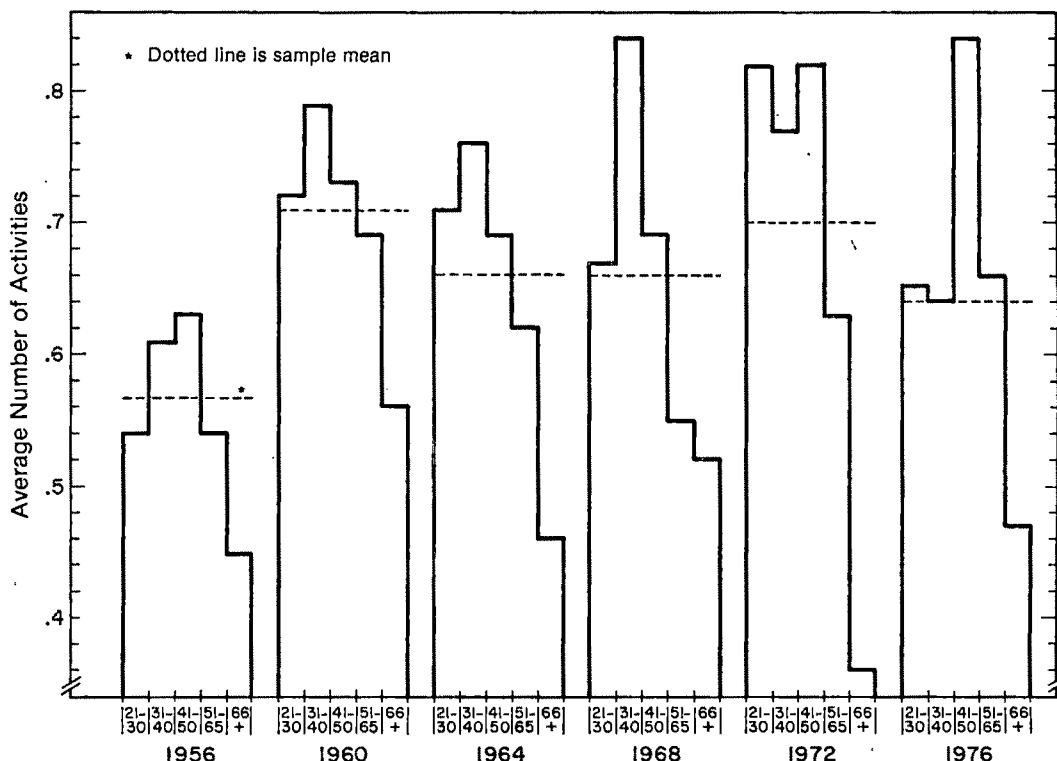
the opposite of those reported in research on earlier periods fueled our expectations that pervasive period effects were at work in the late 1960s and early 1970s. But for all of their considerable advantages, the youth-parent data have limited ability to answer the broader questions we have raised about the unique effects of the period. Because they reflect only two slices of the age-arrayed population distribution, they cannot provide a complete picture of the relationship between age and participation during this period. More critically, because they are time-bound, they cannot confirm our speculations about how the relationships we found differ from those of other periods. We must move beyond this initial data set for a broader perspective.

Evidence from the presidential election series of the Survey Research Center/Center for Political Studies provides this larger canvas. We first used these election series data to determine the relationship between age and participation in each presidential election year in the 1956–1976 period. We gauged participation by an additive index of campaign-oriented activities, using the same campaign activities contained in the overall index of participation in the two-generation analysis: attempting to influence voters, attending meetings and rallies, donating money to campaigns, displaying campaign buttons or stickers, and engaging in other campaign-related work.<sup>11</sup> Respondents in each year were separated into five age cohorts: 21–30, 31–40, 41–50, 51–65, and over 65. The means for the campaign activity index are displayed in Figure 2, by election year and age cohort within the year.

These data show temporal variations in both the participation rates of the young adults and, more generally, in the relationship between age and participation. Of singular interest, young adult campaign activity peaked in 1972, rising well above the rates for the same age group in previous years as well as the succeeding year, 1976. So active were the young in 1972 that their average activity rate fell just short of the highest levels attained by *any* age group throughout the entire series. This finding lends considerable support to the prevailing image of

<sup>10</sup>These empirical findings stand up well when alternative measures of ideology are employed. We examined the relationship between ideology and participation for each issue separately and for self-placement on an explicit liberal-conservative continuum. For the youths, the liberals participated more in every case, usually by a very wide margin. For the parents, the liberals were more active in the cases of ideological self-placement and five of the seven issues. In the parental exceptions to the general rule, moderates showed the highest activity levels and liberals continued to outperform conservatives.

<sup>11</sup>Summarizing campaign activity by use of a single cumulative index is methodologically justifiable in each year because each item is correlated substantially and positively with the initial factor in the principal components analysis. Data from 1952 were not used because the question on displaying campaign buttons or stickers was not asked.



Source: Presidential Election Series, Center for Political Studies, University of Michigan.

\*Dotted line is sample mean.

Figure 2. Campaign Activity by Age, 1956-1976

exceptional youthful participation around the turn of the decade, especially so for 1972. It is a fair surmise that the youth activity rate in 1972 would appear even more enlarged, and the 1968 rate correspondingly larger too, if protest activities were included. Using the benchmark of similar age cohorts in earlier and later years, these results support our earlier ones in depicting an exceptionally active group of young adults in the early seventies.

Just as striking is the fate of the general relationship between age and participation in 1972 compared with other years. In each election year prior to 1972, a parabolic relationship between age and participation appeared. In three of the years the 31-40 age group occupied the apex of the parabola; in the remaining year, the 41-50 age group held this position. No matter which of these was the peak group, the distribution assumed the neat unimodal form reported by Verba and Nie (1972, p. 139) and widely expected to be the constant pattern for the relationship. The 1976

figures depart from this pattern so slightly as to be inconsequential, as activity in the 31-40 group dropped considerably. But the 1972 results stand in dramatic contrast to the other years, disconfirming any notion of an invariant relationship between age and participation. On the crest of their surge in activity for that year, the youngest cohort outstripped every other age cohort in campaign activism. The campaign activity of the 21-30 group was so high, in fact, that it pulled the overall activity average for that year to within reach of the 1960 activity high for the series, in spite of abnormally low rates of involvement by those over 50. Even in comparison with their elders, then, the young were exceptionally active in 1972.

Before leaving Figure 2 behind, we should note one other interesting feature of young adult participation—this time in years other than 1972. While one or both middle-age cohorts (31-40 and 41-50) outparticipated the young in all but 1972, the young were more active than both of the cohorts over 50 in every

year but 1976. Perhaps including community activities and the various forms of personalized contacting would reverse this decline in involvement after age 50. We cannot tell from our materials. What our data do suggest, however, is that one should exercise considerable caution in depicting the twenties as a low participation stage of the life cycle. Even in so-called normal times, Americans in this age group participate more than those over 50. And, in an abnormal time, as the 1972 election period appears to be, the young may come to outparticipate those in every other age group.

Having employed the longitudinal materials to expand upon our initial findings about participation rates, we now employ them to explore the equally provocative findings about the disproportionately greater participation of liberals in the late 1960s and early 1970s. Because the two generations in the youth-parent study exhibited similar bias, explanations need to focus on factors common to both generations instead of factors which differentiate them. Our hypothesis is that the principal source of this pervasive bias among liberals lies in the nature of the stimuli for political action during the period. While partially a function of the general disposition to participate, political activity is also surely responsive to the opportunities for participation. During the late 1960s and early 1970s, these opportunities were especially widespread for those on the left.

To a great extent the events leading up to and surrounding the presidential elections come to generate and symbolize the divisive forces and strong movements at work in the polity and to characterize the opportunity structure for action. Consequently, it would be wise to look for changes according to ideology precisely in the fluctuating nature of election campaigns. The nature of the candidates in the general election campaigns, where the bulk of the activity is concentrated, should be especially important in determining participation opportunities. Just as the novel and fervent stimulus was on the right in 1964 in the form of the Goldwater movement, it seems to have been on the left in 1968—with the McCarthy and Kennedy candidacies—and even more so in 1972, via the McGovern candidacy and nomination. Of course 1968 also witnessed a strong challenge from the right in the form of the Wallace candidacy, which was channelled outside as well as inside the Democratic party. The Wallace candidacy may have helped to mute the liberal advantage in participation in 1968.

The linkage of the bias of participation to the nature of ideologically based opportunities

might help reconcile the liberal bias we have found for the 1965–1973 period with the conservative bias found by Verba and Nie (1972) for the immediately preceding period. The youth and parent respondents were asked to specify their involvement in campaign activities during the 1965–1973 period. This span encompassed two long presidential campaigns, 1968 and 1972, which were characterized by unusually attractive ideological alternatives on the left. Furthermore, the 1972 general election campaign was unprecedented in modern times in that a major party nominee was from the left. Two of the four campaign activities measured by Verba and Nie, on the other hand, were (given the time frame of the questions used) restricted to the 1964 election period, when the opportunities for ideologically inspired involvement lay more on the right. If our supposition about the mobilizing effect of the election period is correct, moreover, then other activities of a non-campaign nature may also have been more attractive to the right at this time.

Analysis of the election series can show whether the relationship between ideology and participation varied over the 1956–1976 period, a necessary condition for the existence of the kind of period effects posited here. As in the case of the age cohort comparisons across time, the indicator of political participation is the campaign activity index generated for each study year. We used opinions on domestic issues of the day as the basis for constructing an ideology index in much the same fashion as for the parental and young adult samples. The index score for ideology was the number of liberal positions minus the number of conservative positions on a series of domestic issues, all of which were substantially correlated with the principal component of a factor analysis. These scores were divided into five categories for presentation: strong liberal (+3 and above), weak liberal (+2 to +1), moderate (0), weak conservative (–1 to –2), and strong conservative (–3 and below). In the 1956–1968 analysis, six issues were included. They dealt with the role of the federal government in the following areas: school integration, fair employment practices, education, medical care, guaranteed jobs, and housing integration (measured with fair employment practices in 1956). Support for government action was coded as liberal, opposition as conservative. The ideology index was comprised of eight issues in 1972. School integration, fair employment practices, and guaranteed jobs were repeated from before. The new issues were legalization of marijuana, women's rights, rights of the accused, govern-

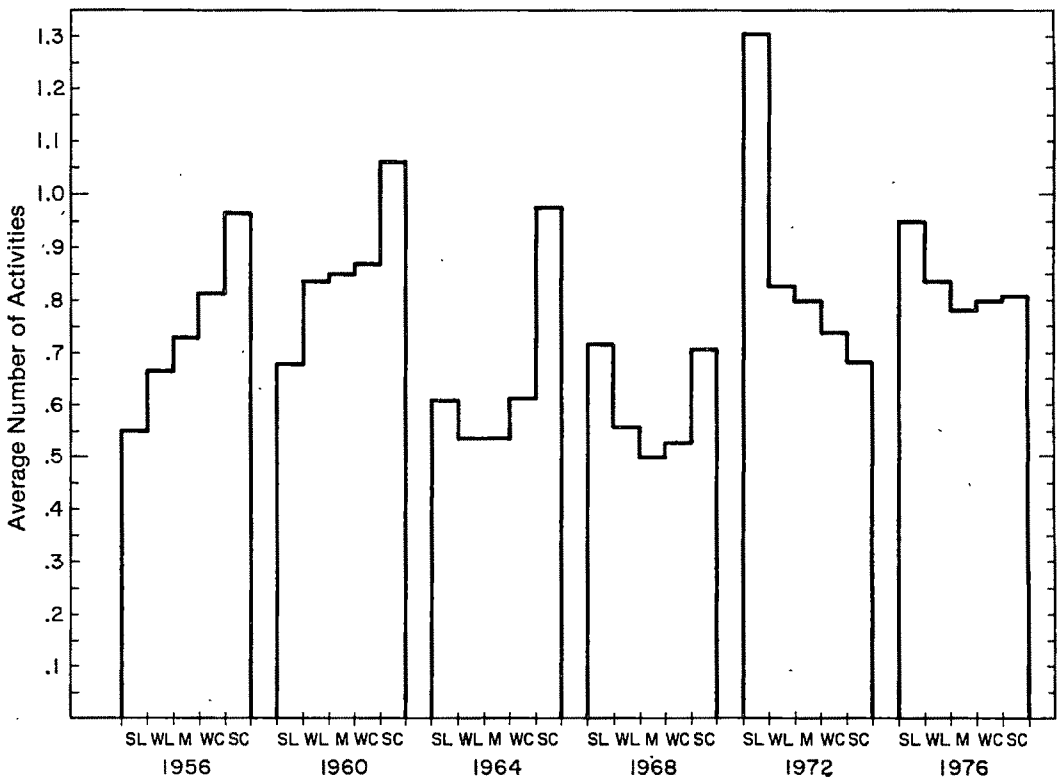
ment aid for minorities, and busing. Support for each of these policies (1–3) was coded as liberal, opposition (5–7) as conservative, while the middle position (4) was coded as moderate. In 1976, changes in taxes (coded like the new issues in 1972) was substituted for fair employment practices from the 1972 list.<sup>12</sup> Despite the narrower definition of political participation employed, an examination of participation rate by ideological groupings through time

should go a long way toward either supporting or impairing the explanation we have proposed for the political activity bias found in the youth and parent generations.

Figure 3 presents the mean activity levels for the five ideological categories across the election series. From left to right in each year are strong liberals (SL), weak liberals (WL), moderates (M), weak conservatives (WC), and strong conservatives (SC). The results provide dramatic support for our proposition that differential bias arises in part from the nature of the participation opportunities of the period.<sup>13</sup> In the elections of 1956, 1960, and 1964 the conservatives participated far more actively

<sup>12</sup>All other responses to the questions in each of the surveys were coded as missing data, and respondents with any missing data were eliminated from analysis. This elimination lowered the number of cases substantially in several years but did not affect our substantive conclusions. In fact, the effects of eliminating missing data here parallel closely those reported in footnote 9 for the parent and youth data: adding respondents with missing data dampens the political activity rates of the moderates without having much impact on the extremes.

<sup>13</sup>The aggregation of particular issues into an index had little effect on our findings. The pattern displayed using the index was typically replicated (albeit with less clarity) in the patterns of pair-wise association between political activity and the issue positions taken individually.



Source: Presidential Election Series, Center for Political Studies, University of Michigan.

Figure 3. Campaign Activity by Political Ideology, 1956–1976

than the liberals. Although only 1956 reveals an almost perfect stepladder pattern, the marked tendency was for activity rates to rise with increasing conservatism. This pattern fits a view of American politics in which a vociferous conservative minority are seen as drowning out the more numerous liberals and middle-of-the-roads. Indeed, failure to hold this view led the Goldwater forces in 1964 to overestimate drastically the breadth of their electoral support (Converse, Clausen, and Miller, 1965). Significantly, the 1964 results reported here echo the bias reported by Verba and Nie (1972), whose measures of electoral activities in particular were drawn from the 1964 era.

By 1968 the regularity had been broken. Strong liberals and strong conservatives took part at approximately equal rates, as did weak liberals and weak conservatives at lower levels; moderates were least active. Although this U-shaped distribution fits the classic prescription of a direct relationship between ideological fervor and manifest behavior, it occurs but once across the six elections. If the 1968 results cast serious doubt on the model of invariant relationships between ideology and activity, those for 1972 effectively lay the model to rest. Here the pattern is a mirror image, in slightly exaggerated form, of the 1956 stepladder. Beginning with a tremendously high level of liberal activity, every decrease in liberalism brings a decrease in activity.<sup>14</sup> As of 1976 the strong liberals still held an edge over strong conservatives.

These findings make it clear that ideological bias in campaign activity is variable. Conservative in the 1956–1964 period, the bias shifted to liberal from 1968 to 1976. Which of these periods is the more “normal” cannot be determined with any certainty and is the subject of spirited debate among students of voting behavior (Nie, Verba, and Petrocik, 1976, pp. 110–55; Converse, 1976, pp. 98–107). However this debate is resolved, we have established that the relationship between ideology and participation varies over time, presumably dependent in part upon the nature and attractiveness of the ideological alternatives presented to the mass public. Within this framework, the

bias displayed by the parental and filial generations in our earlier analysis acquires an almost inevitable shape. Indeed, the comparison of the activity means for the ideological groupings shown in Figure 1 bears remarkable similarity to that for the national cross-section sample of 1972 in Figure 3, even though our two-generation samples are biased upwards in educational attainment. Both parents and youth in our initial study were reacting to the broad sweep of political forces in the same manner as was the adult population as a whole. Further, as the data in the appendix show, these findings are unchanged when the Verba-Nie technique for expressing bias is used.

This changing relationship between ideology and participation over time may well contain the key to the unexpectedly high relative rates of participation by the young in the early 1970s. For the first time in the electoral series, the ideological position of the young diverged markedly from that of older adults, as there were almost twice as many young among the strong liberals. Indeed, the 1972 ratio of youthful to older strong liberals *itself* was virtually twice what it had been in any earlier year. This disproportionate youthful propensity for strong liberalism persisted into 1976, although at a level slightly below that recorded in 1972. While liberals outparticipated conservatives again, the gap had narrowed considerably and youthful activity had declined. Perhaps the absence of a candidate on the left led to both changes. As we saw with the two-generation data, uneven rates of mobilization from different sectors of the ideological continuum produced greater rates of participation by the young in the early seventies, but only because there were proportionally more of them on the left. It appears that the youthful edge in participation at this time can be traced to the fact that opportunities for left-oriented participation came at a time of disproportionate youth liberalism—a much more complex compositional effect than we entertained earlier.<sup>15</sup>

<sup>14</sup>A parallel change in the relationship between participation and ideology in 1972 is noted by Nie, Verba, and Petrocik (1976, pp. 207–09), although they do not analyze or interpret it as fully as we have. Among both Republicans and Democrats, they found that ideologues participated more in that year. In previous years, by contrast, ideologues participated noticeably more only among Republicans.

<sup>15</sup>Direct comparisons of the ideological distributions across the different years should be avoided. Even where the same issues are used to define ideology, changes in the meaning of the issue and in the format of the question undermine strict equivalence (Sullivan, Piereson, and Marcus, 1978; and Bishop, Tuchfarber, and Oldendick, 1978). We have not made much of the changing distributions along the ideology continuum for this reason, restricting our attention instead to group differences within an election year and how these *differences* varied over the years.

### Conclusion

Analysis of two complementary data sets, the youth-parent cohorts and the 1956–1976 presidential election series, demonstrates that young adults in the late 1960s and early 1970s were indeed exceptional in their political participation. The young participated at higher rates than did the young in other years, and they were more active than mature adults during this period. Both patterns were unusual and unexpected. But the exceptionality of the young was confined to their rates of participation. Where ideological bias is involved, young liberals were joined by older liberals in being more active than their age cohorts who occupied more conservative positions along the ideological continuum—again an unexpected and perhaps exceptional pattern.

Our findings suggest that substantial revisions are in order for two aspects of participation theory. In the first place, there is no constant relationship between age and political participation. In certain periods, the neat pattern of increasing participation rates until about age 65 is disrupted. The late 1960s and early 1970s were one such period, as the youngest members of the electorate came to outparticipate their elders. Nor can one account for these findings by introducing standard compositional variables. Although the educational attainment of same-age cohorts was rising over time, participation was fluctuating. More to the point, radical discontinuities in the shape of the relationships, such as in the 1968–1972–1976 series of elections, could not possibly be accounted for by demographics alone.

In the second place, there is no immutable tendency for conservatives to be more politically active than either moderates or liberals. Again, in certain periods, the expected conservative edge is overcome. For example, in the early 1970s, liberals became the most vigorous activists among both the young and other age groups.

These findings show that political participation does not exist in a political vacuum. It is commonly, some might say always, a mass response to elite activities (Miller and Levitin, 1976). This can be demonstrated most easily for campaign-related activities, but it probably extends to other activities as well. One's involvement in a political campaign may be seen as a function of, *inter alia*, the attractiveness of the serious candidates in that campaign. It is surely no accident that the largest participation advantage of strong conservatives came in the Goldwater era. By the same token, a Democratic primary, such as that in 1968 or 1972,

with many candidates on the left, offers more inducements for leftists to participate than does a campaign in which there are no candidates from the left. Even greater opportunities to participate arise in the context of the general election campaign. Thus, it should come as no surprise to find that left-wing participation peaked in 1972, just as right-wing participation was relatively highest in 1964. Similarly, a general election campaign in which two centrists are the combatants (such as 1976) probably curtails the incentives for left- and right-oriented involvement and decreases the ideological biases in participation.

The period beginning around the mid-sixties and extending into the early seventies saw a decisive change in the battleground of American politics. Challenges to the prevailing liberal consensus were launched primarily from the left, with the result that the consensus and perhaps even liberalism itself were shattered. The first challenges came over civil rights and the war in Vietnam, but the focus quickly spread to matters of urban decay and non-black minority rights. Serious candidates championing these causes emerged especially in contests for the Democratic party nomination in both 1968 and 1972, and success in these preliminary rounds enabled one candidate to carry his party's banner in the presidential election. Opportunities for protest participation were likewise expanded on the left. One has to go back at least to the 1930s in American political history to find comparable stimulation for left-oriented participation.

The findings presented here show convincingly that these opportunities were seized by people whose operational ideologies placed them on the left. In this period leftists attained levels of participation far in excess of those of their peers elsewhere along the ideological continuum—in contrast to what was thought to be normal, right-biased patterns of participation. By charging the left pole of the opportunity magnet, in other words, participation on the left was made relatively more common. But left-oriented Americans were not distributed evenly among all age groups. Instead, ideology was heavily skewed by age, with the familiar result that the young were much more left-oriented than older adults, including their parents. It seems quite likely that the unusual youthful edge in participation during the late 1960s and early 1970s was the result of these greater openings on the left. While the opportunities may have been seized at roughly equal rates by young and old, the distinctively youthful cast to the composition of left-wing Americans pulled proportionally more of the young

into participation. Thus, the potential for a disruption in the "normal" tendency for lower participation among the young exists whenever the ideological postures of the young diverge from those of the remainder of the population.

The surprising patterns of participation for the late 1960s and early 1970s presented in this study seem far removed from current, quieter times. As of 1976, the young adult advantage had vanished, and the liberal bias had narrowed considerably. That these advantages emerged during one period of American politics, however, illustrates how the considerable slack in the use of participation resources can be used to benefit different groups at different times. Propositions positing static relationships between either age or ideology and participation are misleadingly incomplete. Far more useful is an approach which can handle "deviance" as well as "normality" by adding an explicit political factor, the opportunities for involvement, into the participation equation.

#### Appendix: An Alternative Measure of Bias

Verba and Nie (1972, Ch. 15) employ exactly the opposite method from ours for expressing the relationship between participation and ideological position. They array the ideology averages by each level of political activity. This procedure is well suited to their goal of distinguishing activist preferences, which are most likely to be perceived by leaders, from non-activist preferences. Our purpose is different, to show whether people with one ideological persuasion participate more than those with another. In this respect, our analysis parallels the brief considerations in the latter portions of Verba and Nie's chapter 12 and again in Nie, Verba, and Petrocik (1976, pp. 206-09). These two procedures are, in effect, opposite ways of analyzing the same

data matrix. As such, their results are not entirely independent of one another.

We have run the analysis both ways to demonstrate that our findings are not merely an artifact of the way we chose to "percentage the table." The results from the Verba and Nie procedure applied to our data are presented in Table 2. The cell entries are the ideology means for each activity category; the higher the ideology mean, the more liberal are the respondents at that activity level. Comparing these results with those from Figure 3, it is clear that essentially the same substantive conclusions are in order regardless of which variable is spread against which variable. Contrary to patterns in earlier years, liberals are more active (as we showed in Figure 3) and activists are more liberal in 1972 and again, though less uniformly and less sharply, in 1976. As in Figure 3, inter-year comparisons in liberalism scores are perilous due to the lack of equivalence in measures.

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Table 2. Mean Liberalism by Activity Level

Year	Number of Activities Performed					
	0	1	2	3	4	5
1956	2.3	2.0	1.2	1.1	1.2	0.6
1960	2.5	1.6	1.6	1.4	1.2	1.8
1964	0.4	0.3	0.0	-1.1	0.1	-2.1
1968	0.2	0.2	0.7	0.4	0.6	-0.6
1972	-0.7	-0.3	0.3	2.0	2.2	4.1
1976	-1.3	-1.3	-0.7	-0.6	-0.5	-0.4

Source: Presidential Election Series, Center for Political Studies, University of Michigan.

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# Ideological Interpretations of Presidential Elections

TERESA E. LEVITIN  
WARREN E. MILLER  
*University of Michigan*

*This article presents a new way to define and measure the ideological sentiments of the mass electorate. Citizens are classified in terms of their evaluations and perceptions of liberals and conservatives. The measure is then used to assess the impact of ideology on the 1972 and 1976 presidential elections, to explore citizens' applications of ideological labels to parties, issues, and presidential candidates, and to describe the relationship between ideology and the potential for party realignment as well as meanings of issue voting.*

*Although many Americans use ideological labels in ways that suggest only a partial understanding of the terms and their implications, those labels have political significance for their political attitudes and election-day decisions.*

Two decades of research on political ideology in mass electorates have produced two general conclusions and a host of controversies. The first conclusion, well summarized and persuasively argued by Converse (1975), by Klingemann (1979, forthcoming), and by Stimson (1976), is that only small minorities, perhaps 15–20 percent of the electorate in the United States, Austria, Great Britain, France, Germany, Italy and the Netherlands, exhibit even the rudimentary attributes of the political ideologue.<sup>1</sup> The second conclusion is that people often use ideological symbols or labels with little comprehension of their ideological meaning, even though 70 percent or more of the citizens in these mass electorates may use them to describe the political parties (Miller and Miller, 1976, p. 844; Klingemann, 1979, forthcoming). This article classifies members of the electorate in terms of their ideological understanding of politics, and argues that the use of ideological labels to describe political objects and events, as well as the parties, the issues and the candidates involved in presidential elections, plays an important role in voters' behavior (Edelman, 1964). We shall demonstrate that examining the popular use of the ideological labels *liberal* and *conservative* provides important insights into the nature of mass electorate behavior, and that the "nonideological" use of ideological labels is an important feature of mass politics in America.<sup>2</sup>

The major controversies in the study of ideology in mass societies have focused on assessments of the prevalence and content of ideological thinking among rank and file citizens. One controversy has concerned the appropriateness of the criteria and the methods used to define and measure the prevalence of ideological thought. Stimson's (1976) discussion of some of the differences in perspective followed the earlier extended discussion of issue voting in the June 1972 *American Political Science Review* and anticipated some of the argument in the subsequent *Review* symposium (Miller et al., 1976; see also Converse, Clausen and Miller, 1965, pp. 321–36; Miller and Levitin, 1977, pp. 171–74). This article will neither address that controversy nor extend the discussion of whether, as a result of the methods used to define and measure ideological thought, the "ideological glass" is nearly empty or almost full. Rather we propose to assess the use of the terms *liberal* and *conservative* as they are applied by citizens to describe themselves, the political parties, presidential candidates, and positions on issues of public policy. Thus we will not present a content analysis of verbal responses to open-ended questions, a strategy followed by both Converse (1975) and Klinge-

<sup>1</sup>See Klingemann (1979, forthcoming) for a new set of estimates for Britain, the United States, Germany, Austria and the Netherlands; see also Barnes (1971); Butler and Stokes (1969).

<sup>2</sup>The data used in this analysis were originally collected by the Center for Political Studies of the

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mann (1972).<sup>3</sup> In our own earlier work we adopted the more common strategy of assessing intercorrelations among issue positions to test for the presence of ideological proclivities. However, this often ambiguous mode of exploring the consequences of ideological thinking, the basis for the work of Nie and Andersen, is not central to the present analysis (Miller and Levitin, 1977, pp. 171–74).<sup>4</sup> Instead, our principal measurements are based on citizens' direct application of the liberal and conservative labels to themselves and to political objects. This choice is not intended to by-pass or settle the controversies over method; it is intended to provide a new focus on the role of ideological labels in American mass electoral politics.

We focus on the ways in which citizens use ideological labels to understand the world of politics. The first section describes our measure of ideology and examines the distribution of people, and changes in this distribution, as they classify themselves on a liberal/conservative continuum. Next, we explore the relationship between ideological location, or self-placement, and vote preferences to begin to delineate the role of ideology in mass electoral politics. We attribute changes in the presidential vote between 1972 and 1976 to perceptions that are

related, in turn, to ideological self-classification and to party identification. Our next two sections examine citizens' applications of the ideological labels *liberal* or *conservative* to parties, issues, and presidential candidates. These sections demonstrate the extent to which the use of ideological labels has permeated American politics and also suggest how ideological labeling may contribute to the absence of party realignment. Finally, we present a detailed inspection of the linkage between issue positions and liberal/conservative distinctions to describe how American citizens use—and misuse—ideological labels to describe their policy positions.

Our analyses lead to two major conclusions: (1) many Americans use ideological labels in ways that suggest only a partial understanding of the terms "liberal" and "conservative" and of their implications, and (2) those labels nevertheless have political significance for these citizens, just as they do for more sophisticated voters. It has been demonstrated that a primitive and unelaborated sense of party identification links some persons to a political process from which they would otherwise be separate. We think that ideological labels may create links that help many citizens make sense out of the remote world of politics, even though they attach little systematic content to those labels. A network of associations between the ideological labels *liberal* and *conservative* and political objects may provide political anchoring. Though relatively devoid of articulable meaning, these labels may provide many people with a basis for a fuller understanding of the political world.<sup>5</sup>

#### Measuring Ideology in Terms of Liberal/Conservative Self-Labeling and Evaluation

In both 1972 and 1976 respondents in the

<sup>3</sup>For many purposes the direct coding of such material doubtless constitutes the most valid of alternative methods, particularly if the questions used to elicit the verbal responses are structured and worded to avoid coding distinctions that reflect no more than the difference between respondents' active and passive vocabularies. The early levels of conceptualization measure is open to questions not appropriate to subsequent work of Converse and Klingemann in that persons for whom relevant abstractions were a well-understood part of their *passive* vocabulary may have been mis-classified by level of conceptualization because the abstractions were simply not used as people described what they liked or disliked about the parties or the candidates. The problem may be analogous to that encountered by studies of mass response to congressional candidates. Much of the literature documenting voters' lack of knowledge about congressional candidates is based on questions that demand recall of the candidates' names as the first criterion of knowledge. If, however, the voter is simply asked about *named* candidates, the measure of voters' knowledge increases rather dramatically. In any event, where open questions ask directly what terms such as *liberal* and *conservative* or *left* and *right* mean, these problems are obviously avoided.

<sup>4</sup>It should also be noted that the interpretation of correlations among issue positions is not always straightforward and, indeed, one of the current controversies centers on that fact. See Nie (1974); Bishop et al. (1978); and Sullivan et al. (1978).

<sup>5</sup>Klingemann has suggested to us that in Europe it is likely that many citizens with only limited knowledge of the politics of neighboring countries may well make sense out of the international news of politics largely through using the left-right labels to link the vaguely familiar—their own parties—with the unknown entities abroad. If, as he further suggests, the non-ideologue understands left-right labels largely in affective terms—you can't trust the left, people on the right are kind—the linkages may transfer the affect and produce political reactions that are as important as those based on much more elaborate "ideological" views. The recent elections for the European parliament may provide an interesting test of the existence and the consequences of such linkages.

Center for Political Studies surveys were asked directly about their own associations with the terms *liberal* and *conservative* in three different ways. First, respondents were asked to locate themselves on a seven-point scale bounded by "people whose political views" were either liberal or conservative. Later in the interviews respondents were presented with a list of groups and asked to identify the groups "that [they] feel close to—people who are most like [them] in their ideas, interests and feelings about things." Liberals and conservatives were among the groups on the list. Finally, still later in the interviews, respondents rated liberals and conservatives on the "feeling thermometer."

We have combined responses to these three items to form a measure of ideological self-placement. This measure combines both cognitive and affective responses, both opinions and feelings, in order to produce a global assessment of the degree to which respondents use these labels as reference points to locate themselves in the political world. Specifically, respondents who located themselves as liberal (or conservative) on the initial seven-point scale by choosing scale positions 1, 2, 3 (or 5, 6, 7) and who subsequently said they felt close to liberals (or conservatives) were classified at the extremes of the final distribution as Ideological Liberals (or Conservatives). The two next and less extreme categories were composed of those who located their own political views as liberal (or conservative) on the seven-point scale (selecting positions 1–3 or 5–7, respectively) but who did not feel close to either ideological group. These respondents were simply classified as Liberals (or Conservatives). Those respondents who said they were in the middle, neither liberal nor conservative (point 4 on the seven-point scale), not saying they felt closer to liberals or conservatives, were classified as centrists. However, the thermometer measure was used to divide the centrists into three categories. If a respondent rated either liberals or conservatives at 60° or higher, and if that rating was at least 10° higher than the rating for the other group, the respondent was classified as a centrist with either a Liberal or Conservative Preference. Those who initially located themselves precisely in the middle of the continuum and who showed no affinity for either liberals or conservatives on either the "closeness" or thermometer measures were labeled Centrists. We maintained a residential category of those who initially said they had not previously thought of where they would locate themselves on the liberal/conservative continuum, or who gave contradictory responses in the question sequence just described.

This strategy produced a measure that combined all of the direct assessments of responses into a global rating that allows us to differentiate degrees of ideological self-placement within the mass electorate. This strategy also produced a measure that closely parallels the traditional measure of party identification. Our residual category of those not located is a reasonable match for the Apolitical category of those who do not think of themselves as Republicans, Independents or Democrats; the middle category is the ideological analogue to the classification of the Independent—Independents who reject any partisan labeling; the categories adjoining the center are based on preferences similar to those of the Independent Leaners; and the end-points of our new measure where the most ideological of the electorate locate themselves are analogous to those occupied by the persons who describe themselves as strong party identifiers. As is the case with party identification, this scale of ideological location is based on responses to structured but open-ended questions, rather than forced-choice questions, and is, therefore, based on individuals' own descriptions of their locations, affinities, and preferences. Table 1 presents the national distribution of the electorate on this measure for the last two presidential elections.

The table indicates that, in sharp contrast to the limited incidence of apolitical persons on the party identification measure where only one or two percent fall into that category, about one-third of the national electorate describe themselves as not having thought about how to locate themselves as liberals or conservatives.<sup>6</sup> Another one in ten placed themselves in the center with no preference for either ideological alternative in 1972 or 1976. The remaining 60 percent did describe themselves in terms of some degree of ideological preference. Thus, well over half the electorate can, by this measure, be said to think of themselves as expressing some ideological sentiment or having some ideological location.

The table also indicates that conservatives outnumbered liberals by a margin of almost 2 to 1 in both years. The preponderance of

<sup>6</sup>The persons classified as "Not Located" include 2 or 3 percent with inconsistent response patterns, e.g., self-placed liberals who felt closer to conservatives, etc. The problems of measurement that result from the use of the verbal stimuli "Republican, Democrat or Independent" for party identification in contrast to the labeled seven-point scale for liberal/conservative location are being explored in methodological work preparing for the 1980 CPS/National Election Study.

**Table 1. Ideological Location of the Electorate, 1972 and 1976**

	1972	1976
Ideological Liberal	7%	7%
Liberal	10	9
Liberal preference	5	4
Centrist	10	12
Conservative preference	12	9
Conservative	16	13
Ideological Conservative	10	12
Not Located	30	34
Total	100%	100%
N =	2147	2355

*Source:* The data are from the 1972 and 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

self-proclaimed conservatives preceded any post-1976 swing to the right, if such has occurred.<sup>7</sup> The lack of any substantial indica-

<sup>7</sup>An index of sentiments toward liberals and conservatives, based on thermometer ratings of the groups from 1964 to 1976 (percent favoring liberals/percent favoring conservatives) reflects the following consistently pro-conservative balance over the past seven elections:

1964	1966	1968	1970	1972	1974	1976
-8	-14	-15	-20	-19	-14	-17

The persistence of the same magnitude of preference for conservatives over liberals contrasts with the spate of semi-popular and professional writing on the general theme of a post-1972 shift to the right in American political thinking.

tion of such movement in the post-1972 period calls into question the presumption that the reelection of Nixon and Agnew and the virtual disappearance of agitation from the New Left marked the beginning of a mass reaction to and rejection of the policies and symbols of the Left. Furthermore, the distribution of the preferences is quite stable across the four-year interval separating the two independent derivations of the measure. However, it is true in this instance, as in many others, that aggregate stability conceals substantial individual instability and change.

#### Stability of Ideological Location Across Two Presidential Elections

The aggregate stability for the nation between 1972 and 1976 was the product of countervailing changes that cancelled each other. Specifically, a sense of a national move to the right might have been produced by the conservative swing among the most visible of citizens, those who participate most actively at election time. Indeed, much of the popular press suggested that the electorate had moved to the right in that period and was continuing to do so following the election of 1976. At the same time, the least active, articulate and visible (including the nonvoters) actually moved to the left in sufficient numbers to offset the changes among the more visible and active citizens.<sup>8</sup>

<sup>8</sup>A similar hypothetical situation was suggested in Campbell's *The American Voter* (1960, pp. 256-65) in a discussion of "differences in conceptualization and the interpretation of partisan change." Also see Converse (1965) for a discussion of the distinctive characteristics of those who are politically active.

**Table 2. Ideological Location in Relation to Levels of Political Participation, 1972 and 1976**

Ideological Location	Participation					
	Active Participants		Voted Only		Nonvoters	
	1972	1976	1972	1976	1972	1976
Ideological Liberal	10%	11%	9%	6%	2%	3%
Liberal	11	10	9	9	9	6
Liberal Preference	1	6	5	5	4	1
Centrist	9	9	9	11	7	14
Conservative Preference	17	10	13	10	10	7
Conservative	16	14	16	14	14	13
Ideological Conservative	14	21	14	14	3	1
Not Located	21	20	25	30	52	55
Total	100%*	100%	100%	100%	100%	100%

*Source:* The data are from the 1972 and 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

\*Because of rounding to integers, some columns do not add to 100 percent.

Table 2 presents the distributions of the ideological locations of citizens when they are sorted by their degree of participation in each of the last two presidential elections. The measure of participation that is used is the familiar separation of (1) nonvoters, (2) voters who did not participate beyond going to the polls, and (3) voters who also engaged in some other activity—predominantly consisting of talking to others and trying to persuade them to support their own party or candidate.

The distributions of all three participation groupings show some change between 1972 and 1976. However, as might be expected from the relative reduction in the number of cases alone, the smaller groups—the activists and the non-participants, each containing about 20 percent of the electorate—show more “wobble” suggesting individual change than does the much larger middle group of voters who make up almost 60 percent of the electorate. Moreover, among the nonvoters, the proportion showing no ideological preference—the centrists *and* those not located on the measure—increased by a full 10 percentage points between 1972 and 1976. There was, however, no similar change at all among the active participants. Within the non-voter and voted-only groups, the net result of the changes suggests no large shift in the liberal-to-conservative ratio, and certainly no major shift to the left. Among the active participants, however, there is some suggestion of a shift in the net balance; but it was a shift to the left, a shift that increased the number of liberals and decreased the number of conservatives. At the same time, the growth of liberal strength among active participants was entirely among those with Liberal Preferences; on the conservative side, although larger losses occurred among the conservatives and those with only a Conservative Preference, the number of Ideological Conservatives actually increased by a full 50 percent (from 14 to 21 percent of all active participants). Within the ranks of the political activists, therefore, the balance between the more extreme groups of Ideological Liberals and Ideological Conservatives thus shifted from less than 3 to 2 conservative over liberal in 1972 to almost 2 to 1 conservative over liberal in 1976. This latter change could have contributed to a widespread—though mistaken—impression of a more general national swing to the right throughout the electorate. This shift to the right by a very small minority of activists was actually more than offset by a series of smaller shifts to the left among the remainder of the electorate, but the shift leftward occurred among less visible citizens, and was, therefore, overlooked by many politi-

cians and political analysts alike.<sup>9</sup>

Our search for countervailing shifts among politically significant subgroups in the population was extended to groups of party identifiers. Among Democrats and Independents, particularly among the latter, there was a small decline in conservative strength between 1972 and 1976. In both groups this was largely the result of Conservatives and Centrists with Conservative Preferences in 1972 describing themselves only as Centrists or as having no ideological location by 1976. Among Republican identifiers and sympathizers the pattern of change was markedly different. Those who described themselves as any type of conservative increased slightly in *total* numbers, from 60 to 63 percent. However, the largest change was in the proportion of Ideological Conservatives among Republicans; they increased sharply, from 24 percent in 1972 to 32 percent in 1976.

Thus it appears that, although there was no national swing to the right between 1972 and 1976, a swing to the right did occur among Republicans as well as among the most active of the electorate. This documentation of a shift to the right among Republicans is consonant with the strong support given to Ronald Reagan (in 1976) who received more votes than Gerald Ford in the Republican primaries. But politicians and political analysts who concluded that support for Reagan reflected a general move to the right by the entire electorate were incorrect; in fact, such movement was to be found only among Republican partisans.

The analyses of interelection shifts in ideological locations within different levels of political participation and within different categories of party identification provide evidence of some individual-level change and instability of ideological sentiments. However, much of the change in the balance of ideological preferences was concentrated within the more partisan and most politically active groups. The

<sup>9</sup>A very similar problem in elite perceptions and interpretations of change in public opinion was examined in our earlier analysis of changing attitudes toward the politics of the New Liberals in the period 1970 to 1972 (see Miller and Levitin, 1977, pp. 93–118). In that instance, however, the true magnitude of the shift to the left was *underestimated* by most political observers because the shift of the less active was *congruent* with the movement of the more active—away from the right toward the center as active citizens moved from the center to the left. In the current instance, the invisible shift of the inactive more than countered that of the high participants, but the invisibility of their move led to an *overestimation* of the pervasiveness of movement to the right.

relative concentration of such change in such groups—groups that usually reflect the greatest stability in their attitudes as well as their behavior—suggests that these changes were neither random nor the result of unreliable measurement. When this interpretation of observed change is taken into account, an overall estimate of the stability of ideological location suggests that it may be useful for the continuing analysis of American electoral behavior.

A summary of the stability of ideological location at the individual level can be provided by a continuity correlation. This correlation can be calculated across the four-year interval for that subset of respondents who formed the 1972–1976 panel in the CPS election study series. In order to attach some relative meaning to this correlational summary, we also computed the correlation for party identification (one of the constants for electoral analysis) and for each of the policy questions probed with a seven-point scale in both studies (questions that have been used to assess changes in the prevalence of ideological thought).<sup>10</sup> The comparable correlations were .80 for party identification, .65 for ideological location and an average of .46 across policy preferences on nine issues.<sup>11</sup>

<sup>10</sup>Analogous continuity correlations for party identification and issue positions are reported for two-year intervals in Converse and Markus (1979). To increase comparability with their work, particularly with the correlations based on seven-point scales of policy preferences, we have followed identical procedures here in handling “missing data” categories—including the eighth category composed of those who could not be located on the liberal-conservative dimension. However, the four-year continuity correlations reported here are not strictly comparable to the Converse-Markus correlations. Most importantly, their Party Identification correlations (averaging .71) are based on virtually the entire population (excluding only the apoliticals, minor party adherents and the “NA”), while the party, ideological and issue correlations reported in this article are based on only the 60–65 percent who held relevant ideological attitudes at both points in time. When those who “fall out” on the ideology or issue correlations are also excluded from the Party Identification correlations, the Party Identification measure rises to the reported .80. Three different measures have been used in the construction of the ideology measure—although they are not all used on all of the population but are employed only selectively—and the multiple-item base may contribute something to the greater stability of the ideology measure over that of the issue measures.

<sup>11</sup>The continuity correlation for ideological locations goes up to .74 when limited to persons who were high in political participation both in 1972 and in

The full meaning of the differences in these correlations is not obvious. In the absence of national political organizations or parties of liberal and conservatives, and of national elections institutionally defined as competitions between liberals and conservatives, it is hard to imagine circumstances in the United States under which ideological locations would be as well anchored and as stable as partisan locations. On the other hand, these correlations indicate that ideological location was much more stable than were the several expressions of policy preference which it is sometimes presumed to summarize. Why this should be so is not clear unless, as we shall suggest, ideological location is more than a summary statement of policy preferences. John D. Holm and John P. Robinson (1978) draw just such a conclusion. In any event, the measurements we are using are available only for the two presidential elections being reported here, and we do not know how the ideological character of the national electorate during this brief period compares with that of other eras.

Monitoring changes in individuals’ ideological location can help us to understand its role in electoral behavior. In the meantime, we should remember that the importance of the concept and measurement of party identification goes beyond the fact that voters consistently classify themselves in terms of their party loyalties. Rather, the principal value of measuring party identification lies in how it can be used to understand the voting choices, responses to issues, evaluations of candidates, and other political attitudes and behavior of the electorate. So too, the value of conceptualizing and measuring liberal/conservative ideological location as we have proposed will rest on its contribution to understanding the political attitudes and behavior of the electorate. The remainder of this article will focus on some of

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1976 and it drops to .44 among the habitual non-voters. Given the volatility with which perceptions, attitudes and political beliefs of nonparticipants appear to change, and given the minimal impact that nonparticipants have on election outcomes, it may be useful in future work to consider a general analytic strategy for studies of electoral behavior that eliminates nonparticipants at the early stages of any analysis that concentrates on partisan choice. As can be seen in Table 2, estimates of meaningful national parameters change with the exclusion of habitual nonvoters who constitute a large group that includes 60–70 percent with no ideological preference at all and no more than 4 to 5 percent who fall in either extreme category of liberal-conservative location.

the relationships between ideological self-labeling and responses to the world of politics.

### Ideological Location and the Vote

It is generally agreed that ideological concerns were especially salient in the 1972 election, an election said to be uniquely influenced by issue voting, the presumed expression of these ideological concerns. The election of 1976 was not an issue election, however. Ford's congressional record as a conservative Republican was not a major focus of discussion, and many observers and reporters saw Carter as neither conservative nor liberal, but, instead, as "fuzzy" on major questions of governmental policy. Most analysts agree that in the 1976 election party loyalties and candidate preferences were once more the most important determinants of the vote for substantial proportions of the population, and that ideological concerns and issue voting were of far less consequence than in 1972.

To assess the relative contribution of party identification and political ideology to the vote outcome in both elections we examined the relationship between them and then proceeded to a Normal Vote Analysis of the two years.<sup>12</sup> Table 3 shows how citizens classified themselves in 1976 both as to ideological location and partisanship. The joint distribution of the two measures is very much the same in the two

years and so only the data for 1976 are presented here.

As Table 3 indicates, the Democrats were rather evenly divided between liberals and conservatives, and relatively few Democrats across all categories of liberals could be classified as Ideological Liberals. Among Republicans, however, Ideological Conservatives were the modal group, and among strong Republicans, where conservatives outnumbered liberals by 72 to 4, Ideological Conservatives outnumbered other conservatives by 39 to 33. Thus, Republicans were more homogeneous than Democrats in their ideological self-placement, and they were also much more often unqualified in their ideological commitments.<sup>13</sup>

The comparative normal vote analysis presented in Figure 1 permits us to evaluate the direct role of ideological self-placement, apart from partisanship, in determining the actual vote divisions. As can be seen from Figure 1, ideological concerns apparently made a separate and significant contribution to the vote decision in both 1972 and 1976.<sup>14</sup> Although the

<sup>13</sup>Here, as in other bivariate distributions of party identification and the use of ideological labels, the ideological cast of partisan leaners is apparent. Within their respective party groups, the Independent Leaners are more likely to locate themselves on the liberal/conservative continuum than are the strong party identifiers. Moreover, they are more homogeneous in their ideological self-characterization than are the weak identifiers (see Brody, 1977; Miller and Levitin, 1977, pp. 251-53).

<sup>14</sup>The "expected" values are derived using the Normal Vote algorithm developed and first presented in Philip E. Converse (1966). The derivations of the long-term and short-term indices are after Richard W. Boyd (1972a, 1972b).

Table 3. Party Identification in Relation to Ideological Location, 1976

Ideological Location	Party Identification						
	D <sub>s</sub>	D <sub>w</sub>	I <sub>d</sub>	I <sub>i</sub>	I <sub>r</sub>	R <sub>w</sub>	R <sub>s</sub>
Ideological Liberal	11%	11%	14%	3%	2%	1%	1%
Liberal	13	10	14	9	7	4	2
Liberal Preference	5	4	8	5	4	2	1
Centrist	11	15	11	15	10	11	2
Conservative Preference	6	7	8	10	14	13	10
Conservative	10	11	9	9	19	21	23
Ideological Conservative	4	6	7	6	27	20	39
Not Located	40	37	30	42	19	28	23
Total	100%*	100%	100%	100%	100%	100%	100%

Source: Data are from the 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

\*Because of rounding to integers, some columns do not add to 100 percent.

correlation between partisanship and ideological position did increase slightly by 1976, as reflected in the increase in the long-term index, from 9.5 to 11.5, there was virtually no decline in the independent contribution of liberal-conservative sentiments to the vote decision between the Nixon-McGovern and the Carter-Ford contests.<sup>15</sup> The conventional wisdom about differences in the ideological ambience of the two elections suggests that ideological preferences should have been far more highly associated with the vote in 1972 than in 1976. Yet the impact of ideological sentiments on the vote was virtually the same in both years. Party voting may have been more important to the election outcomes in 1976 than in 1972, and issue voting may have been less important in 1976 than in 1972, but voting in accord with one's ideological sentiments was virtually as prevalent—independent of party voting, as reflected in the short-term indices—in 1976 as in 1972.<sup>16</sup>

This normal vote analysis draws attention to a continuing and significant relationship between ideological location and presidential vote choice in both 1972 and 1976, a continuity that has not been widely noted. The analysis also permits identification of those who changed their vote from Republican to Democrat, thereby describing more precisely the

source of continuity and change in voting behavior associated with differences in ideological location. Figure 1 reveals substantial variations in the extent of this change across the liberal-conservative continuum. The fortunes of the Democratic candidate improved by almost the same margin among Ideological Conservatives as among Ideological Conservatives between 1972 and 1976. Larger increases in the Democratic vote were located within all but one of the other five categories of ideological location.

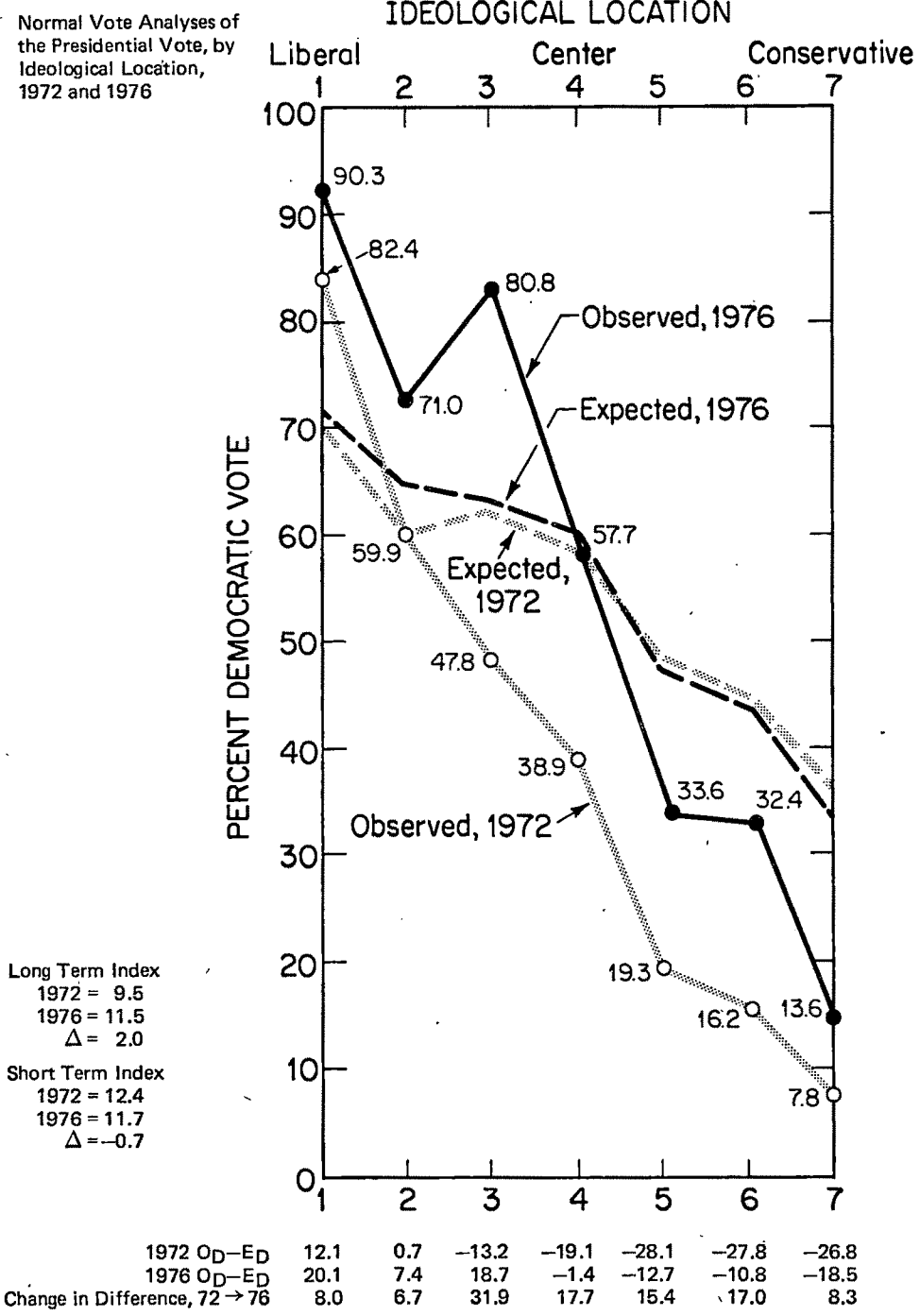
Once again, however, conclusions appropriate to large groups of the population may be modified by analyses permitting more detailed scrutiny of strategic subgroups. Table 4 indicates that changes in vote decisions among liberals, centrists and conservatives were differently influenced by the partisanship of the voters within each of these ideological groups. With the expected "normal" vote as the baseline for comparisons, it is clear that changes in vote choices between 1972 and 1976 were primarily a matter of most centrists and certain conservatives voting more Democratic and less Republican in the 1976 election. The largest changes were among centrist as well as conservative Democrats, centrist and conservative Independents, and among centrist Republicans. Liberals, including liberal Democrats, and conservative Republicans showed almost no net change in their vote decisions. Thus, while it is not incorrect to conclude that the pro-Democratic vote changes were to be found primarily among conservatives and centrists, such general conclusions obscure the important fact that there was almost no change among liberal Democrats or among conservative Republicans, who together make up some 40 percent of more of the voting public.

The more differentiated view of the electorate presented in Table 4 also reveals the extent to which those who switched their vote were concentrated among Democrats and Independents who constitute the continuing potential base for a national realignment and therefore for change in the partisan balance within the electorate. While it is true that we have documented change over only two elections, the relatively high proportion of conservative and centrist Democrats and conservative Independents who changed their votes in the short term to give greater support to the Democratic candidate raises questions about the likelihood of long-term changes that would result in party realignment. In 1972 it was the conservative Democrats who deviated most from their normal vote division, followed by conservative Independents and centrist Democrats. Conser-

<sup>15</sup>The differences in language remarking on the "sharp increase" of Identifying Conservatives among Republicans (p. 12, above) and noting a "slight increase" in the present context is a result of the difference between emphasizing the diagnostically useful location of individual-level change and observing the net consequences of such change.

<sup>16</sup>The Boyd statistics for assessing the party-relatedness of ideological location and the party-independent contribution of ideological location to the vote choice indicates that the long-term index increased from 9.5 in 1972 to 11.5 in 1976 while the short-term index dropped only from 12.4 to 11.7. This article provides a different perspective on ideological voting from the investigation of the continuing impact of New Politics sentiments on the presidential vote we described in *Leadership and Change* (1977). Although New Politics sentiments exhibited a relatively constant relationship to liberal/conservative self-placement on the seven-point scale in the 1972-76 period, the independent contribution of the New Politics sentiments to the vote choice dropped sharply between 1972 and 1976. Since the two measures were intercorrelated at .33 in 1972 and .42 in 1976, the differences in their independent relationship to the vote in the two years suggests a clear difference in the extent to which the two electoral contests stimulates responses to two somewhat different dimensions of ideological thought.





Source: The data are from the 1972 and 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

Figure 1. Normal Vote Analyses of the Presidential Vote, by Ideological Location, 1972 and 1976

Table 4. Change in Deviations of Observed Democratic Vote from Expected Democratic Vote, 1972-76\*

Ideological Location	Party Identification		
	Democratic	Independent	Republican
Liberal	+ 4 (18)	0 (3)	+ 8 ( 4)
Centrist	+28 (19)	+16 (6)	+15 (12)
Conservative	+28 (12)	+24 (3)	+ 2 (23)

Source: The data are from the 1972 and 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

\*A positive entry means the observed Democratic proportion of the two party vote increased, relative to the Normal Vote expected proportion Democratic, between 1972 and 1976. The approximate proportions of the classifiable portions of the population located in each cell of the cross-classification are shown in parentheses. Approximately 35 percent of the total electorate cannot be categorized because they report no ideological location and no party identification.

vative Democrats gave Nixon a bonus of 45 percentage points beyond their normal Republican vote. In 1976 they joined other conservatives, again favoring the Republican candidate, but this time with a bonus of only 17 percentage points over normal (a 28-point increase in their Democratic vote). Change between 1972 and 1976 in short-term deviations was in the direction of conservative Democrats "returning" to their partisan homes, even though that return fell 17 percentage points short of a full restoration to a normal party vote. Table 3 documents the familiar message that an electorate that is 3 to 2 Democratic and 2 to 1 conservative must have a lot of conservative Democrats; the normal vote analyses underlying Table 4 reveal both the propensity of these conservative Democrats to vote Republican and the volatility with which they undergo short-term change. The question of whether conservative Democrats will ultimately be moved to partisan realignment is central to the possibility of a major party realignment.

#### Ideological Location, Perceptions of Parties and Candidates, and Party Realignment

Much of the literature on the potential for realignment assumes that individuals experience tension when their ideological sentiments are incongruent or in conflict with their partisan positions. Since this tension is presumed to be unpleasant and uncomfortable, it is further presumed that individuals with dissonant political beliefs will reduce this tension by changing their partisan loyalties so that they become

more consistent with their ideological views. This line of argument also assumes that there is wide consensus across the electorate as to which sets of views are consonant and which are dissonant. Many of the expectations of realignment have assumed that conservative Democrats must experience dissonance because the congruent patterns of political beliefs and values are found in the liberal-Democratic and conservative-Republican pairings.

However, one may reasonably suggest that the perceptions that create feelings of dissonance and congruence are, themselves, heavily influenced by voters' ideological and partisan positions. If so, many conservative Democrats may remain both conservative and Democratic because they do not perceive or experience any conflict between their partisanship and their ideological stance. They may perceive issues, parties and candidates in a manner consistent with both their partisan and ideological commitments, even though those perceptions may differ from the perceptions of most others.

Ideological views which deviate substantially from the "normal" range of views are often taken as *prima facie* evidence of ignorance about, lack of interest in, or a misunderstanding of partisan ideological reality. But these same "misperceptions," which are sometimes used as evidence of the absence of ideology, may also be seen as evidence of elaborate, well-integrated systems of political beliefs. Seemingly incongruent ties to an ideological location and a partisan commitment may lead to views of the political world which—even though "wrong"—are experienced as consistent by those holding them and indicate great involvement rather than lack of

interest in politics.

For this argument to hold, ideological location must function, at least in part, as a filter through which parties and candidates are seen and evaluated. This argument also presumes that ideological location is a relatively fixed vantage point from which people view the political world. However, partisanship may be the more fundamental anchoring point. If so, perceptions of the partisan nature of candidates and issues would define the choice of ideological location, and the nature of the perception could not be attributed to the influence of a prior ideological location. In such a case ideological location would change with changing political circumstances and particularly with changes in perceptions of partisan-based politics. It is not our intention to try to identify those for whom partisanship is a more fundamental political anchor than ideological commitment, or vice versa. Moreover, we recognize that the relationships are complex, that partisanship and ideological beliefs function in different ways for different people, and that establishing causal links is most problematic. However, the data available to us do suggest that ideological location is a *relatively* stable and *relatively* independent vantage point from which many citizens view politics. The circumstances of the two elections of 1972 and 1976, in which the roles of candidates, issues and

party loyalties were so different, provided one test of the stability of ideological location: the evidence suggests that location was not highly responsive to changes in the political situation. Thus, ideology may serve as a filter through which those citizens who hold "objectively" incongruent political views may organize the world of politics in ways that are consistent and congruent for them. And it may also serve as a basis for organizing the world for those citizens with "objectively" congruent political views as well. Table 5 demonstrates the extent to which ideological beliefs do, in fact, serve to orient voters to parties and candidates. It presents the joint relationship of one's own ideological location and partisanship to one's perceived proximity to each of the parties and to each of the candidates in 1972 and again in 1976.

As can be seen from Table 5, there is a strong relationship between where one places oneself ideologically and one's perceptions of ideological locations of the parties and their presidential candidates. In both years Democrats were consistently more likely than Independents or Republicans to see the Democratic party, rather than the Republican party, as close to their own ideological locations. In like manner, Democrats were more likely to see the Democratic candidate, rather than the Republican candidate, as close to their own ideological locations. Independents, particularly centrists

Table 5. Perceptions of Relative Proximities to Parties and Candidates  
vis-à-vis Ideological Location, 1972 and 1976\*

	Party Identification					
	Perceived Proximity to Parties			Perceived Proximity to Candidates		
	Democratic	Independent	Republican	Democratic	Independent	Republican
1972 Ideological Location:						
Liberal	+77	+45	+52	+74	+43	+33
Centrist	+11	- 6	-26	-10	-21	-48
Conservative	-27	-64	-79	-50	-69	-86
1976 Ideological Location:						
Liberal	+84	+42	+36	+81	+31	+33
Centrist	+15	0	-14	+21	+10	-12
Conservative	-17	-44	-83	-14	-29	-77
Pro-Democratic Change:						
Liberal	+ 7	- 3	-16	+ 7	-12	0
Centrist	+ 4	+ 6	+12	+31	+22	+36
Conservative	+10	+20	+ 4	+36	+40	+ 9

Source: The data are from the 1972 and 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

\*Entries for 1972 and 1976 indicate the preponderance of those who see the Democratic entity as closer (+) or the Republican entity as closer (-). The 1972-1976 change is scored so that an increase in the preponderance of those more proximate to the Democratic entity is (+) and an increase in proximity to the Republican entity is (-).

and liberals, were somewhat more likely than Republicans to see themselves as closer to Democratic rather than Republican positions, but clearly less likely to do so than Democrats. Republicans were most likely to see the Republican party and Republican candidates as close to their own ideological locations.<sup>17</sup>

The association between partisanship and perceived proximity to parties and candidates is overshadowed by the magnitude of the differences in perceived proximities across the ideological categories *within* partisan groups. Among Democrats, Independents and Republicans alike, those with liberal sentiments, when compared with centrists or conservatives, more often saw Democrats rather than Republicans as closer to their own ideological locations, while conservatives consistently located Republicans rather than Democrats as closer to their own ideological location.

The magnitudes of these differences indicate a linkage between the voters' own ideological location and their perceptions of the ideological location of the parties and their candidates. This linkage is stronger than the partisanship of the conservative Democrats and of the liberal Republicans in influencing their evaluations of parties and candidates. At the same time, as we have noted, their partisanship does modify these perceptions. In short, both party loyalty and sense of ideological location appear to influence perceptions of the relative ideological locations of both parties and candidates.

Perceptions of the relative ideological proximities of the two pairs of presidential candidates changed considerably between 1972 and 1976. The changes were concentrated among centrists of all partisan stripes and among conservatives, except for conservative Republicans. Table 5 demonstrates that assessments of McGovern and Nixon were different from those of Carter and Ford, and that the differences in assessments varied with the voters' own ideological and partisan positions. Furthermore, the magnitude of the differences indicates that differences between 1972 and 1976 were more pronounced because of the change in candidates than because of changes originating with parties. (The average change in the measures of proximity to parties was 9; in the measures of proximity to candidates, it was 21.) This is not surprising. Other studies have demonstrated both the continuity of evaluations of party and

the variability introduced by the unique characteristics of each party's nominee. However, Table 5 provides important inferential evidence that the ideological assessments of the candidates, not the ideological assessment of the parties, produced the patterns of vote changes presented in Table 4.

This conclusion can be inferred from the fact that the changes in perception associated with the candidates rather than those associated with the parties (see the bottom three rows of Table 5) produce a pattern very similar to the pattern of entries in Table 4, entries which described changes in the deviations of observed vote from expected normal vote within the same partisanship-ideological location matrix. This similarity suggests that vote changes associated with the ideological location of voters were a function of voters' assessments of the ideological positions of the candidates. A more detailed exploration of how the candidates were perceived by citizens with different ideological sentiments further clarifies how a voter's own ideological position becomes a factor in evaluating the candidates' ideological sentiments. A new measure combining two questions in the 1976 CPS study that linked candidates to ideological positions was created for this purpose.

#### Perceptions of Candidates' Ideological Positions and Realignment

First, all persons who were able to place themselves on the seven-point scale bounded by "people whose political views" were either liberal or conservative, were asked to locate Ford and Carter, successively, on the same continuum.<sup>18</sup> Of the 54 percent of the total population who placed the two candidates at different locations on the scale, 46 percent located Carter as more liberal than Ford, while the remaining 8 percent saw Carter as more conservative than Ford.

Second, at a later point in the study, respondents were also asked to select from a list of groups (e.g., women, young people, farmers,

<sup>17</sup>Independent Leaners are included with other partisan groups; therefore the Independents consist only of "pure" Independent-Independents.

<sup>18</sup>Approximately 31 percent of the total population did not locate themselves on the initial seven-point item; an additional 8 percent of the total population could not locate either candidate; and approximately 7 percent placed both candidates at the same place on the seven-point scale. The proximity measures reported in Table 5 were derived from the locational data provided by the 61 percent of the total population who could locate the candidates as well as themselves.

southerners) those whom they expected that Carter, as president, would be likely to help. The same list was then presented again, and respondents were asked to select those groups who would be likely to be helped by Ford if he were elected. Liberals and conservatives were included in the list. Some 36 percent of the respondents volunteered the belief that Carter would help liberals or that Ford would help conservatives, or both; one in 20 (5 percent) thought that Carter would help conservatives or that Ford would help liberals, or both.<sup>19</sup>

We combined responses to perceptions of the candidates' liberal-conservative sentiments with judgments of which ideological group might be helped by which candidate to create a two-item summary measure of how voters saw the candidates aligned on liberal-conservative differences. This summary measure indicates that 29 percent not only saw Carter as more liberal than Ford but also thought Carter would help liberals or Ford would help conservatives, or both. Another 16 percent produced the same pairing, matching Carter with liberal views or Ford with conservative views, but did so on only one of the two measures, not on both. Eight percent of the total population had the order reversed on one or both of the measures; thus, for example, Carter was seen either as more conservative than Ford or as more apt to help conservatives. Five percent saw no dif-

ference on either measure, and 3 percent produced one ordering on one measure but the reverse on the other; that is, for example, they viewed Ford as more liberal and as more apt to help conservatives than Carter. The remaining 39 percent could not be categorized because they did not locate themselves or the candidates on the initial seven-point liberal/conservative scale.<sup>20</sup>

Table 6 presents the distribution by party identification and ideological location of that 45 percent of the electorate who not only saw Carter as more liberal than Ford but also thought Carter would help liberals or Ford would help conservatives, or both. It can be seen from Table 6 that perceptions of the candidates' ideological sentiments vary as a function of the perceiver's own partisanship and ideological location. Virtually all of the most liberal Democrats and a great many among the most conservative Republicans gave the same answers to both questions. They were most apt to locate the candidates, and they consistently paired Ford with conservative positions and Carter with liberal positions.

In this instance, as in all others in which respondents were asked to (a) locate the parties on the same dimension, (b) specify the relative policy preferences of the candidates, and (c) identify the relative issue positions of the parties, nearly all liberal Democrats and conservative Republicans had the same perceptions of

<sup>19</sup>Over half the population, 56 percent, saw no difference between the candidates, either seeing both as helping one or both groups or seeing neither as helping either liberals or conservatives. About 3 percent gave no response.

<sup>20</sup>Thirty-one percent did not classify themselves on the initial liberal/conservative self-placement question; 9 percent could not locate one or both candidates on that same ideological continuum.

Table 6. Prevalence of Perceptions that Carter is More Liberal than Ford, 1976\*

	Party Identification						
Ideological Location	D <sub>s</sub>	D <sub>w</sub>	I <sub>d</sub>	I <sub>i</sub>	I <sub>r</sub>	R <sub>w</sub>	R <sub>s</sub>
Ideological Liberal	88	83	91	33	**	**	**
Liberal	47	55	57	22	**	**	**
Liberal Preference	43	42	53	50	**	**	**
Centrist	28	25	24	13	21	19	**
Conservative Preference	31	34	18	19	22	46	39
Conservative	7	15	21	26	49	41	38
Ideological Conservative	**	36	40	47	87	57	74

Source: Data are from the 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

\*Entry is the proportion of the persons within the cell who rated Carter as more liberal than Ford on the seven-point liberal-conservative scale and who saw Carter as helping liberals or Ford as helping conservatives if elected.

\*\*Too few cases to permit a reliable estimate.

both candidate and party positions, in 1976 as well as in 1972. Carter may have seemed fuzzy on the issues to many, and Ford's conservatism may have been blurred for some by the contrast with Reagan. But within the mass electorate these most ideological of voters, even though they held diametrically opposed ideological positions, agreed almost completely in their perceptions of the candidates' ideological sentiments. For conservative Republicans and especially for liberal Democrats, there was little ambiguity in their understanding of partisan differences whenever those differences were expressed in ideological terms.

However, this shared view of the world of partisan politics was not even the majority view in most other partisanship-ideology groupings. Although there are too few cases to evaluate the perceptions of liberal Republicans, relatively few of their ideological and partisan opposites—the conservative Democrats—shared the perceptions held by both liberal Democrats and conservative Republicans.

Table 7 also shows the divergence between the conservative Democrats and both the liberal Democrats and conservative Republicans in their perceptions of partisan ideological differences. This table is based on the 8 percent of the population that equated Democratic with conservative and Republican with liberal when locating the candidates. As Table 7 reveals, virtually no conservative Republicans or liberal Democrats held such views. Those heterodox perceptions were heavily concentrated in the conservative-Democratic portion of the matrix, along with occasionally disproportionate representation among some centrists and some Independents. This general pattern was found in every instance in which the ideological associa-

tions of parties, candidates and issue positions could be assessed, both in 1972 and 1976. Liberal Democrats and conservative Republicans have very similar perceptions of ideology-partisan linkages or associations, while conservative Democrats perceive a quite different structure of relationships among partisan and ideological alternatives.

The concentration of heterodox orderings among conservative Democrats is apparently not the result of several possibly confounding factors such as region or education. It is not because conservative Democrats are disproportionately from the South; northern Democrats more often have heterodox perceptions than do southern Democrats and northern conservative Democrats have them more often than southern conservative Democrats, particularly among those Democrats categorized as Ideological Conservatives. Contrary to what one might expect after reading the work of Converse and Klingemann, these heterodox perceptions are not associated with relatively limited education, limited exposure to the mass media, limited interest or involvement in political affairs, or limited political participation. Often as not, it is the best educated, most involved and most active among conservative Democrats who are most inclined to hold heterodox perceptions of the ideological locations of the parties and the candidates. This apparent contrast with earlier work may not seem quite so startling once it is recognized that the present analysis is confined to those persons who could be classified with our new measure of ideological location. The 34 percent of the electorate who were not so classified (see Table 1) may well include many persons who, if asked to assign ideological labels to parties and candidates, would often

Table 7. Prevalence of Perceptions that Ford is More Liberal than Carter, 1976\*

Ideological Location	Party Identification						
	D <sub>s</sub>	D <sub>w</sub>	I <sub>d</sub>	I <sub>i</sub>	I <sub>r</sub>	R <sub>w</sub>	R <sub>s</sub>
Ideological Liberal	0	0	7	8	**	**	**
Liberal	9	7	8	16	**	**	**
Liberal Preference	19	11	19	5	**	**	**
Centrist	8	14	17	8	24	13	**
Conservative Preference	15	16	0	19	9	6	5
Conservative	38	19	17	4	5	5	12
Ideological Conservative	**	24	24	14	0	6	2

Source: Data are from the 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

\*Entry is the proportion of persons within the cell who rated Ford as more liberal than Carter on the seven-point liberal-conservative scale or who saw Ford as helping liberals or Carter as helping conservatives if elected.

\*\*Too few cases in cell to permit a reliable estimate.

produce heterodox orderings out of confusion or ignorance. Such persons are also limited in their political resources and activities (see Table 2). It is likely that they have been included in earlier work that used "mislabeling" as a criterion of ideological comprehension and political sophistication. Thus, studies of citizens with heterodox views may have grouped together two very different types of citizens: the genuinely apathetic, or ignorant for whom ideology is irrelevant, and the active and informed for whom ideology provides a frame of reference from which seemingly heterodox views are perceived as reasonable, consistent and appropriate. Future studies of ideological beliefs must distinguish between these two types of citizens.

The same caveat may also need to be extended to other categories of response. Among liberal Democrats the incidence of "No Difference" or "Don't Know" responses is a direct function of lack of education, little use of the media, minimal involvement or participation; the proportions of such responses increase as education, media use, involvement and participation decrease. However, among conservative Democrats, these relationships are weakened and, at times, even reversed. As with reversals in the ideological orderings of political objects, the absence of perceptions differentiating party and candidate ideological location may indicate something other than error born of ignorance or indifference. As with heterodox ordering, non-ordering may be a function of a clearer understanding of and interest in politics and a higher level of ideological sophistication than has usually been attributed to citizens making such responses.

None of this denies the conclusion that deviations from the orthodox, from perceptions such as those shared by liberal Democrats and conservative Republicans, are often—and undoubtedly most often—indications of limited ideological understanding. For example, among those who are classified as centrists because they do not label themselves as ideologues, ideology is, as would be expected, of limited relevance in evaluating the rest of the political world. Among the centrists, the "don't know" response is common, as are inconsistent portrayals of party and candidate positions. Their various patterns of ordering parties' and candidates' ideological locations probably reflect confusion, inattention, indifference, and a limited grasp of the ideological elements of politics.

Taken together, Tables 6 and 7 suggest that conservatives and liberals perceive the world of politics with persistently ideological frames of

reference. They are, in short, responding predictably both if one knows that they hold ideologically strong and consistent views and if one assumes that these views serve as a context within which to perceive and evaluate political issues, people, and ideas. However, those ideologues whose ideological positions seem to come into "conflict" with partisan commitments hold perceptions of the political world different from the perceptions shared by other ideologues. While many conservative Democrats and liberal Republicans report that they don't know the candidates' ideological locations, a substantial minority hold views about the ideological positions of the candidates that are opposite of those reported by other ideological groups in the electorate.

We do not know the extent to which conservative Democrats and liberal Republicans experience tension because of a divergence between their partisan loyalties and their ideological commitments. Nor do we know, if tension exists, how it is resolved—if at all. Although one may plausibly suggest that an ideological preference may serve as a filter or frame of reference through which political candidates, parties, issues, and events are perceived and evaluated and that this frame of reference may provide a consistent, coherent view of politics, no one has yet developed relevant and direct measures of the existence of tension and of the processes by which it may be reduced. We do know that conservative Democrats and liberal Republicans demonstrate a heterodox structure in their perceptions of political phenomena. However, as we noted earlier, much of the theorizing about the prospects for partisan realignment has presumed that conservative Democrats have the same view of the political world as do liberal Democrats and therefore feel some pressure to achieve consistency by changing their partisanship to match their ideology. The data we have examined suggest that many individuals thought by the political analyst to experience inconsistency, and therefore tension, have instead achieved some congruence between their partisanship and their ideological positions. The vast majority of conservative Democrats view parties, candidates, and issues in a way compatible with both their partisan and ideological commitments.

In sum, the continued absence of the partisan realignment which would result from conservative Democrats—or liberal Republicans—shifting their party loyalties may be partially explained by their unique perceptions of the ideological locations of the parties and the presidential candidates. In 1976, liberal

Democrats and conservative Republicans were nearly identical in their perceptions of the ideological differences separating Carter from Ford and Democrats from Republicans. Yet, relatively few conservative Democrats joined this consensus. Instead, most conservative Democrats perceived the ideological locations of both the parties and their candidates so as to either eliminate or greatly reduce the possibility of conflict between their party loyalties and their ideological commitments. Therefore, it is reasonable to suggest that only the minority who share the dominant perceptions of the liberal Democrats and conservative Republicans (as shown in Table 6) are likely candidates for a change in partisanship that might produce some degree of party realignment.

These results suggest not only substantive conclusions about the potential for realignment but also methodological concerns about the meaning of some of the common measures of ideology. Those who hold heterodox political beliefs may do so because they are highly ideological or because they are uninformed and lack interest. If both a highly developed ideological filtering system and a confused or apathetic posture toward politics can be responsible for heterodox views of the positions of parties and candidates, analyses must separate the two. Further, correlational tests of constraint will be flawed if heterodox ideologues are not separated from orthodox ideologues. However, the heterodox ideologues would be hard to identify in most previous correlational tests of ideology precisely because the inversions indicating their ideological commitment are the same inversions expected among the nonideological, uninterested members of the electorate.

### Ideological Location and Some Implications for Issue Voting

It is often simply presumed that ideological sentiments represent some summary position on numerous questions of government policy. We have suggested, however, that the concept of ideological location (like the concept of party identification) may have many meanings or functions, only one of which is as an abstract or general summary of discrete policy positions. Ideological position is correlated with many different expressions of policy preferences (see Miller and Levitin, 1977, p. 264). As Table 8 shows, there was a modest relationship, both in 1972 and 1976, between ideological location and several statements of policy preferences. The correlations with issue positions are also reasonably stable across the same period. At the same time, the correlations are of a relatively modest magnitude; they are certainly too small to warrant the conclusion that ideological location is simply a summary of policy preferences (but cf. Miller et al., 1976, pp. 776-78).

The certain error of that conclusion becomes apparent as we look next at how individuals link their own policy preferences to the ideological meanings they give to policy alternatives. In both 1972 and 1976 respondents were asked what positions liberals and conservatives would take on each of the two different issues. Table 9 presents proximity measures which join the voters' issue positions and the voters' understanding of the positions of liberals and conservatives on these issues.

When Table 9 is compared with Table 5, it is clear that differences between the relative proximities to perceived liberal and conservative positions on issues (within party iden-

Table 8. Relationship Between Ideological Location and Policy Preference on Nine Issues, 1972-1976\*

	Jobs and Standard of Living	Rights of the Accused	Busing	Aid to Minorities	Govern- mental Health Insurance	Urban Unrest	Tax Policy	Equal Role for Women	Legaliza- tion of Marijuana	Average
1972	.31	.32	.38	.37	.28	.29	.12	.20	.35	.29
1976	.32	.22	.26	.31	.34	.30	.16	.21	.35	.27

Source: The data are from the 1972 and 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

\*Entry is the correlation between ideological location on the seven-point continuum and policy preferences on seven-point issue scales. "No Opinion" and "Don't Know" were treated as missing data. The issues in this table are all of the policy questions presented both in 1972 and in 1976 in the seven-point scale format with a clear policy alternative specified at each end of the scale.



tification) are much smaller than the comparable differences between the relative proximities to the parties' and candidates' ideological positions. That is, liberals and conservatives are less apt to differentiate the particular issue positions of other liberals and conservatives (and thereby establish their own proximity to the liberal, or conservative, alternatives on issues) than they are to differentiate the ideological locations of parties and candidates. It would appear that the parties and the candidates are more likely to be drawn into structures of perceived ideological relationships and linkages than are ideologically defined policy alternatives. If policy questions are less associated with ideological sentiments than are parties and candidates, as these data suggest, then policy positions alone may be of relatively limited importance in developing and maintaining an ideological stance. This conclusion is, of course, contrary to conclusions drawn from both empirical studies and conventional wisdom about the basis and content of mass political ideology where there is the pervasive assumption that ideology is based entirely on issue preferences.<sup>21</sup>

<sup>21</sup>It may be that these surprising results reflect a selection of policy questions with little ideological meaning. However, this does not seem likely, given the

The second conclusion to be drawn from Table 9 is that party identification may have a direct influence on perceptions of the ideological meaning of policy alternatives. The proximity measures in Table 9 link respondents' own issue positions to the positions they attribute to liberals and conservatives. But these proximity scores vary directly with party identification. Specifically, Republican liberals are more likely to see conservatives as closer to their own issue positions than are Democratic liberals, and Republican conservatives are more likely than Democratic conservatives to see conservatives as closer to their own issue positions. It may be that one of the meanings of an ideological position—independent of what it may mean for policy preferences—is what it may mean for partisanship. Ideology and partisanship are tightly linked. It is as though party identification denotes responses to political parties and connotes ideological responses as well. This ideological aspect of party loyalty is so strong that, for example, Democrats (even if

correlations of ideological location with issue position described in Table 8. It seems more appropriate to suggest that the substantive meanings, the connotations, and the implications of ideological locations are diverse and include far more than a summary measure of policy preferences.

Table 9. Perceptions of Relative Proximity to Liberals and Conservatives on Four Selected Issues, within the Joint Distribution of Party Identification and Ideological Location\*

Ideological Location	Party Identification		
	Democratic	Independent	Republican
Jobs and Standard of Living, 1976			
Liberal	+35	+ 5	- 6
Centrist	- 2	- 6	-17
Conservative	-27	-24	-58
Rights of the Accused, 1976			
Liberal	+35	+18	- 2
Centrist	-13	+ 1	- 9
Conservative	-23	-31	-39
Campus Unrest, 1972			
Liberal	+41	+16	-14
Centrist	-17	-27	-41
Conservative	-47	-45	-66
Inflation, 1972			
Liberal	+38	+20	+14
Centrist	+ 4	- 4	- 3
Conservative	-22	- 8	-27

Source: The data are from the 1972 and 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

\*A (+) entry is the preponderance of those who see themselves as closer to liberals than to conservatives; (-) indicates a preponderance of those who see themselves as closer to the conservative position.

they are conservative Democrats) more often than Republicans describe their issue preferences as closer to the issue positions of liberals than of conservatives. And if liberals with an affinity for other liberals are Republicans rather than Democrats, they are apt to describe their issue positions as relatively closer to conservative than to liberal positions. In sum, when people describe themselves as having an ideological position, they also seem to be saying something about their positions on the parties (and presidential candidates) quite apart from their issue or policy stands. This provides further evidence for questioning the conclusion that an ideological position is entirely, or perhaps even primarily, a summary measure of positions on public policy.<sup>22</sup>

Table 10, which indicates a need for a more complex and inclusive interpretation of ideo-

logical location, provides the most direct test of the extent to which policy positions are—or are not—seen as measuring or implementing ideological sentiments. This table reveals the extent to which an ideological stance is associated with describing federal socioeconomic policy alternatives in ideological terms. If issue voting is a direct translation of ideological concern, then the most ideological voter should be able to link policy positions and ideological positions clearly and coherently. Moreover, both liberals and conservatives ought to agree as to which of the policy alternatives on a particular issue is liberal and which is conservative. Table 10 is based on only those citizens with some college education who, by virtue of that education, are presumably most skilled in linking abstract ideology to a specific policy question. The table shows that many of those citizens were unable to assign ideological labels to policy alternatives or make correct assignments, even though the question of the role of the government in guaranteeing jobs and a good standard of living has been an enduring source of partisan ideological dispute over the years.<sup>23</sup>

There was, however, a high degree of agreement about the policy-ideology linkage among

<sup>22</sup>The evidence based on the contrast between intra-column differences of Table 9 and Table 5 may be less conclusive than that provided by the intra-row differences among partisans in Table 9 alone. This is true because, as we will note shortly, even the most ideological are apparently often unclear about what constitutes a liberal position (or a conservative position) on an issue, and it is possible, although we have not explored the possibility, that both liberals and conservatives, holding appropriate policy preferences themselves, misperceive positions of other liberals and conservatives often enough to reduce the frequency with which they see their own group as the more proximal. This would limit the intra-column differences on issues without necessarily demonstrating anything about the relative prominence of parties, candidates and issues in one's ideologically structured belief system.

<sup>23</sup>The question was: "Some people feel that the government in Washington should see to it that every person has a job and a good standard of living. Suppose that these people are at one end of this scale—at point number 1. Others think the government should just let each person get ahead on his [sic] own. Suppose that these people are at the other end—at point number 7. And, of course, some other people have opinions somewhere in between. . . ."

Table 10. The Perceived Association of Liberals and Conservatives with Positions on the Question of Governmental Guarantees of Jobs and a High Standard of Living, among Persons with at Least Some College Education

	Liberal is Pro Governmental Guarantee; Conservative is Pro Self-Help	Liberal is Pro Self-Help; Conservative is Pro Government	No Difference Between Liberal and Conservative	Don't Know Liberal or Conservative Position	Total
Ideological Liberal	78%	11	2	9	100%
Liberal	50%	22	12	16	100%
Liberal Preference	51%	16	11	22	100%
Centrist	30%	19	15	36	100%
Conservative Preference	50%	15	16	19	100%
Conservative	56%	13	6	25	100%
Ideological Conservative	81%	4	1	14	100%

Source: Data are from the 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

the most ideological of these citizens. About four out of five of the most conservative and of the most liberal agreed that the liberal position was that of "supporting a governmental guarantee of full employment and a high standard of living," while the conservative position favored "allowing each person to get ahead on his [sic] own." The less extreme ideologues agreed far less often among themselves about the ideological labels for policy alternatives. The number of respondents who actually reversed the ideological positions, or said they did not know the liberal or conservative position, is particularly interesting; about a fifth of the most ideological conservatives and liberals gave one of these responses. Partisan disagreements, often couched in ideological terms, have, over the last four decades, centered on the government's role in guaranteeing full employment and insuring a high standard of living. Yet, in 1976, the highest levels of ideological commitment were required before substantial proportions of citizens with some college education were able to specify the liberal-conservative alternatives on this question of economic and social policy. And, even among the most ideological, about a fifth did not make the correct linkage.

Three other policy questions are treated more summarily in Table 11. The 1972 perspectives on the proper treatment of unrest on the nation's college campuses were those most clearly seen in ideological terms. And it was the college-educated among the liberals and conservatives who were most apt to make the correct link between policy alternatives and ideological position. Even the policy alternatives on the question of the rights of the accused, an issue central to much of the ideological debate over the law and order in the period from the middle sixties to early seventies, were in 1976 only moderately well understood in ideological terms. Again, it is the college-educated who were most apt to associate the policy alternatives posed by this issue with the correct ideological position.

#### Elections as Policy Mandates

Ideological location is an important factor in shaping voters' choices on election day. In both 1972 and 1976 ideological location made a significant contribution to the vote outcome. And that contribution was independent of party identification. In both years ideological location was also associated with differences in perceptions of the ideological positions of the parties and their candidates. But it does not necessarily follow that clear policy mandates

can be inferred from such "ideologically" based voting. To the contrary, we have shown that there are no simple relationships among ideological location, perceptions of the ideological meaning of issues and the substance of specific policy preferences.

It is true that a substantial minority of that electorate, perhaps 15–20 percent, hold views suggesting that their policy preferences and their perceptions of party and candidate issue positions are based on coherent ideology. At the other extreme, an even larger minority, at least 30–35 percent of the population, show no interest in organizing their preferences and perceptions of the political world in ideological terms. The balance, 45–55 percent, seem to have political perspectives which, however they may serve those individuals, do not permit aggregation into more general, ideological perspectives with common meanings. These individuals describe themselves with ideological labels, often hold attitudes similar to those of "real" ideologues, and may vote similarly; but unlike "real" ideologues they do not have coherent, well-articulated views of the political world. Their presence in the electorate suggests that even an electoral victory heavily influenced by the ideological self-labeling of the voters may defy interpretation in terms of a policy mandate. Conversely, issue voting may be most difficult to summarize with an ideological terminology of uniform relevance even to those who share policy preferences.

Apparently, the translation of policy preferences into ideologically based partisan electoral choices is subject to considerable slippage, primarily because most voters are unable to distinguish the liberal from the conservative positions on many issues. Apparently only the best-educated among the most ideological of voters understand the ideological implications of their policy preferences clearly enough to link themselves ideologically to policy preferences and to party or candidate as well as, ultimately, to the vote decision. Thus, one frequent break in this chain occurs when voters attempt to associate policy alternatives with liberal or conservative issue positions. The meaning of the strong relationship between ideological location and the vote is, therefore, open to many interpretations; however, it is clear that issue voting and ideological voting are not coterminous, and that an ideological position is not simply a summary statement of many issue positions. Our demonstration of the extent to which ideological sentiments do not translate directly into policy preferences is entirely congruent with the research of Converse, Klingemann, Stimson and others who

Table 11. The Perceived Association of Liberals and Conservatives with Particular Positions on Issues, within Educational Levels

Ideological Location	Campus Unrest, 1972 <sup>a</sup>			Rights of the Accused, 1976 <sup>b</sup>		
	College	High School	Grade School	College	High School	Grade School
Ideological Liberal	96	89	— <sup>e</sup>	78	68	—
Liberal	89	49	41	53	37	28
Liberal Preference	88	44	—	51	49	—
Centrist	64	41	35	40	24	13
Conservative Preference	83	52	—	42	32	—
Conservative	78	67	47	62	35	27
Ideological Conservative	88	79	43	70	61	37

Ideological Location	Jobs and Standard of Living, 1976 <sup>c</sup>			Inflation, 1972 <sup>d</sup>		
	College	High School	Grade School	College	High School	Grade School
Ideological Liberal	78	22	—	63	58	—
Liberal	50	35	30	55	36	28
Liberal Preference	51	47	—	31	36	—
Centrist	30	28	26	30	11	5
Conservative Preference	50	30	—	26	24	—
Conservative	56	42	23	22	22	18
Ideological Conservative	81	67	54	35	12	9

Source: The data are from the 1972 and 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

<sup>a</sup>Entry is proportion who associate liberalism with support for protesters, maintaining law and order with conservatism.

<sup>b</sup>Entry is proportion who associate liberalism with concern for the civil liberties of the accused, reduction of crime with conservatism.

<sup>c</sup>Entry is proportion who associate liberalism with active role for government, individual self-help with conservatism.

<sup>d</sup>Entry is proportion who associate liberalism with government action against inflation, no immediate government action with conservatism.

<sup>e</sup>Too few cases for reliable estimation of entry.

have documented the lack of ideological thinking in mass electorates in the United States and Europe. The explanation of the various linkages and breaks in the linkages among policy preferences, vote choices, perceptions of candidates and partisanship for voters with various ideological sentiments provides a research agenda for the future.

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# Gender Roles and Party Roles\*

DIANE L. FOWLKES  
JERRY PERKINS  
*Georgia State University*

SUE TOLLESON RINEHART  
*Rutgers University*

*This article uses discriminant analysis to assess sex and party differences across four aspects of party organization: incentives for participation, party role definitions, party activities and electoral ambition. The results illustrate that gender roles operate most distinctly in the two areas of electoral ambition and party activities. Party context is more important than sex in terms of the other two areas, party role definitions and incentives for participation. The authors conclude that party activists' gender-related behavior can be better understood by discovering how the context of party organization either modifies or reinforces such behavior.*

Though an increasing number of women are active in party politics, their emergence into the ranks of the politically active does not appear to be totally unrestricted by traditional gender role expectations.<sup>1</sup> Even where there is a conscious attempt to promote equality between the sexes, women still tend to perform routine tasks while men tend to dominate the functions surrounding organizational goals and political candidacy (e.g., Maisel, 1975, pp. 202-08). This research examines gender roles and party roles in one southern party setting. The analysis differs from previous work in two respects. First, the usual bivariate correlations of sex with other variables are replaced by multivariate analysis in an attempt to map the overall pattern of hypothesized gender role differences. Second, the study moves away from a purely sociological perspective to a consideration of the effects of Republican or Democratic organizational context and asks, in effect, "What are the motivations, roles, activities and career

aspirations that differentiate, respectively, Democratic and Republican men and women activists?"

The research perspective draws upon explanations of gender differences offered in previous studies of party activists (Jennings and Thomas, 1968; Costantini and Craik, 1972). Employing the Parsons and Bales theory of *expressive* and *instrumental* functions, Costantini and Craik (1972, p. 235) posit a parallel between the family and party organization: "The male party leader, like the husband, is more likely to specialize in the instrumental functions . . . those functions related to the external world. The female party leader, like the wife, tends to specialize in expressive functions . . . those concerned with . . . internal affairs." To the extent that party activists receive and follow the gender role socialization traditionally prescribed for each sex, women would be expected to be more expressive and men more instrumental across four aspects of party organization: the pursuit of electoral careers, incentives for participation, party role definitions and performance of party duties.

The instrumental nature of political ambition is quite straightforward. As in the family, presumably some party members will spend a life of support within the organization (expressive roles) while others will venture forth in pursuit of career goals outside the basic unit (instrumental roles). The organizational "family" promotes the extra-organizational political careers of its ambitious members, who are expected to be men.

In a similar vein, the standard three-part division of incentives for participation (Clark and Wilson, 1961) can be divided into *instrumental/expressive* categories. Material motives

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<sup>1</sup>Existing research focusing directly on women in party organization is limited. The work most relevant to this undertaking includes Jennings and Thomas (1968), Costantini and Craik (1972) and Lee (1976). A larger body of literature describing the incidence of women in party and elective leadership roles and as convention delegates includes the work of Johnson and Stanwick (1976), Huckshorn (1976, pp. 25-26), Lynn and Flora (1977), Kirkpatrick (1974), Kirkpatrick (1976), Gruberg (1968) and Jaquette (1974).

involving the pursuit of individual monetary and career rewards would be instrumental, while solitary or social considerations would be expressive. Purposive incentives involving attempts to affect public policy by using the organization to alter the direction of government could be considered instrumental.

Role definitions and activities in party studies typically fall into an *organizational/electoral* dichotomy.<sup>2</sup> This dichotomy closely parallels the expressive/instrumental breakdown, with electoral activities in effect representing instrumental efforts. A more fundamental dimension, however, may cut across organizational and electoral categories. Both organizing and electoral work involve perfunctory tasks as well as nonroutine activities. The rather routine task of attending meetings is to be contrasted with the more innovative work involved in recruiting new members and expanding the organization. Electorally, tasks such as putting out the mail must be contrasted with recruiting candidates and raising money. In this study, *routine* organizational or electoral roles and activities are considered expressive, while *nonroutine* organizational or electoral roles and activities are considered instrumental.

Because this study is concerned with building a political taxonomy of the sexes, we must also consider a second category, party membership. Much evidence illustrates the differences between the parties in recruiting and other activities, which depend upon majority-minority status, the social setting and the like (Crotty, 1967; Pomper, 1965; Patterson, 1963; Bowman and Boynton, 1966; Conway and Feigert, 1974; Ippolito, 1969; Althoff and Patterson, 1966; Beck, 1974). Although some of this evidence is contradictory and some difficult to compare, the important point from our perspective is that gender roles do not operate in a political vacuum. It is to be expected, for example, that one party's emphasis on organizational as opposed to electoral goals or varying emphasis between the parties on the cultivation of political careers could create more party than gender differences.

<sup>2</sup>Other roles and activity dimensions can be isolated, of course. Ideological or opinion leadership, for example, is oftentimes separated from other activities, but such activity does not constitute a large proportion of the party worker's time, particularly at the precinct level (Conway and Feigert, 1974; Eldersveld, 1964).

## Methodology

**The Data.** The data for this study come from a survey of Democratic and Republican political party officials in Fulton and DeKalb Counties, Georgia, conducted from January to late April of 1977. The counties contain approximately two-thirds of metropolitan Atlanta's 1.5 million population. The Atlanta area supports viable Republican organizations that have emerged as successful competitors with the Democratic organizations, especially at the local level, although the Democrats remain the dominant party.

The study focused on the people at the base of the four organizations because of the comparability of positions across parties and because the greatest proportions of women are found at the lowest levels. While there are some differences in the organizational groupings of workers, all four organizations attempt to secure precinct leaders. Many precinct leaders also occupy higher positions, especially in the Democratic party, but most are active only at the lowest level. Using official party lists, we sent letters to all precinct people. Trained interviewers then attempted to contact and interview all potential respondents. The standard survey schedule took an average of one hour to complete. Of 419 potential respondents, 286 were reached and interviewed—a success rate of 68 percent.<sup>3</sup> Of this group 27 respondents provided incomplete information and were consequently eliminated from the data set. The respondents analyzed here include 78 Democratic men, 56 Democratic women, 66 Republican men and 59 Republican women. The questionnaire included items appropriate to the four major areas of concern. Respondents were provided with a series of reasons for their continuing involvement in party politics and were asked to rate the importance of each.<sup>4</sup> We asked open-ended questions to tap

<sup>3</sup>Thirteen percent of the party officials refused to allow an interview. The remaining 19 percent could not be reached and/or interviewed because of change of address, inaccuracies in party lists or illness. However, those completing the interviews closely resemble the known characteristics of the larger population of Atlanta party activists, although blacks, men, Republicans and Fulton County officials are slightly under-represented.

<sup>4</sup>Respondents were asked to rate the importance of the following on a one to five (low to high) scale: job experience, opportunity to run for elective office, to affect government policy, and to make business and professional contacts, loyalty to the party, the excite-

role perceptions and administered a series of specific items on actual activities performed.<sup>5</sup> As for political ambition, each respondent was asked if s/he were interested in running for office if the opportunity arose.

**Data Analysis.** We used discriminant function analysis to distinguish local party activists on the basis of their sex and party.<sup>6</sup> The maximum number of functions that can be derived is equal to one less than the number of groups. In the present study, discriminant function analysis can distinguish the four groups—Democratic men, Democratic women, Republican men,

ment of politics, the enjoyment of working with others. The coding scheme for this article was ordered to be consistent with the expressive/instrumental dimension, with the latter scored high. The last three incentives—loyalty, excitement and enjoyment—were reverse-coded to provide consistency across all items.

<sup>5</sup>Up to four responses were coded on the role perceptions. The responses were classified as organizational or electoral, summed and given value consistent with the expressive/instrumental dichotomy. The open-ended responses also were divided in terms of a routine/innovative breakdown such that routine organizational responses (e.g., attendance at meetings) were differentiated from innovative roles (e.g., recruitment of new members); routine electoral roles (e.g., telephoning for candidates) were distinguished from innovative electoral roles (e.g., candidate recruitment). In both routine/innovative measures, the former was subtracted from the latter. As for activities, several objective questions were employed. The instrumental activities asked about were: raising money, talking to public officials, number of recruitment conversations, having run for office in the past. The expressive activities were: incidence of telephoning experience, transporting of voters, attendance at party meetings, routine between-election work within the party. The expressive activities were reverse coded.

<sup>6</sup>The analytical potential of discriminant function analysis is threefold. It is a more appropriate technique than is multiple regression when the criterion variables in question are dichotomous (Kort, 1973) as party and sex in the present study. It can illustrate the relative contribution of the discriminating variables (Cooley and Lohnes, 1962, pp. 116–33). It may be used to classify groups, whether or not a priori probabilities of membership are known (Hoel, 1954, pp. 179–84; Anderson, 1958, pp. 133–52; Cooley and Lohnes, 1962). Discriminant analysis assumes a multivariate normal distribution and equal variance-covariance across all groups. However, Cooley and Lohnes (1971, p. 228) contend that the issue of homogeneity of group dispersions might be ignored as the technique is “fairly robust.” The covariance matrices of the four groups in this study were nonetheless examined, and the degree of equality was judged to be within acceptable limits.

Republican women—by up to three linear functions best able to capitalize on the differences among the groups. Since the formal logic of the discriminant function model is not disturbed by treating independent criterion variables as “dependent” and thus to be “predicted” by what normally would be considered dependent variables (Cooley and Lohnes, 1971), the method affords an opportunity to assess the magnitude of the differences between the characteristics of the criterion groups. Thus, the investigation seeks to determine patterns of group differences by treating two dichotomous variables, sex and party affiliation, as “dependent” and employing the party organization variables described earlier to separate the group of political party activists into their correct sex and party categories.<sup>7</sup> Since membership in the four groups is known a priori, the ability of the discriminant function(s) to categorize Democratic and Republican men and women will serve to evaluate the impact of gender role and party on political behavior in party organizations.

### Findings

The findings support the expectation that gender roles are important in the life of the party organization but that the form of gender role relationships may be modified by the party context. The discriminant function analysis produces two functions. The first function is dominated by the enjoyment of politics as an incentive, but also contributing are the incentives of the opportunity to make business contacts and the chance to run for office, and the routine-nonroutine nature of organizational role definitions (see Table 1).<sup>8</sup> The second function is dominated by electoral ambition, but also contributing are the activities of attending party meetings, telephoning and canvassing, and the routine-nonroutine nature of electoral role definitions. The incentive of desiring to make business contacts also appears on the second function (see Table 2). The group means in Table 1 show that the mean

<sup>7</sup>Rao's  $V$  was employed as the criterion for including variables in the discriminant function (Klecka, 1975).

<sup>8</sup>In each function, some party variables make larger contributions than others to the function's discriminating ability, and the party variables are arranged according to the absolute magnitude of their standardized discriminant function coefficients. Those coefficients greater than  $|.25|$  are the basis for the characterization of each function.



Table 1. Distinguishing Characteristics of the Parties: Incentives and Roles

Discriminating Party Variables	Standardized Discriminant Function Coefficients Function I Incentives and Roles
Incentive: Enjoying politics	.53956
Role: Defining role as routine-nonroutine organizational	.37611
Incentive: Desiring to make business contacts	-.37103
Incentive: Having the chance to run for office	-.27391
Ambition: Expressing aspirations for office	.24094
Activity: Telephoning	.23740
Incentive: Expressing party loyalty	.22014
Activity: Transporting voters	.21233
Role: Defining role as organizational	.21179
Role: Defining role as electoral	.20335
Activity: Registering voters	.19938
Activity: Talking to public officials	-.18364
Activity: Taking initiative in recruiting candidates	.18106
Incentive: Finding politics exciting	-.17252
Activity: Canvassing	-.10986
Role: Defining role as routine-nonroutine electoral	.10364
Activity: Collecting money	.08646
Activity: Attending party meetings	.00811
Group	Means
Democratic Women	-.879
Democratic Men	-.181
Republican Women	.244
Republican Men	.741

Source: Computations from survey data in the Fowlkes-Perkins Study of Atlanta Area Party Members, January-April, 1977.

discriminant function scores for Democrats (both men and women) are negative and analogous scores for Republicans are positive, thus suggesting that the first function distinguishes between political parties. The group means in Table 2 show that the mean discriminant function scores for women (both Democratic and Republican) are negative and the scores for men of both parties are positive, thus suggesting that the second function distinguishes between the sexes.

The predictive power of the two discriminant functions taken together is demonstrated in Table 3. On the basis of random assignment to four groups, one should be able to predict correct group membership 25 percent of the time. The use of the discriminant functions raises predictive ability to 57 percent, on the average. As Table 3 shows, the membership group of Democratic men is most easily predicted, and the least easily predicted membership group is Republican men.

The relative importance of the two functions can be determined by their ability to explain variance in all the discriminating variables and

by their relationship to the four groups being distinguished. The first function, distinguishing between the parties, with an eigenvalue<sup>9</sup> of .496, explains 52.6 percent of the variance existing in the discriminating variables. The relationship between this function and the four groups is represented by a canonical correlation of .576. The second function, distinguishing between the sexes, with an eigenvalue of .401, explains 42.5 percent of the variance. Its relationship to the four groups is represented by a canonical correlation of .535. Together these two functions explain about 95 percent of the variance in the discriminating party variables.<sup>10</sup>

The findings suggest that party activists can

<sup>9</sup>The eigenvalue of a discriminant function represents the part of the variance of all the discriminating variables explained by that function.

<sup>10</sup>A third function, explaining 5 percent of the variance, was considered insignificant and is not presented or discussed.

Table 2. Distinguishing Characteristics of the Sexes: Ambition and Activities

Discriminating Party Variables	Standardized Discriminant Function Coefficients Function II Ambition and Activities
Ambition: Expressing aspirations for office	.55978
Activity: Attending party meetings	.36430
Activity: Telephoning	.35627
Incentive: Desiring to make business contacts	.29833
Activity: Canvassing	.27326
Role: Defining role as routine-nonroutine electoral	.27180
Role: Defining role as routine-nonroutine organizational	-.23715
Activity: Collecting money	.21908
Incentive: Finding politics exciting	.19887
Incentive: Enjoying politics	.15717
Role: Defining role as organizational	-.14668
Incentive: Expressing party loyalty	-.09247
Activity: Registering voters	.08414
Activity: Taking initiative in recruiting candidates	.07987
Role: Defining role as electoral	-.06569
Incentive: Having the chance to run for office	.04323
Activity: Talking to public officials	-.03305
Activity: Transporting voters	-.00301
Group	Means
Republican Women	-.778
Democratic Women	-.262
Republican Men	.152
Democratic Men	.648

Source: Computations from survey data in the Fowlkes-Perkins Study of Atlanta Area Party Members, January-April, 1977.

be distinguished by party and by sex. But which party members exhibit the more instrumental or expressive incentives and role definitions? Which sex is more ambitious and more responsible for the instrumental or expressive activities? To answer these questions and to relate the findings to the theoretical perspective of instrumental and expressive gender and party roles, we must consider the instrumental/expressive nature of the scoring for party variables and their positive or negative contribution to each set of individual discriminant function

scores. The instrumental/expressive scoring of the party variables is illustrated in Table 4, which displays the mean scores on each discriminating variable for men and women of the two parties. The scoring procedure is such that instrumental values are always scored "high" and expressive values "low." Thus, for example, those attending a high number of organizational meetings actually are scored lower than those attending few or no meetings.

The contribution of each party variable to each individual's discriminant function score is

Table 3. Proportions of Correct Classifications by Two Discriminant Functions

Actual Group	N	Percent Correctly Predicted (N)
All Respondents	259	57.1 (148)
Democratic Men	78	65.4 ( 51)
Democratic Women	56	51.8 ( 29)
Republican Men	66	45.5 ( 30)
Republican Women	59	55.9 ( 33)

Source: Computations from survey data in the Fowlkes-Perkins Study of Atlanta Area Party Members, January-April, 1977.

Table 4. Discriminating Variables<sup>a</sup> with Mean Scores for Each Group

Party Organization Variables	Democrats		Republicans	
	Men	Women	Men	Women
<b>Incentives</b>				
Having chance to run for office	2.44	2.07	1.76	1.39
Desiring to make business contacts	2.67	2.63	1.94	1.49
Expressing party loyalty*	2.31	1.88	3.02	2.27
Finding politics exciting*	2.68	2.09	3.26	2.64
Enjoying working with others in politics*	2.22	1.43	2.92	2.12
<b>Role Definitions</b>				
Organizational*	2.44	2.27	2.86	2.63
Electoral	.87	.63	1.02	1.07
Routine-nonroutine organizational	.33	.32	.42	.61
Routine-nonroutine electoral	.29	.09	.23	.19
<b>Activities</b>				
Telephoning*	.23	.07	.30	.07
Canvassing	.78	.73	.60	.58
Transporting voters*	.63	.64	.85	.69
Collecting money	.56	.46	.55	.53
Registering voters	.44	.30	.24	.32
Taking initiative in recruiting candidates	2.69	2.46	2.70	2.56
Attending party meetings*	74.59	63.79	73.44	63.42
Talking to public officials	2.31	2.36	2.02	2.20
<b>Electoral Ambition</b>	2.17	1.75	1.85	1.42

Source: Computations from survey data in the Fowlkes-Perkins Study of Atlanta Area Party Members, January-April, 1977.

<sup>a</sup>Other variables entered in the discriminant analysis but not appearing in the final list of predictor variables were: Incentives: job experience, opportunity to be influential in the community; Activities: contacting other party officials, recruiting, talking to public officials on behalf of others, having run for public office in the past.

\*These variables indicate expressive functions and were reverse-coded so that higher values indicate instrumentality.

determined by multiplying the party variable's function coefficient by the individual's standardized ( $z$ ) score on the party variable. The products for all party variable function coefficients and related individual  $z$  scores are summed to provide each individual with a discriminant function score. Because all party variables are scored so that instrumental values are higher than expressive values, instrumental raw scores result in positive  $z$  scores and expressive raw scores result in negative  $z$  scores. The positive or negative result of the calculation of each major part of an individual's discriminant score on Function I is displayed in Table 5, and on Function II in Table 6.

Table 5, based on those variables characterizing Function I, demonstrates that it is the Democrats who score on the expressive ends of the incentive of enjoyment of politics and routine-nonroutine organizational roles but on the instrumental ends of the incentives of business contacts and future office-seeking. The Republicans are the reverse image of the Democrats on these party variables. Looking at the

mean scores for these party variables in Table 4, we can verify that the Democrats tend to have instrumental scores on the incentives of the chance to run for office and the chance to make business contacts. Democrats have expressive scores reflecting enjoyment of politics and expressive, that is, routine scores on organizational roles. Thus, the Democratic activists are more likely to emphasize material and solidary incentives than are the Republicans, while the Republicans adopt more nonroutine organizational roles than do the Democrats.

Table 6, based on those variables characterizing Function II, demonstrates that men score consistently on the instrumental ends of the party variables. Women score consistently on the expressive ends of the party variables. A glance at the mean scores in Table 4 verifies that women exhibit less electoral ambition than men, attend more meetings, do more telephoning and less canvassing. Women also are less motivated to work for the party because of the opportunity to make business contacts and are more likely to define their electoral roles as

Table 5. Contributions of Major Party Variables in Function I to Individual Discriminant Scores on Function I

Most Discriminating Party Variables	Sign of Discriminant Function Coefficient	Contribution to Individual's Discriminant Score	
		With Instrumental z Score (+)	With Expressive z Score (-)
Incentive: Enjoying politics	+	+	-
Role: Defining role as routine-nonroutine organizational	+	+	-
Incentive: Desiring to make business contacts	-	-	+
Incentive: Having chance to run for office	-	-	+

Source: Summarized from computations of survey data in the Fowlkes-Perkins Study of Atlanta Area Party Members, January-April, 1977.

routine.

Tables 5 and 6 show that the expressive/instrumental concepts consistently distinguish between the sexes but not between the parties. The Democrats and the Republicans, when considered simply as party members, exhibit a combination of expressive and instrumental incentives and party role definitions.

The discriminant functions, of course, do not derive perfect divisions between the sexes on either function. Thus, on Function I, which divides the parties, Democratic women and Republican men are at the extremes with the means of Democratic men and Republican women falling to the inside of the extreme positions. On Function II, which divides the sexes, the extreme group scores are those of the Republican women and the Democratic men, while Democratic women and Republican men occupy the center portion of the array.

The relative positions of men and women on the two functions allow an appraisal of the findings of Jennings and Thomas (1968, pp. 483-84) that differences between the sexes are of less magnitude in the Democratic party than

in the Republican party. The data employed in this study produce mixed findings. On the one hand, differences between the sexes are similar within both parties for discriminant Function II. The group centroids in Table 2, when compared between sexes within each party, show that on the average these differences are nearly the same within each party (a difference of .91 for Democrats and of .93 for Republicans). At the same time, Democratic women are more instrumental than Republican women, as are Democratic men in comparison to Republican men (differences of .52 for the women and .50 for the men). In other words, the level of instrumentality for both sexes is higher in the Democratic party, but the magnitude of differences between the sexes is similar in both parties. This pattern is especially marked for electoral ambition, the variable contributing most to distinguishing between the sexes (note the group mean scores for electoral ambition in Table 4). On the other hand, differences between the sexes are not similar within each party for discriminant Function I distinguishing the parties. The group means in Table 1,

Table 6. Contributions of Major Party Variables in Function II to Individual Discriminant Scores on Function II

Most Discriminating Party Variables	Sign of Discriminant Function Coefficient	Contribution to Individual's Discriminant Score	
		With Instrumental z Score (+)	With Expressive z Score (-)
Ambition: Expressing aspirations for office	+	+	-
Activity: Attending party meetings	+	+	-
Activity: Telephoning	+	+	-
Incentive: Desiring to make business contacts	+	+	-
Activity: Canvassing	+	+	-
Role: Defining role as routine-nonroutine electoral	+	+	-

Source: Summarized from computations of survey data in the Fowlkes-Perkins Study of Atlanta Area Party Members, January-April, 1977.

when compared within each party, show that on the average, differences between the sexes concerning this function are somewhat greater for the Democrats (a difference of .70 between Democratic men and women) than for the Republicans (a difference of .50 between Republican men and women). Democratic women have the negative group means most removed from all the other groups largely because of their expressive position on the enjoyment of politics, the variable most responsible for distinguishing between the parties (see the group mean scores for enjoyment of politics in Table 4).

### Discussion

As in other organizations, many aspects of life within the political party are gender-related in that men and women are differently employed. This study specifies and measures four aspects of party organization: party role definitions, party duties or activities, incentives for participation and electoral ambition. The evidence presented here suggests that in these party organizations gender distinctions are particularly evident in two areas: ambition and activities. Activities such as attending meetings and telephoning can be understood as fulfilling the expressive support function and more commonly as "women's work." More importantly, the party's major goal—that of recruiting candidates and capturing office—is also gender-related. According to our research, men are more ambitious than women, an expressive/instrumental differential that comports well with recent evidence that the under-representation of women in elective office is more the result of a paucity of women candidates than discrimination against them at the polls (Karnig and Walter, 1976; Darcy and Schramm, 1977).

What are the sources of sex-typed behavior? What are the implications for the party organization of change in gender roles? Possible sources of gender discrimination are early socialization and adherence to stereotypes because of fear of sanctions against deviant behavior (Lynn and Flora, 1977). And, of course, discrimination on the part of the party elite also is a possible source. However, as change occurs in society and challenges are carried out by women in party organization (Saloma and Sontag, 1973, pp. 108–10), one would expect that fewer women would be available for the routine functions they have performed in the past. Women may well be accommodated by being given a share of the more important positions, but will men begin

to share in carrying out the very important expressive functions at the base of the party? As women adopt less traditional gender roles and become less responsible for performing their traditional party roles, and if men cannot be encouraged to do the same, the party organization, which is already weak because of other reasons, may become weaker.

The whole organizational life of the party, however, cannot be understood in terms of gender distinctions. The parties face common requirements in different ways, and the manner in which they meet common needs can be more a function of interparty variation than of gender differences. In two of the areas investigated—party role definitions and incentives for participation—party rather than sex is the more important variable. With respect to party role perceptions, Republican men and women see their organizational work as innovative, while Democratic women and men see their organizational roles as routine. And, while it is true that within both parties women are somewhat more motivated than men to work because they enjoy politics, the larger distinction is between the parties; both Democratic women and men are more expressive than their Republican counterparts. Similarly, women have less motivation to work for material rewards than men, but the important distinction is between the parties, with the Democrats the more instrumental of the two groups of activists. Even on those matters that are primarily gender-related such as political ambition, the Democratic party contains higher levels of ambition than does the Republican party.

The differences between the parties in the combination of instrumental and expressive incentives and party role definitions perhaps suggest something about the party system under investigation. The Democratic party is not only dominant in terms of current election returns but is also the long-standing party of the South. Those seeking material opportunity and the pleasure of politics may well find them in larger supply in the established party. Contrary to what some might expect of the minority party (Jewell and Olson, 1978, p. 74), the Republicans talk less about organizational roles and more about electoral roles than the Democrats. However, the organizational roles reported by Republicans are more innovative than those reported by Democrats, suggesting that the newer party necessarily must pursue organizational development while the established party is more concerned with routine maintenance functions.

The effects of party context may be unique to this particular setting, but the results of our

study do encourage a closer and more general examination of the political structures in which men and women operate. The actual mechanisms by which party context affects organizational orientations and behavior are yet to be determined. The parties may attract people of different backgrounds and expectations, or the organizational setting and its political environment may mold and shape party recruits. It is highly probable that both processes are at work. The evidence at hand encourages a research concentration on the comparative entry characteristics of women and men who become involved in party politics and how these characteristics are modified or reinforced by organizational context.

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# An Alternative Conceptualization of Political Tolerance: Illusory Increases 1950s-1970s\*

JOHN L. SULLIVAN  
*University of Minnesota*

JAMES PIERESON  
*University of Pennsylvania*

GEORGE E. MARCUS  
*Williams College*

*This article proposes an alternative conceptualization of political tolerance, a new measurement strategy consistent with that conceptualization, and some new findings based upon this measurement strategy. Briefly put, we argue that tolerance presumes a political objection to a group or to an idea, and if such an objection does not arise, neither does the problem of tolerance. Working from this understanding, we argue that previous efforts to measure tolerance have failed because they have asked respondents about groups preselected by the investigators. Those groups selected as points of reference in measuring tolerance have generally been of a leftist persuasion. Our measurement strategy allowed respondents themselves to select a political group to which they were strongly opposed. They were then asked a series of questions testing the extent to which they were prepared to extend procedural claims to these self-selected targets. Using this approach, we found little change between the 1950s and the 1970s in levels of tolerance in the United States, a result that contradicts much recent research on the problem.*

Many theorists have argued that although a democratic regime may be divided by fierce conflicts, it can remain stable if citizens remain attached to democratic or constitutional procedures and maintain a willingness to apply such procedures—the right to speak, to publish, to run for office—on an equal basis to all, even to those who challenge its way of life. In this instrumental sense, tolerance is understood as valuable because it helps to maintain a stable democratic regime. In addition, since a tolerant regime is generally thought to be a good regime, tolerance is sometimes understood as a good in itself, as an essential characteristic of the good society.

The earliest empirical studies of tolerance conducted during the 1950s (Stouffer, 1955; Prothro and Grigg, 1960; McClosky, 1964)

found high levels of intolerance and a good deal of unwillingness to extend civil liberties to objectionable groups. Many have therefore taken heart in recent findings which purport to show that levels of tolerance in the American public have increased substantially since these earlier studies were conducted (Davis, 1976; Nunn et al., 1978). It would appear that the political ferment of the 1960s and the declining salience of the cold war and of the communist issue have contributed to a more tolerant climate for political debate and dissent. According to this research, then, much progress has been made in the United States over the past two decades in building a more tolerant political regime.

However, the apparent connection between the social and political trends of the 1960s and 1970s and the changing levels of tolerance reported in these studies may dissolve upon closer inspection. Though domestic communists declined in salience and visibility during this period relative to the 1950s, they were replaced as potential targets of tolerance by other groups challenging the political consensus. These groups, representing all shades of political opinion, were not generally received in a tolerant manner, either by members of the elite or by the public at large. The claim that a changed climate of opinion produced higher levels of tolerance is thus too facile, and it begs a number of questions about the sources of

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tolerance and its meaning in a democratic regime.

These conclusions about changing levels of tolerance in American society derive from a particular tradition of research. This tradition incorporates common assumptions about the meaning of tolerance and about the way the concept should be measured. These assumptions, in turn, contributed heavily to the conclusions that were reached. As will be shown below, these assumptions about the meaning and measurement of tolerance are unwarranted and, hence, the conclusions based upon them are inaccurate or, at least, incomplete.

This article, therefore, will reconsider the problem from the beginning. Since the tradition of empirical research on tolerance is powerful and persuasive, it will be necessary to examine its assumptions in some detail to demonstrate our objections to it. After reformulating the problem on the basis of more tenable assumptions, we will proceed to our own analysis of the level of political tolerance in the United States. This analysis, in turn, will produce conclusions different from those of the studies cited above.

### The Empirical Literature on Tolerance

Despite the importance of the subject, the literature on political tolerance is not particularly extensive. We can point to six important studies spanning two decades: Stouffer's (1955) study of attitudes toward communism; the Prothro-Grigg (1960) study of political tolerance; McClosky's (1964) study of levels of support for democratic norms; Lawrence's (1976) study connecting tolerance with positions on specific issues; Davis' (1975) study testing Stouffer's predictions about the effects of generation, age, and education on tolerance toward communists and atheists; and the Nunn, Crockett, and Williams (1978) more extensive update of Stouffer's work.

These studies are especially worthy of attention because they draw some important conclusions about the way in which democracy in the United States supposedly operates. As their conclusions are based upon empirical findings, close attention should be given to the way the studies are executed and to the evidence upon which these conclusions rest. In particular, they should be examined in light of the following questions: How is tolerance defined? How is it measured? And what assumptions are made concerning the role of tolerance in a democratic polity?

Among the earliest empirical studies of

tolerance in the United States was Stouffer's analysis of public attitudes toward communism, published in 1955. Based upon a large national survey conducted in 1954, the study was designed to measure public attitudes toward communism and the extent to which Americans were prepared to extend procedural rights to communists and suspected communists. Stouffer also examined attitudes toward two other groups on the left, atheists and socialists. Though the study purported to study tolerance of "nonconformity," all but four of the fifteen items used to measure tolerance listed communists or suspected communists as points of reference (Stouffer, 1955, Appendix C). The conclusions of the study, therefore, bear more closely upon tolerance of communists than upon tolerance more broadly understood.

Stouffer's findings were nevertheless important—and disturbing. Substantial majorities said that an admitted communist should not be permitted to speak publicly, or to teach in high schools or colleges, or, indeed, to work as a clerk in a store. Majorities also agreed that communists should have their citizenship revoked, that books written by communists should be taken out of public libraries, that the government should have the authority to tap personal telephone conversations to acquire evidence against communists, and that, withal, admitted communists should be thrown in jail (Stouffer, 1955, Ch. 2). These attitudes softened considerably when the same questions were posed about socialists, atheists, and suspected communists. However, large numbers of citizens responded intolerantly to these targets as well. These results have to be interpreted against the background of the McCarthy period, but they undermined the assumption that there existed a consensus in the society around procedural norms that allow extremist groups access to political institutions.

Stouffer's conclusions about tolerance in the United States, however, were more optimistic than his empirical findings at first glance seemed to warrant. He suggested that tolerant norms in the society would inevitably grow stronger as time passed:

Great social, economic, and technological forces are operating slowly and imperceptibly on the side of spreading tolerance. The rising level of education and the accompanying decline in authoritarian childrearing practices increase independence of thought and respect for others whose ideas are different. The increasing geographical movement of people has a similar consequence, as well as the vicarious experiences supplied by the magic of our even more powerful media of communications (p. 236).



Stouffer concluded, then, that the intolerance of the early 1950s would abate because of increased education and other factors.

Prothro and Grigg (1960) tried to discover whether there existed a consensus on general procedural norms of democracy and minority rights, and whether citizens were prepared to apply these abstract principles to specific situations in which unpopular groups of individuals might be involved. They did find a general consensus on the principles, but this broke down on the specific applications of the norms, particularly when the principles were applied to communists.

In a related study, McClosky (1964) compared political influentials and rank and file citizens in levels of support for abstract principles and for the application of these principles to specific situations. He found members of the elite to be much more sympathetic to statements expressing the "rules of the game"; in addition, they were more likely than the general electorate to support the application of the general principles of free speech and opinion to specific situations. He therefore concluded that "a large proportion of the electorate has failed to grasp certain of the underlying ideas and principles on which the American political system rests."<sup>1</sup>

These studies were carried out and written during the peak of the cold war era when the denial of procedural rights to communists and related groups was the major concern of those interested in civil liberties. In the interim, the dimensions of political conflict have grown more complex, and challenges to the political consensus have come from many sources, including civil rights activists, feminists, opponents of the war in Vietnam, and various radicals and reactionaries. As the potential targets of intolerance have proliferated, it is even less appropriate now to measure tolerance solely with reference to communists and associated groups. At the same time, by broadening the range of political opinion in the society, the ferment of the 1960s may have created a more tolerant environment for dissent. For these reasons, the conclusions of these earlier studies need to be reconsidered.

In a recent article, using National Opinion Research Center data collected in the early 1970s, Lawrence (1976) has reconsidered the problem and has refined the conclusions of these earlier studies. Like Prothro and Grigg, he

is concerned with the relationship between positions on specific issues and the willingness to tolerate the actions of various groups concerned with these issues. Lawrence's findings report a higher level of tolerance in the 1970s than that suggested by earlier studies. A majority of citizens would permit all of the general acts of protest, except that involving the blocking of a government building, and there was considerable consistency between evaluations of the abstract acts and their specific applications. This consistency, however, varied from issue to issue.

The question of whether tolerance has in fact increased since Stouffer's study has been addressed in two separate studies. Davis (1975) attempted to test Stouffer's prediction that tolerance would increase as levels of education in the society increased and as the average age of the population declined. Drawing upon the same NORC sample Lawrence used, Davis found a 22 percent increase in tolerance between 1954 and 1971. He attributed 4 percent of this increase to higher levels of education, 5 percent to cohort replacement, and 13 percent to increasing levels of tolerance among all cohort and educational groups. Thus, the bulk of the change apparently reflected general trends that strengthened tolerant political norms. What these trends were he did not venture to say, though he suggested that they involved a "general movement" in the society toward more liberal positions on all sorts of non-economic issues.

Similarly, Nunn, Crockett, and Williams (1978) have attempted to measure changes in levels of tolerance by analyzing their own national survey conducted in 1973 that repeated the Stouffer items. Like Lawrence and Davis, they found a considerable increase in tolerance between 1954 and the early 1970s. Since their survey contained the same questions Stouffer used, they were able to measure quite precisely the changes in levels of tolerance of communists, atheists, and socialists. This is demonstrated in Table 1, where, using NORC data, we present the responses to six questions about communists and atheists in both 1954 and 1977. It is clear from these data that tolerance for these two groups increased significantly over this 23-year span. The increases range from 25 to 35 percentage points, depending upon the question. Nunn et al., relying upon data from their own survey and from several NORC surveys conducted in the early 1970s, observed a similar change. Thus, they found that while in 1954 only 31 percent of the public could be classified as tolerant on their overall tolerance index, fully 55 percent

<sup>1</sup>Jackman (1972) shows that most of the differences between elite and mass disappear once education is controlled.

Table 1. Increases in Tolerance of Atheists and Communists, 1954–1977 (Percent)

	1977 NORC	1954 Stouffer	Increase
Should an atheist be allowed to speak?*	63	37	26
Should an atheist be allowed to teach?	39	12	27
Should a book written by an atheist be removed from the library?	60	35	25
Should a communist be allowed to speak?	57	27	30
Should a communist be allowed to teach?	41	6	35
Should a book written by a communist be removed from the library?	57	27	30

Source: Center for Political Studies, University of Michigan, 1954 data; National Opinion Research Center, University of Chicago, 1977 data.

\*The data reported are the percentages giving the tolerant response. These questions have been repeated by NORC several times in the 1970s and the results are all virtually identical to those in column one above.

could be so classified by 1973. They concluded, "The most important finding from our efforts to track trends in American tolerance is that citizens who are most supportive of civil liberties have emerged as the majority in our society—and they are not a 'silent majority'" (p. 12).

The conclusions of these two studies then are, first, that levels of tolerance in the United States have increased significantly in the last 20 years and, second, that these changes flow partially from higher levels of education, partially from aging and cohort replacement, but primarily from broader political forces that have improved the climate for tolerance and civil liberties. Nunn and his associates mention such factors as the secularization of society and the Watergate episode as sources of the change, and they go so far to conclude that, "given the substantial increase in public support for democratic principles, the risk of demagogic takeover or the undermining of civil liberties, is less now than it once was" (p. 158). In other words, they have gone well beyond their empirical finding that tolerance of communists, atheists, and socialists has increased and have proceeded to conclude that tolerant beliefs in general are now more widely held, and that support for democratic principles has increased as well. Furthermore, the conclusion that tolerance has increased since 1954 rests upon assumptions about the meaning and measurement of tolerance that Stouffer first introduced. The conclusion therefore hinges in part on the validity of these assumptions. To challenge this conclusion, we must reconsider the assumptions upon which it is based. As we do this, a more tenable basis for the study of tolerance will be developed.

### A Reconsideration of Political Tolerance

Tolerance implies a willingness to "put up with" those things that one rejects. Politically, it implies a willingness to permit the expression of those ideas or interests that one opposes. A tolerant regime, then, like a tolerant individual, is one that allows a wide berth to those ideas that challenge its way of life (see Crick, 1973, Ch. 3).

We should observe at the outset, then, that tolerance presumes opposition or disagreement. If there is no reason to oppose, then there is no occasion for one to be tolerant or intolerant. The question does not arise, since it is pointless to ask people to tolerate a doctrine or practice of which they approve or toward which they are indifferent. The problem of tolerance only arises once there are grounds for real disagreement; what one proceeds to do at this point determines whether one is tolerant.

In this sense, tolerance is conceptually "content-free" in that the content of the ideas that one opposes are irrelevant to the principle itself. One is tolerant to the extent one is prepared to extend freedoms to those whose ideas one rejects, whatever these might be. The analytical problem, from our standpoint, arises from the fact that people oppose or reject different groups or ideas. Smith may be particularly concerned about communists, while Jones is concerned about the Ku Klux Klan, and so on. To measure tolerance, we must first discern an objection, and this cannot be assumed by simply asking respondents about generally unpopular groups. They must identify such groups for themselves, since the targets of intolerance may vary widely among individuals. If respon-

dents are simply asked about groups that are generally assumed to be unpopular and tolerance is measured with reference to attitudes about these groups, we are bound to confuse tolerance with the contents of respondents' beliefs about the groups the investigator selects.

In the studies previously reviewed, the measures of tolerance were not content-free, since the questions asked invariably referred to specific groups, generally of a leftist persuasion. The items used in Stouffer's study, and in subsequent attempts to monitor changing levels of tolerance, referred to communists, atheists, and socialists. Hence, in these studies, tolerance and intolerance for these particular groups have been confused with tolerance and intolerance more generally understood. The difficulty with this technique is that while one might be tolerant of communists or other radical groups, one might at the same time be quite intolerant of other groups on the right, such as racists, fascists, or nativists. At the same time, those who support repression against groups on the extreme left could be very tolerant of the groups just mentioned. Thus, the validity of items used to measure tolerance is fundamentally called into question.<sup>2</sup>

#### A Content-Controlled Measure of Tolerance

What is needed is a measurement procedure which allows respondents themselves to specify the groups they most strongly oppose. In an attempt to obtain such a measure, we developed and tested the following measurement approach. First, we provided each interview respondent with a list of potentially unpopular groups that ranged from communists and social-

ists on the left, to fascists, John Birch Society members, and Ku Klux Klan members on the right. We also included a number of groups, such as atheists, pro-abortionists, and anti-abortionists, which we expected in some ways to represent positions that are orthogonal to the left-right dimensions. (See Appendix A for the exact question wording and the specific groups listed.) Respondents were then asked to identify the group they liked the *least*, and we made it very clear that they could select a group *not* on our list. Respondents were then presented with a series of statements in an agree-disagree format which elicited their views about a range of activities in which members of that group might participate. The following statements were among those included in the series:

- (1) Members of the \_\_\_\_\_ should be banned from being president of the United States.
- (2) Members of the \_\_\_\_\_ should be allowed to teach in the public schools.
- (3) The \_\_\_\_\_ should be outlawed.
- (4) Members of the \_\_\_\_\_ should be allowed to make a speech in this city.
- (5) The \_\_\_\_\_ should have their phones tapped by our government.
- (6) The \_\_\_\_\_ should be allowed to hold public rallies in our city.

The statements were read as they appear above with the blanks filled with the group selected by each respondent. Respondents were also asked to pick their *second* least-liked group, and questions 1-6 were repeated. (Additional groups could be volunteered by the respondent; in fact, we encouraged this. See Appendix A.)

Our intention was to avoid contaminating the tolerance-intolerance dimension with the respondents' political beliefs. If we had merely asked all respondents whether communists should be allowed to hold public office, their responses would depend not only on their levels of tolerance, but also on their feelings toward communists. Previous studies have looked at tolerance in either of two ways: (1) by asking respondents whether they would tolerate certain groups, such as communists, socialists or atheists (Stouffer, 1955; Davis, 1975); or (2) by asking respondents whether they agree with general procedural norms in abstract terms. (Prothro and Grigg, 1960, and McClosky, 1964, used both of these procedures.) The advantage of our procedure is that it creates a situation in which the evaluation of each respondent toward the group in question is held constant. This measurement generates "content-con-

<sup>2</sup>In order to examine the validity of our critique by using the Stouffer items, we have performed a maximum likelihood factor analysis of the 1954 and 1974 Stouffer items, applying the SIFASP computer program which allows a simultaneous fitting of a model to separate populations (Jöreskog, 1969, 1970, and 1973). For a brief presentation of these results, see Sullivan et al. (1979). In brief, we find that almost all of the change in tolerance in the Stouffer items can be explained as a result of the decreasing salience of the three groups studied: communists, socialists, and atheists. Almost none of it can be accounted for by changes in attitudes toward the three acts noted in Table 1 (allowing atheists or communists to speak, to teach, or to have their books in a public library). A somewhat more detailed analysis of these data can be obtained from the authors. Our case, however, rests primarily upon the original data presented in this article.

trolled" responses and also prevents respondents from expressing agreement with general norms which they then fail to apply to specific groups. Clearly, our measures are not "content-free" since there is a context and a specific group toward which each respondent must react. We thus call it a "content-controlled" measure, to emphasize that we have attempted to "control for" the content by allowing respondents to select functionally equivalent groups. We view the group selected by each respondent as having a similar meaning across respondents' psychic organizations, even though different groups are selected.

A second sample was presented with the Stouffer approach to measuring tolerance, which specified either communists or atheists as points of reference. These items are identical to those used by Stouffer in his 1955 study (and by NORC in the 1970s) to measure tolerance of nonconformity, and they consisted of the following questions:

- (1) There are always some people whose ideas are considered bad or dangerous by other people. For instance, somebody who is against all churches and religion. If such a person wanted to make a speech in your city against churches and religion, should he be allowed to speak, or not?
- (2) Should such a person be allowed to teach in a college or university, or not?
- (3) If some people in your community suggested that a book he wrote against churches and religion should be taken out of your public library, would you favor removing this book, or not?
- (4) Now, I should like to ask you some questions about a man who admits he is a communist. Suppose this admitted communist wanted to make a speech in your community. Should he be allowed to speak, or not?
- (5) Suppose he is teaching in a college. Should he be fired, or not?
- (6) Suppose he wrote a book which is in your public library. Somebody in your community suggests that the book should be removed from the library. Would you favor removing it, or not?

By our standards, these questions do not guarantee equivalence of group meaning, since they measure tolerance with reference to groups selected by the investigators. We would expect these items primarily to measure attitudes toward communists and atheists rather than tolerance. Since we have presented these

different sets of questions to separate random samples, we can readily measure the influence of different measurement strategies.

#### Levels of Tolerance: Differences by Measurement Strategy

We conducted two independent surveys in Minneapolis and St. Paul, Minnesota during the spring and summer of 1976. One independent random sample of respondents was asked our new set of tolerance questions while the other was asked the old Stouffer set. Two independent random samples of size 300 were selected from the Twin Cities' city directories, which include a listing of all adults in Minneapolis and St. Paul. Interviews were completed with 200 persons using the old questions and with 198 persons using the new questions, a response rate of about 66 percent.<sup>3</sup>

In addition, we will present data from a national opinion survey conducted for us by NORC in the spring of 1978. We included our content-controlled questions as well as four of the Stouffer questions. Thus we can compare the results both from an equivalent samples design and from a single sample which was asked both sets of questions.

Table 2 presents the percentage of tolerant responses for questions using both the least-liked and the second least-liked groups in one of the Twin Cities samples. The comparable

<sup>3</sup>Interviewers were trained by the senior author during a one-day workshop and subsequent individual training sessions. Each interviewer conducted several practice interviews and was evaluated by the senior author and the subjects being interviewed. A lengthy training manual was prepared and much time was spent to ensure that interviewers would handle similar situations and problems in the same, objective manner. Weekly meetings were held after the interviewing began to go over problems and to standardize responses to these problems. The interviewers were hired through the University Employment Service at the University of Minnesota. Approximately 15 interviewers were used, most of them students. All interviewees were called or visited personally by a research assistant subsequent to the interview to ensure that the reported interview had taken place and that the interviewer had been competent. We have been unable to discover any data problems related to interviewers.

The two samples do not differ from each other or from the population on any demographic characteristics (see Sullivan, Piereson and Marcus, 1978). Both samples are representative of the larger population, and differences in tolerance may thus be attributed to different measurement strategies rather than differences between samples per se.

Table 2. Levels of Tolerance for Content-Controlled and for Stouffer Items

	1976 Twin Cities Split Samples		1978 NORC
	Percent Tolerant, Least-Liked Group	Percent Tolerant, Second Least- Liked Group	Percent Tolerant, Least-Liked Group
<b>A. Content-Controlled Items</b>			
Members of the _____ should be banned from being president of the U.S.	28	32	16
Members of the _____ should be allowed to teach in public schools.	26	30	19
The _____ should be outlawed.	33	43	29
Members of the _____ should be allowed to make a speech in this city.	70	67	50
The _____ should have their phones tapped by our government.	70	70	59
The _____ should be allowed to hold public rallies in our city.	57	51	34
<b>B. Stouffer Items</b>			
	Percent Tolerant		Percent Tolerant
Should an atheist be allowed to speak?*	78		65
Should an atheist be allowed to teach?	63		40**
Should a book written by an atheist be removed from the library?	80		62
Should a communist be allowed to speak?	71		63
Should a communist be allowed to teach?	62		40**
Should a book written by a communist be removed from the library?	79		64

Source: Twin Cities Survey and National Opinion Research Center, University of Chicago.

\*See the text for the exact question wording. For part A, N = 198 and for part B, N = 200, in the 1976 split samples.

\*\*Question not asked in the 1978 survey. Figures are from the 1977 NORC General Social Survey. N = 1509, in the 1978 NORC Survey.

percentages for the Stouffer items, given to the other Twin Cities sample, are also in Table 2, as well as the results from the 1978 NORC survey. According to the Twin Cities sample which responded to the Stouffer items, strong majorities are tolerant on every item, ranging from 62 percent tolerant on the "communist-teach" question to 80 percent tolerant on the "atheist-book" question. According to the other Twin Cities sample, which responded to the content-controlled questions, the portrait looks more bleak. The percentage tolerant ranges from 26 on the teaching question, to 70 on the speech and wiretapping questions. In fact, one finds only one-third of the citizenry willing to say that their least-liked group ought not to be outlawed. Further, only about one-fourth of the respondents were willing to allow members of their least-liked group to teach in schools, and to say they ought *not* to be banned from being president. Only about half were willing to

allow this group to hold public rallies in their cities.<sup>4</sup>

It is our interpretation that the content-controlled items reveal more intolerance than the Stouffer items because they allow respondents to select from a much wider range of groups. People who may be willing to allow communists and atheists their full range of civil liberties, perhaps because they sympathize with such groups or fail to find them particularly threatening, may be unwilling to allow the Ku Klux Klan the same rights, perhaps because they do not sympathize with the Klan and find it dangerous.

None of the Stouffer items in column one

<sup>4</sup>Since our questions are five-point agree-disagree items, the percentage tolerant reflects those who agree or agree strongly (or disagree, depending upon the direction of the statement).

produces a majority in favor of depriving either of these groups of fundamental civil liberties. It should be noted, however, that these sets of questions are not *strictly* comparable. The contents of the questions are not identical, and in addition, the content-controlled items were presented in an *agree-disagree* format while the Stouffer items were presented in a *yes-no* format. Nevertheless, over half of the Twin Cities respondents given the content-controlled items believe that their least-liked group should be outlawed, hardly consistent with the recent conclusion that the mass public is increasingly tolerant.

Our content-controlled question on allowing the least-liked group to teach is worded similarly to the Stouffer item on allowing atheists or communists to teach, and while only 26 percent of the content-controlled sample thought members of a least-liked group should be allowed to teach, fully 63 and 62 percent of the sample responding to the Stouffer items gave the tolerant response. Certainly this suggests that although tolerance of communists and atheists has increased, the overall extent of tolerance may not have changed much at all. Our analysis suggests that the explanation for Davis' and Nunn's findings could well be one Davis rejects: that the cold war and fervor of the 1950s produced a convenient outlet for intolerance against communists and their "fellow traveler" atheists. Since the number of targets (on both the right and left) for political intolerance multiplied during the 1960s and 1970s, and since the cold war fervor of the political elites waned considerably during this time, we might well expect more tolerant responses toward communists and atheists while, at the same time, other groups were becoming the major targets of this intolerant impulse. We suggest that the aggregate level of tolerance may not have increased very much, even though tolerance toward these particular groups has undeniably increased.

Table 2 also presents the results from the national survey. In general, the results are consistent with our question-wording experiment. In the national sample, using the content-controlled questions, we find that 19 percent are tolerant on the teaching question, while with the Stouffer teaching questions, we find 40 percent are tolerant (for atheists and for communists). Similarly, 50 percent are tolerant if we rely on the controlled question about freedom of speech, while 65 and 63 percent are tolerant if we use the Stouffer items. So it appears that whether one uses one sample, including both sets of questions, or uses equivalent samples, asking each sample one set

of questions, the conclusion is inescapable: higher levels of tolerance are indicated with the Stouffer items. And although some of the content-controlled items indicate a majority of tolerant citizens, most do not.

Some of the differences between the two sets of items are the result of somewhat different wordings of the questions. Further, the degree of tolerance depends in part on the nature of the act to be tolerated. Some acts are "easier" to tolerate: for example, allowing someone to speak as against allowing someone to teach. Nevertheless, the wording of two of the items across the sets, the speaking and teaching items, are similar enough to attribute the observed differences in tolerance to differences in measurement strategies.

The responses on the second least-liked group are slightly more tolerant than on the least-liked group. Certainly this is true of the first three items in Table 2, those that show the smallest proportion of tolerant responses. The first two indicate an increase in tolerance of 4 percent, the third an increase of 10 percent. This suggests that for each respondent, as we move from the least-liked group toward groups that are assessed more neutrally, we find the indicated tolerance to be higher. Therefore the Stouffer items produce more apparent tolerance because the communists and atheists are not "least-liked" for many respondents in the current population, so that these respondents appear to be more tolerant.

We can examine this point in more detail by analyzing the data in Table 3. In the content-controlled survey, after we asked respondents our six tolerance questions about their least-liked group, each respondent then judged that group (on a seven-point scale) on the seven pairs of adjectives listed in Table 3. We also repeated this procedure for each respondent's second least-liked group. In the Twin Cities sample that contained the Stouffer items, we asked each respondent to judge communists and atheists on the same seven pairs of adjectives.

Looking first at the mean scores for the content-controlled items, we see that the three most extreme adjective pairs are *good-bad*, *democratic-undemocratic*, and *dangerous-safe*. The other items all have mean evaluations among the middle three categories. The respondents' least-liked groups are perceived as uniformly bad, undemocratic, and dangerous. As one would expect, the means are less negative, closer to the middle categories, for the second least-liked group. A different picture emerges, however, for the Stouffer items. For the sample given these items, almost all seven adjective

pairs for *both* communists and atheists have means in the middle categories of 3, 4, and 5. For example, although the least-liked group has a mean of 6.14 on the *good-bad* dimension, that figure is 5.41 for communists and only 4.61 for atheists; while for the least-liked group the mean on *democratic-undemocratic* is 6.38, for communists it is 6.07 and for atheists it is 4.81. For the least-liked group it is 1.89 on *dangerous-safe*, for communists it is 2.62 and for atheists it is 3.37. In fact, when one compares the means for the least-liked group to the means for communists and atheists, in all 14 instances the means move from the less to the more desirable evaluation. Thus, atheists are perceived as less important, less dishonest, less bad, less unpredictable, less dangerous, less strong, and less undemocratic than the respondents' least-liked group. The same is true of communists.

We feel quite certain, then, that the mass public is still generally intolerant today. Tolerance of atheists and communists has increased primarily because they are now perceived as less threatening and dangerous than they were in the 1950s. Other groups are now more salient, and one must study a multiplicity of groups to study tolerance. The reader might note that in every case, the mean score for the second least-liked group is less "negative" than that of the least-liked group. Although the differences are generally small, they are consistent. We expect that if one were to progress down the line toward third, fourth, fifth, etc. least-liked groups, tolerance would slowly increase.

In the national sample, the mean score for the least-liked group was 1.97 on *dangerous-*

*safe* and 6.09 on *good-bad*. Thus, we have again found groups toward which almost all respondents feel negative, and which almost all feel are dangerous. That, of course, is the intent of our measurement procedure. If some respondents feel more positive than others toward the group we ask them about, it is not clear that we are measuring tolerance in a meaningful way. We feel confident that our procedure controls for such exogenous influences.

#### The Content-Controlled Measure: A Brief Look at Validity

The exact question wording of our content-controlled measure is given in Appendix A. The frequency distributions for groups selected as least-liked and second least-liked are found in Table 4. (All data presented from this point on are from the NORC 1978 national survey.) Only 38 percent of all respondents selected the socialists, communists or atheists as their least-liked groups, which suggests that in the earlier NORC studies used by Davis (1975) and the study by Nunn et al. (1978), approximately 60 percent of respondents were probably questioned about other than their least-preferred group. In fact, fully 30 percent of our respondents selected one of the three radical-right groups included in our analysis, and an additional 14 percent chose the Black Panthers and the Symbionese Liberation Army, radical-left groups not in existence in the 1950s. The major point, of course, is that the choices were spread out across the ten groups; if we were to ask respondents about only a subset of them, it would be more difficult for some respondents

Table 3. Semantic Differential Items for Both the Content-Controlled and the Stouffer Measures:  
Twin Cities Samples

	Content-Controlled Measures		Stouffer Measures	
	Mean Score* for Least-Liked Group	Mean Score for Second-Least- Liked Group	Mean Score for Communists	Mean Score for Atheists
Important-Unimportant	4.55	4.29	4.33	4.48
Honest-Dishonest	5.27	5.20	4.77	3.74
Good-Bad	6.14	6.09	5.41	4.61
Predictable-Unpredictable	4.96	4.66	4.71	4.65
Dangerous-Safe	1.89	2.20	2.62	3.37
Strong-Weak	3.62	3.48	3.63	3.92
Democratic-Undemocratic	6.38	6.24	6.07	4.81

Source: Twin Cities Survey, 1976.

\*These means are based on semantic differential scales ranging from 1-7. The higher the mean, the closer the average perception was to the second of the adjective pairs. For example, the mean of 6.14 for *good-bad* reflects the fact that almost every respondent perceived their least-favorite group as *bad*, while the mean of 1.89 on *dangerous-safe* means almost all of them perceived this group as *dangerous* (rather than *safe*).

Table 4. Targets of Intolerance (Percent)

	Least-Liked Group	Second Least-Liked Group
Socialists	1	2
Communists	29	19
Atheists	8	7
Symbionese Liberation Army	8	10
Black Panthers	6	10
Pro-abortionists	4	5
Anti-abortionists	2	3
John Birch Society	1	3
Ku Klux Klan	24	14
Fascists	5	6
Other Group	2	1
Don't Know	10	19
N = 1509		

Source: National Opinion Research Center, University of Chicago, 1978 Survey.

to be tolerant. For the 29 percent of our respondents who selected communists as their least-liked group, it would be quite difficult for them to exhibit tolerance if we merely asked them questions about the communists. For the remaining 60 or 70 percent, however, it might be much easier because they do not fear the communists, or feel them dangerous or threatening. This is likely to be true no matter which group we study, so long as we do the selecting and force each respondent to react to our predetermined groups.

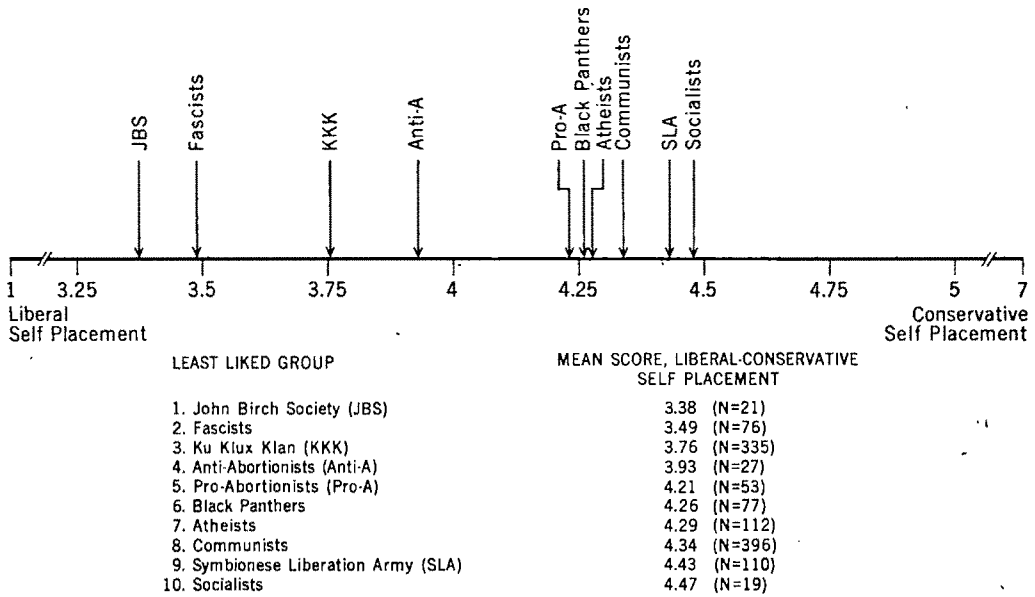
To examine the validity of our measurement, we gave our respondents the traditional seven-point liberal-conservative self-identification scale. In Figure 1, we present the mean score on this scale for respondents who picked each of the various groups as their least-liked group. For example, the respondents who selected the John Birch Society as their least-liked group had a mean score on the self-placement scale of 3.38, toward the liberal end; those who selected the socialists had a mean score of 4.47, toward the conservative end of the scale. As is evident from the rank-ordering in the figure, the groups are ordered roughly as one would expect if one assumes that liberals generally dislike right-wing groups and conservatives dislike left-wing groups. Four groups of respondents had mean scores on the liberal side of 4 (the neutral point): those who selected the Birch Society, fascists, the Klan, and anti-abortionists as their least-liked. Six groups had means on the conservative side of 4: those who selected the socialists, the SLA, communists, atheists, the Black Panthers, and pro-abortionists. The groups of respondents selecting the groups studied by Stouffer (and subsequently

by Davis and Nunn) had means ranging between 4.29 and 4.47, clearly on the conservative side. Thus in those studies, it is probably true that respondents who identified themselves as conservatives had more difficulty giving tolerant responses about the communists, socialists, and atheists than did those who considered themselves liberals. To study tolerance correctly, one needs to include groups from the entire ideological spectrum, and apparently our procedure affords such coverage.<sup>5</sup>

It appears that our measure does what it should—it discriminates between liberals and conservatives, and it presents a number of groups from across the ideological spectrum. This makes it likely that most of our respon-

<sup>5</sup>To examine further the validity of our measurement, we repeated the analysis in Figure 1 using respondents' second least-liked group, and our results were identical. The extremes are again represented by those whose second least-liked group is the Birch Society (mean of 3.33) and those whose second least-liked group is the socialists (mean of 4.92). Again, respondents whose second least-liked group is the Birch Society, fascists, anti-abortionists, and the Klan, have mean self-placement scores on the liberal end of the continuum, while those whose second least-liked group is the Black Panthers, the SLA, communists, pro-abortionists, atheists, or socialists have mean scores on the conservative end of the continuum. We repeated this exercise using four of the Survey Research Center's issues questions—on jobs, medical care, school integration, and black welfare—and the results are basically the same. (We created a scale using these four issues, and did our analysis for both least-liked and second least-liked group. These figures are available from the authors.)





Source: National Opinion Research Center, University of Chicago, 1978 Survey.

Figure 1. Liberal-Conservative Self-Placement by Least-Liked Group

dents were able to find one of their disliked groups on our list.

Another way to examine the validity of our measurement procedure is to examine what we shall label "crossovers." By crossovers we refer to those respondents who select one right-wing and one left-wing group as their two least-liked groups. If our respondents do indeed select as disliked those groups furthest away from themselves on the ideological continuum, then we expect that respondents near the middle of the continuum would be more likely to be crossovers than those who more clearly label them-

selves as liberals or conservatives. These results are presented in Table 5. Among people who define themselves as the most liberal on our seven-point scale, 40 percent of them select two right-wing groups as their two least-liked, despite the fact that there are only three groups that we can clearly label as right-wing. There is a sharp decline in the percent selecting two right-wing groups as we go across the categories of conservatism. At the other extreme, only one percent and zero percent of those who score themselves as 6 or 7 on the conservatism scale select two right-wing groups.

Table 5. Liberal-Conservative Self-Placement and Target Groups Picked (Percent)

Groups Picked Are:	Liberal					Conservative	
	1	2	3	4	5	6	7
Both Left-Wing	11	16	20	35	38	47	53
One Left and One Center*	0	6	9	14	10	12	6
One Left and One Right	40	49	53	41	42	38	29
One Right and One Center	9	9	5	6	5	2	12
Both Right-Wing	40	18	13	4	4	1	0
N =	(35)	(109)	(136)	(428)	(224)	(114)	(17)

Source: National Opinion Research Center, University of Chicago, 1978 Survey.

\*The two abortion groups are defined as centrist groups. (No respondents selected both abortion groups as their two least-liked groups.) Note that in Figure 1, the two abortion groups are the two closest to 4, which is the midpoint on the liberal-conservative scale.

On the other hand, fully half of those who label themselves as conservatives select two left-wing groups, while only about one in ten of the liberals does so. Looking at the crossovers, we see that those respondents who place themselves toward the middle of the scale are in fact more likely to select one left- and one right-wing group. Going from left to right, 40 percent of liberals are crossovers, and this percentage rises to 53 percent for the third category, and then begins a decrease until only 29 percent of conservatives are crossovers. (More liberals are crossovers because there are five left-wing groups and only three right-wing groups on our list.) Although the pattern does not peak in category 4, it is clearly curvilinear, particularly in comparison to the straight linearity exhibited by the first and last rows in the table.

### Concluding Comments

The foregoing analysis leads to two important conclusions about tolerance in the United States:

(1) Stouffer's method of measuring tolerance with reference to communists, socialists, and atheists is inadequate and, to a large extent, time-bound. It is inadequate because it does not fully capture the meaning of the concept of tolerance. It is time-bound because it presumes that these particular groups are the only important targets of intolerance in the society. This *may* have been more or less true in 1954, so that Stouffer's conclusions *may* have been appropriate for the limited purposes of his research. But it is certainly not true now. Hence, attempts to monitor changing levels of tolerance with this procedure are inappropriate and produce misleading conclusions.

(2) Substantively, the content-controlled method of studying tolerance developed above reveals that intolerance has not necessarily declined much over the past 25 years, but merely has been turned toward new targets. Our data show that while the mass public is now more tolerant of communists, atheists, and socialists than it was in 1954, other targets of intolerance have emerged in the meantime to neutralize this change. On the face of things, then, it appears that the present period differs from the earlier one in that there are now more targets of intolerance but none which is sufficiently important to generate a major threat to civil liberties.

These conclusions, while important in their own right, also raise other questions about the understanding of tolerance that has been handed down through the earlier tradition of re-

search in the area. If we are correct in asserting that tolerance has been incorrectly conceived and measured, we expect that other accepted generalizations in the area might also be of dubious validity. For example, Stouffer and others found that, among individuals, education was the most important "cause" of a tolerant outlook. The work that we have done so far on this question suggests that this relationship is largely an artifact of the groups selected as points of reference against which to measure tolerance. Paradoxically, those with lower levels of education are most threatened by and most opposed to dissident groups on the left—that is, precisely those groups selected in the earlier studies as points of reference for measuring tolerance. When individuals are given the opportunity to select the groups (from both the left and the right) to which they are opposed, the powerful relationship between education and tolerance is reduced considerably. The faith, therefore, that many have placed in education, conventionally conceived, as a solution to the problem of intolerance is apparently misplaced.

It should be emphasized that we are not resurrecting the old argument, developed by Prothro and Grigg (1960) and McClosky (1964), that because large numbers of citizens are intolerant, a meaningful democratic politics, with all that this implies, is a utopian goal. There is a sense in which these writers began their studies with a utopian version of democracy, a major condition of which was that nearly all citizens would accept the creed of tolerance in a form similar to that laid down by John Stuart Mill in *On Liberty*. When citizens did not measure up to the standard, these scholars began to recast their understanding of democracy in order to find sources of democratic stability in places other than in citizen virtue. Now that more recent studies have found that levels of tolerance are on the increase, it has been suggested (Nunn et al., p. 159) that we can begin to resurrect the classical theory. In this strange way, empirical findings concerning levels of tolerance in the society have shaped our understanding of democracy itself.

As others (Berns, 1962; Pateman, 1970) have pointed out, the "classical" view of democracy which served as the theoretical compass for these studies was itself a modern construction. As such, the theory did not take into account a number of fundamental questions about the relationship between tolerance and democratic politics that were raised by earlier liberal thinkers. The theory assumes, for example, that political tolerance is good and that any deviation in the society or among individuals from an absolute standard of toler-

ance is undesirable. In this sense, it rests upon a normative view of democracy that resembles the position taken by Justice Oliver Wendell Holmes in his famous dissent in the case of *Abrams v. United States* (1919: 630):

If you have no doubt of your premises or your power and want a certain result with all your heart you naturally express your wishes in law and sweep away all opposition. To allow opposition by speech seems to indicate that you think the speech impotent, as when a man says he has squared the circle, or that you do not care wholeheartedly for the result, or that you doubt either your power or your premises. But when men have realized that time has upset many fighting faiths, they come to believe even more than they believe in the very foundations of their own conduct that the ultimate good desired is better reached by free trade in ideas—that the best test of truth is the power of the thought to get itself accepted in the competition of the market, and that truth is the only ground upon which their wishes safely can be carried out. That at any rate is the theory of our Constitution.

This is not the place to raise the various criticisms of this doctrine, since most of them are well known. The merits of these abstract points aside, it seems to us that any theory of democracy that relies upon a widespread acceptance of this doctrine, or of something similar to it, is quite unrealistic and, in any case, unnecessary to the functioning of a democratic system.

It is more prudent, in our view, to take one's bearings on these questions from *The Federalist*. Contrary to Holmes, the Framers did not base the Constitution on the notion that political truth emerges from the competition of the market; nor did they believe that it was necessary that citizens accept this doctrine in order for a republican system to survive. As is well known, in *Federalist* 51 Madison (Cooke, 1961, pp. 351–52) put his faith in more practical and realistic safeguards:

In a free government, the security for civil rights must be the same as for religious rights. It consists in the one case in the multiplicity of interests, and in the other, in the multiplicity of sects; and this may be presumed to depend on the extent of the country and number of people comprehended under the same government.

For Madison, then, the safeguards consist in the processes of politics and in the requirements of coalition rather than in the acceptance among citizens of an abstract creed similar to that suggested by Holmes. To be sure, Madison recognized that this was a problematic solution and not a hard and fast safeguard.

It makes some sense, therefore, to interpret the findings of this article in terms of Madison's prescriptions. When the political system provides a multitude of convenient targets for intolerance, the result is what one might call "pluralistic intolerance." The political consequences of such a situation may be quite different from those of a situation in which dominant targets of intolerance exist, a difference not unlike the varying consequences of cross-cutting versus overlapping cleavages. The findings of this study suggest that even though levels of intolerance are now quite high in American society, the diversity of the targets of intolerance prevent, for the time being, a substantial threat to civil liberties. Nevertheless, for those truly concerned with this problem, the dangers of intolerance still exist, for given the right circumstances, these attitudes could be focused and mobilized, as they were in the 1950s. For those who will escape into abstractions, this will seem a pessimistic conclusion. For others, perhaps, who are used to thinking about the realities of politics, it may come as no surprise.

## Appendix A

Instructions to interviewer: Hand the respondent our handout A, the "List of Groups in Politics." Then say:

I am giving you a list of groups in politics. As I read the list please follow along: socialists, fascists, communists, Ku Klux Klan, John Birch Society, Black Panthers, Symbionese Liberation Army, atheists, pro-abortionists, and anti-abortionists. Which of these groups do you like the least? If there is some group that you like even less than the groups listed here, please tell me the name of that group.

(Note to interviewer: If they have trouble making up their mind, encourage them to think, just generally, which group is the most unpleasant, in their opinion. If they really can't decide, mark that opinion below.)

\_\_\_\_\_ respondent can't decide; doesn't know  
 \_\_\_\_\_ respondent dislikes group not listed here  
 (fill in name of group below)

\_\_\_\_\_ socialists  
 \_\_\_\_\_ fascists  
 \_\_\_\_\_ communists  
 \_\_\_\_\_ Ku Klux Klan  
 \_\_\_\_\_ John Birch Society  
 \_\_\_\_\_ Black Panthers  
 \_\_\_\_\_ Symbionese Liberation Army  
 \_\_\_\_\_ atheists  
 \_\_\_\_\_ pro-abortionists  
 \_\_\_\_\_ anti-abortionists (pro-lifers)

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# Presidential Power versus Bureaucratic Intransigence: The Influence of the Nixon Administration on Welfare Policy\*

RONALD RANDALL  
*University of Toledo*

*Many observers routinely assert the relative weakness of presidents before the bureaucracy. The research of this study, guided by a structuralist theory of organizations, provides evidence of the Nixon administration's power to change policy, even over the opposition of the bureaucracy, concerning the Aid to Families with Dependent Children program. The study demonstrates that the management tools available to the president and top officials, when used adroitly, are more powerful than are generally presumed. That presidents can affix their indelible stamps on policy by short-circuiting the legislative process and dominating the bureaucracy is more than a remote possibility.*

Presidents and political scientists often agree on the extraordinary difficulties of exercising presidential direction and control over the bureaucracy. Many officials bore the wrath of Richard Nixon, such as the Small Business Administration agent who disobeyed orders: "[He] deliberately did not—I read the memorandum—he did not carry out an order I personally gave" (*New York Times*, 20 July 1974, p. 14). In an Associated Press interview after eight months in office, Jimmy Carter agreed that presidential powers were more limited than he expected: "I underestimated the inertia or the momentum of the federal bureaucracy. . . . It is difficult to change" (*New York Times*, 23 October 1977, p. 36). Long before Nixon and Carter, other presidents complained of the intransigence of the bureaucracy (Hughes, 1973, pp. 181–95).

From their own frustrating experience of concentrating attention on decisions not implemented, presidents understandably feel that the bureaucracy continually sabotages their programs. Presidents see more intransigence than compliance in the bureaucracy because they necessarily spend more time pursuing failures in direction and control than savoring successes.

Relying on the recollections of presidents, aides, and departmental officials about presidential-bureaucratic relations, many political scientists have come to the same conclusion about the difficulties of exercising presidential direction and control. For example, after interviewing 43 members of recent presidential staffs who recalled a high level of conflict with

the departments and agencies, Cronin (1970, p. 391) concluded that "presidents have been and will continue to be frustrated by the sluggishness of the federal executive branch's response to new priorities."

The 1976 Aberbach and Rockman report, based on a 1970 survey of top federal career and political officials, asserted that bureaucrats are too Democratic and political appointees too liberal for a conservative Republican president to have much policy impact. Their prediction for the "foreseeable future": "The White House will continue to reflect short-term political changes and . . . the bureaucracy will be *relatively immune* to these dynamics" (Aberbach and Rockman, 1976, p. 467, emphasis added).

Just as presidential recollections accentuate the weaknesses of the office and the powers of the bureaucracy to block their initiatives, a political science research strategy which focuses interview questions on presidential-bureaucratic conflict (rather than changes in policy accomplished because of presidential actions) magnifies the obstacles to presidential control. Shortly after the Aberbach-Rockman survey, President Nixon changed his approach, intending "to *take over* the bureaucracy and *take on* the Congress, to concentrate on administrative steps and correspondingly to downgrade legislation as the principal route for bringing about domestic policy change" (Nathan, 1975, p. 8). Many of the stormy presidential-bureaucratic battles that ensued produced a plethora of painful policy losses for the bureaucrats.

Rather than employ a research strategy that emphasizes the recollections of presidential conflict with bureaucrats, this study examines the means used by the Nixon administration to influence the Aid to Families with Dependent Children (AFDC) program, and offers an assessment of the subsequent changes. Although this approach prevents the inevitable presidential-

\*Martha Derthick, Jane S. Randall, and David S. Wilson provided helpful comments. Part of the research for this study was conducted while the author held a University of Toledo Summer Faculty Research Fellowship.

bureaucratic conflict from obscuring the very real powers available to administratively astute presidents, this study does draw on interviews with bureaucrats who are probably inclined to view their programs as especially vulnerable to political interference, inviting the criticism that one methodological bias has been substituted for another. However, the focus on the means actually employed to change the AFDC program and the discussion of the policy changes which followed should blunt that criticism. More importantly, this focus will demonstrate that under certain conditions the tools available to the president, when used adroitly, are more powerful than generally presumed.

The welfare bureaucracy of the Department of Health, Education and Welfare inherited by President Nixon in 1969 was part of a highly complex system that included the state and local welfare offices. For nearly 35 years, federal bureaucrats had been suggesting, guiding, prodding and otherwise encouraging the state and local organizations to be more professional, less restrictive in determining eligibility of claimants, and more generous in payment levels. Nixon's attitude toward welfare was decidedly more restrictive. The techniques his administration employed to implement his views beginning near the end of his first term will not surprise students of organizational theory, especially those of a structuralist persuasion (Perrow, 1972) who stress that a knowledgeable manager usually can find the resources to make significant policy changes despite opposition and conflict with subordinates. March and Simon (1958, p. 129) suggest four possible reactions to organizational conflict: problem solving, persuasion, bargaining, and "politics." A thoroughgoing structuralist approach, however, suggests that skillful managers achieve their greatest policy successes over the bureaucracy by manipulating structure, e.g., changing work assignments, the division of labor, the communication system, and influencing recruitment and promotion, training, and indoctrination. This occurs especially in large organizations, where budgets are big and where deft administrators have slack resources available for modifying the organizational structure to achieve their goals.

The assault on welfare began in 1971. It was manifested initially by an extraordinary change in welfare advisers and administrators. Although there may not have been a detailed blueprint at this time for taking over welfare, the new team responded to Nixon's policy views and cues by developing and refining managerial means to influence policy.

### Changing the Nixon Welfare Administrators

Like many new presidents, Richard Nixon had only vague notions of the specific directions that he wanted domestic policy to take. Despite his years in Congress and the vice-presidency and his acquaintance with Republican party workers and officials around the nation, he knew surprisingly little about government in Washington. Staffing the highest levels of government required naming people with whom he was unacquainted (Evans and Novak, 1971, pp. 9-10, 66). Lacking a clear sense of purpose and specific policies, President Nixon allowed department heads to hire the lesser executives, an action he soon regretted.

When liberal Nixon protégé Robert Finch was appointed to head HEW, he staffed it with a group of progressives. Over the opposition of Governor Ronald Reagan of California, Finch selected John Veneman as under-secretary, responsible for welfare policy. Veneman, a liberal California legislator, expert in welfare and health programs, had opposed Reagan's proposed cutbacks of Medicaid benefits in that state. Veneman brought in Tom Joe, who was largely responsible for exploiting federal regulations to allocate large amounts of federal money for increases in California social services (Burke and Burke, 1974, pp. 50-51; Derthick, 1975). In addition, Veneman surrounded himself with several holdover Democrats who had played important roles in welfare planning in the Johnson administration and gave them a key role in developing the Family Assistance Plan (Burke and Burke, 1974).

At lower levels of HEW the politically appointed welfare administrators also consisted of a surprisingly large number of holdover Democrats and/or welfare professionals. Mary Switzer remained from the Johnson administration as director of the Social and Rehabilitation Service (SRS), the principal HEW welfare unit. She was a professional regarded highly in Congress, especially for her work in the Vocational Rehabilitation Administration. Stephen Simonds, a dedicated welfare professional with an educational background in social services administration, remained as head of the Assistance Payment Administration (APA), the SRS unit most responsible for the AFDC program. In May, 1970 he moved to the directorship of the APA sister agency, the Community Services Administration, where he proved to be a friend of welfare recipients by promoting social services.

The new commissioner of the Assistance

Payments Administration was John L. Costa, also a welfare professional, educated in social work, who had a long record of experience ranging from case work to state and federal social work administration.

Not until April, 1970 was a Republican appointed to head the SRS. Like his successors, John Twiname talked a management line, but this was "more rhetoric than reality," according to Derthick (1975, p. 36), who says his policy "instincts were liberal." He was compassionate and brought little change in policy. In mid-1971, Twiname recruited James Bax as commissioner of the Community Services Administration. Bax was flamboyant, political, and unconstrained by conservative Republican welfare principles. Derthick (1975, p. 72) relates how the Bax promotional philosophy was captured by the aphorism that spread throughout HEW: "You hatch it, we match it."

After two and a half years of Republican control of the executive branch, the administration of welfare rested in the hands of individuals at least as tolerant toward welfare, probably more so, than those in the previous Democratic administration. During this period, a major overhaul of the welfare system—the Family Assistance Plan (FAP)—was thrice submitted to Congress, welfare rolls continued to expand, and social services grants to the states soared from \$354 million in fiscal year 1969 to \$1.6 billion in FY 1972, and would have reached \$4.7 billion in FY 1973, had not the State and Local Fiscal Assistance Act of 1972 (86 Stat, 945–47) put a \$2.5 billion ceiling on the expenditures (U.S. Budgets, FY 1971, p. 436; FY 1974, p. 442).

The Nixon approach to the presidency and to domestic policy was undergoing a metamorphosis in 1971. Frustrated by the first-term legislative strategy which yielded little success in the face of a Democratic Congress and by not "getting domestic agencies to do what was wanted" (Nathan, 1974, p. 72), President Nixon changed from a legislative to a managerial strategy to achieve his goals. Although initially he "seemed excited and pleased about what most would consider the progressive elements of his domestic program," the second-term shift in strategy was accompanied by a significant change in philosophy: a decided shift to the right, "to a more pessimistic and conservative position in the area of domestic affairs," according to Richard Nathan (1975, pp. 70, 72), who held high-level positions in the first term. However, the dropping of the Family Assistance Plan exaggerates the appearance of change in Nixon's welfare philosophy. His

embrace of FAP was partly an aberration from his long-held welfare attitudes, attributable to the extraordinary persuasiveness of presidential advisor Daniel Patrick Moynihan (Burke and Burke, 1976). Mostly, his welfare reform support issued from his own understanding of FAP, which put more emphasis on work incentives than did most other observers (Nathan, 1973, p. 19). Thus, while supporting FAP, Nixon remained truer to his long-held welfare views than is generally perceived.

Whatever the change in Nixon's welfare attitudes, the approach of his administration toward welfare policy changed dramatically as the emphasis turned from helping the poor (albeit the working poor) to welfare cheats, reduction of social services, etc. President Nixon publicly affirmed this changed posture in a celebrated newspaper interview given to Garnett D. Horner on the Sunday before the 1972 election (published shortly afterwards) when he said:

Another thing this election is about is whether we should move toward more massive hand-outs to people, making the people more and more dependent, or whether we say, no, it is up to you. The people are going to carry their share of the load (*Washington Star-News*, 9 September 1972).

President Nixon's determination to take charge of the welfare bureaucracy grew. In HEW, personnel changes occurred at all appointive levels. Robert Finch and John Veneman left. In SRS, John Twiname was eased out. Lower in the department, James Bax, Stephen Simonds, John Costa and others of similar view departed.

Caspar Weinberger became HEW Secretary on February 12, 1973. His protégé, James S. Dwight, Jr., was appointed head of SRS soon afterward. They had served in Governor Reagan's administration in California as director and chief deputy director of finance, respectively, where they vigorously implemented cutbacks in welfare (Derthick, 1975, p. 104). In 1970, Weinberger served a short stint as chairman of the Federal Trade Commission and proved himself a competent administrator. Later that year, he moved to the Office of Management and Budget to become deputy director and then director. Weinberger had a role in the Nixon vetoes of HEW appropriations, which had been routine since 1969, and came to HEW with the sobriquet "Cap the Knife."

Although a good cabinet officer generally is presumed to be an advocate for the department's programs, Weinberger had a political

orientation considerably at odds with that usually associated with the activist disposition of HEW. Concerning the role of the department he led for more than two years, he lamented: "I am highly skeptical of the Federal government's ability to be a social engineer" (Weinberger, 1975, p. 22). In his last major speech as secretary, he decried the very programs that he administered, warning that expanding social programs threaten personal freedom and are likely to consume half the gross national product by the year 2000. "Should that day ever come, half of the American people will be working to support the other half" and "government would be like a gigantic sponge, sopping up all the nation's surplus capital needed for industrial growth and modernization" ([Toledo] *Blade*, 22 July 1975).

Dwight quickly gained his own sobriquet at HEW: "Attila the Hun." His efforts to implement a restrictive welfare policy at SRS were facilitated by his perceived proximity to power. As a bureaucrat in an HEW research unit expressed later:

Most of the people [in SRS] went along with Dwight. He was the sun-god. He was close to Weinberger and Weinberger was next to the president. He was getting support from Weinberger and the White House. He was carrying out the kind of program they wanted (interview, July 1977).

Once entrenched, the new administrators found a number of ways to make national welfare policy more restrictive.

#### Management Resources to Change Welfare

According to the structuralist view of organizations, top administrators have diverse resources for exercising direction and control. After 1971, in reshaping welfare, the Nixon administration most successfully utilized a new monitoring and evaluation system and reorganization of the Social and Rehabilitation Service, including regionalization.

**Monitoring and Evaluation.** The new monitoring and evaluation system provided the Nixon administration with its strongest tool to reshape welfare. At its heart was a "quality-control" system of reviewing case worker actions for certain types of errors while totally ignoring others.

Before the 1970s, administrative reviews of case-work actions toward welfare applicants and recipients were handled on a case-action basis, whereby officials examined a sample of all decisions taken by welfare employees, in-

cluding negative case actions (i.e., rejections of applications), for compliance with welfare laws and state regulations (as well as checking for the length of time required to determine eligibility, the time elapsed before the first payment, legality of terminating recipients, etc.). Quality-control reviews replaced these administrative reviews (which actually had fallen into disuse in the late 1960s).

The major impact of quality control depended on three developments in 1973: (1) establishment of tolerance limits (for maximal allowable error rates); (2) replacement of case-action surveys with a cross-section sample design; and (3) introduction of fiscal penalties.

Tolerance limits were set at 3 percent and 5 percent, respectively, for ineligibility and overpayment error rates for each of the states. Because of the complexity of welfare rules and regulations and the classification of small monetary discrepancies in payments as errors, all states far exceeded the tolerance levels in the first quality-control survey, with average error rates of 10.2 percent for ineligibility and 22.8 percent for overpayments (DHEW news release, 20 December 1973). Welfare workers considered the tolerance levels draconian, but knew they would succeed in forcing case workers in questionable situations to err by rejecting applications and making underpayments. The number of application denials and underpayments would inevitably climb.<sup>1</sup>

The switch from case-action to cross-section quality-control surveys plus the tolerance levels combined to intensify the pressure on intake

<sup>1</sup>With strong anti-welfare sentiment in some parts of the country, many professionals worried about the excessive zeal in certain states that the federal emphasis would produce. Ohio, predictably, succumbed to the contagion of punitive quality-control efforts in the spring of 1975 by embarking on a 100-percent sample. The quality-control review required all Ohio recipients to appear at the county welfare offices with verification of earnings and other information. When some recipients failed to appear—2,279 out of 10,787 interviews scheduled in Lucas county ([Toledo] *Blade*, 7 July 1975)—state officials had their evidence to assert that a high proportion of chiselers were on the welfare rolls. Reasonable voices questioned the wisdom of a 100-percent sample, feeling that it was designed to purge the rolls of cases rather than produce genuine administrative improvements in the program. Of course, many of the no-shows were accounted for by the high turnover of welfare recipients regardless of quality-control surveys, and the number of recipients who were too ill or who lacked transportation and child-care to enable them to go to the welfare offices.



workers to err on the restrictive side. With no examination of negative case action, quality control based on the cross section of active files provided no means to detect denials of eligible applicants. This insured that errors would become systemic and biased against those eligible for welfare. An official in a staff office of the HEW Secretary, who grudgingly admired the purposefulness of welfare policy makers, offered this analysis:

SRS officials are very perceptive; they have goals, which are clear, and they are going to get them carried out. There are basically two types of errors: non-eligibles on the rolls and eligibles refused access to the roles. What SRS wants to do—and is doing—is drive down the first type of error. Because there is a trade-off between the two types of errors, they are increasing the other kind of error. SRS officials are forcing their will on a bureaucracy that would prefer to go another route (interview, March 1975).

Worried about the eligible persons denied welfare, many of the welfare professionals sought approval for methods to examine negative case actions during quality-control surveys. SRS policy makers showed no interest in providing this approval until December, 1976, when they were pressured by a federal judge presiding over a case filed by the National Welfare Rights Organization (*Federal Register*, 22 December 1976, to amend 45 CFR 205.25).

Fiscal penalties were also used to bring about changes in welfare policy, beginning on December 20, 1973, when Weinberger announced his intention to impose them for high error rates. Historically, the federal government has been reluctant to use penalties because they hurt the recipients more than the state administrators guilty of malfeasance (Altmeyer, 1966). Consequently, threats of fiscal penalties have been viewed more as invitations to negotiate differences than as non-negotiable demands for states to capitulate to federal ultimatums. But the states sensed the determination of the administration and were energized by the large amounts of money at stake. An HEW news release (16 December 1976) shows disallowances amounting to 3.7 percent of the total federal participation for one six-month period. For Washington, D.C., threatened withholding exceeded 15 percent. Taking advantage of the strong position granted them by the federal system to influence national policy, the states chose the courts over the bargaining table. Eleven states joined in a lawsuit filed in the federal district court in Washington, D.C. to enjoin the department and secretary from enforcing the disallowances

(*State of Maryland et al. v. F. David Mathews et al.*, Civil Action No. 75-63). Concomitantly, other states filed similar suits in federal district courts located in their states.

In Washington, D.C., the court finally ruled (14 May 1976) that HEW had the power to withhold money on the basis of tolerance levels, but enjoined it from doing so based on its promulgated tolerance levels to the plaintive states because these levels were "framed in an arbitrary and capricious manner." The decision indicates that the court would have sustained the position of the Nixon administration had it shown less zeal to reduce the error rates and, hence, federal welfare expenditures. Secretary Mathews, who had replaced Caspar Weinberger in August 1975, was advised by his general counsel to appeal the decision (interview, December 1977), but the administration, under a new president, mellowed, and Mathews chose to work out more harmonious relations with the states.

Although the states finally received the full federal share of the AFDC expenditures, from 1973 through 1975 the threat of fiscal penalties greatly distressed state officials. A high HEW official reflected on the magnitude of the quality-control change:

Before this quality-control system, we had federal auditors in the states who would make disallowances for individual cases improperly receiving welfare benefits. The states didn't object to this, for the amounts disallowed on an individual case basis were very small. But with quality control, we went from a small sample and extrapolated the results to the entire universe, and made the disallowances on the total number of errors expected in the universe and not on the number of errors found. All in one fell swoop. The amounts of money involved in the disallowances were staggering (interview, December 1977).

**Reorganization.** Lay and academic views about the importance of reorganization are diametrically opposed. For citizen groups, which concentrate on reorganization when asked for suggestions to improve government, its importance is paramount. But many academicians conclude that only other factors, not structure, affect policy, and hence play down the importance of reorganization. Both the citizen and academic views are inaccurate—the first for its lack of understanding of other factors, and the second by its preoccupation with them. A reorganization which changes the underlying forces can affect policy. It is a powerful tool in

the hands of individuals knowledgeable about organizational dynamics.

The Social and Rehabilitation Agency underwent a great many organizational changes over the course of its 10-year life. In the Nixon administration four types of organizational change produced an increasingly restrictive welfare policy: (1) forcing the atrophy of the Social and Rehabilitation Service, (2) dismantling the Assistance Payments Administration, (3) expanding the non-programmatic staff offices in SRS and contracting the programmatic units, including APA, and (4) regionalization.

*Atrophy of SRS.* The creation of the Social and Rehabilitation Service in 1967 held out the promise of a focal point in HEW for all social welfare programs. The promise went unfulfilled. Over the years, reorganization ravaged SRS, leaving it an organizational weakling in social-welfare programming. The *coup de grace* came in March 1977, when it was reorganized out of existence (DHEW news release, 8 March 1977).

The heart of the welfare bureaucracy was found in the Assistance Payments Administration, which assumed responsibility for the categorical public-assistance programs, including Aid to Families with Dependent Children, Aid to the Blind (AB), Aid to the Permanently and Totally Disabled (APTD), Old Age Assistance (OAA), as well as services provided to recipients of these programs (except for those handled by the Children's Bureau, such as foster parent care).

After 1969, a series of organizational changes in the Social and Rehabilitation Service downgraded welfare by shrinking SRS, especially its national office. As one programmatic unit was added to SRS, three were removed. The Community Services Administration (CSA), came to SRS in September, 1969.<sup>2</sup> Shortly thereafter, the Children's Bureau shifted from

SRS to the new Office of Child Development in the Office of the Secretary of HEW. Removal of the Children's Bureau—even though a stodgy and unimaginative agency (Steiner, 1976, p. 37)—left SRS with less capability to coordinate welfare programs.

On April 1, 1973, an Office of Human Development, headed by an HEW assistant secretary, came into existence and immediately absorbed the Administration on Aging from SRS. The Rehabilitation Services Administration finally fell to the same office on February 2, 1975. Three programmatic agencies remained in SRS: the Assistance Payments Administration, the Medical Services Administration, and the Public Services Administration (formerly Community Services Administration). Employment in programmatic units at the national headquarters of the SRS dropped from 705 in 1969 to 279 in 1975, leaving SRS a hollow shell of its former self (unpublished data, Social and Rehabilitation Service).

*Dismantling APA.* Another way to reduce old-line bureaucratic influence in welfare policy was to dismantle and bypass the Assistance Payments Administration, which inherited most of the welfare responsibility from the Bureau of Family Services (BFS) in 1967. This began early, almost unwittingly, when the Community Services Administration was created as a unit of SRS in 1969. At that time, the social services function was separated from maintenance assistance and shifted from APA to CSA (HEW *Annual Report*, 1970, p. 92), thereby fragmenting the welfare system.

Ironically, for the cost-conscious, management-oriented Nixon administration, this separation fueled the subsequent explosion in the costs of services. While encouraging states to provide services, the old-line welfare professionals in BFS and later in APA insisted that federal funding be limited to case work which emphasized the interaction of the case worker with the client. The separation of services severed the old-line professional control and coordination of payments and services and allowed the social-services grants program to become distorted into a "gusher of federal funds" for all kinds of activities in states politically able to exploit certain loopholes (Derthick, 1975, pp. 4, 18–19). Tragically for many clients, this failure to stem the costs spurred the management-oriented administration to produce the promised cost reductions in all areas of welfare. Thus, a cost explosion, caused in part by a shift of responsibility away from welfare professionals, prompted the administration to take away even more of their

<sup>2</sup>The Community Services Administration in HEW is not to be confused with the better-known Community Services Administration created by the Head Start Economic Opportunity and Community Partnership Act of 1974 (88 Stat 2291) as a successor to the Office of Economic Opportunity. The existence of the two CSA's created untold confusion and HEW responded by changing its CSA to the Public Services Administration (41 Federal Register 1937, 13 January 1976). Many welfare professionals objected unsuccessfully to the new name because of its identification with state public service commissions which perform an entirely different function.

responsibility. The dismantling of APA continued.

Nixon officials planned to have the Social Security Administration administer the Family Assistance Plan (FAP), clearing the way for the elimination of most of the welfare bureaucracy, an important goal for President Nixon who welcomed the assurances of urban affairs counselor Daniel Patrick Moynihan that FAP would "wipe out" social workers (Burke and Burke, 1974, p. 99). During the period that FAP was under consideration, the APA languished. It had no involvement in the FAP planning, vacant positions went unfilled, and morale was low. While the welfare load increased nationally from under 5 million recipients in 1968 to 11 million in 1972, APA personnel in Washington dropped from 128 to 116. When FAP was finally laid to rest, hopes rose in APA for more staff and resources. Instead, the dismantling was accelerated. By 1975, the number of personnel was down to 89.

The most visible aspect of the dismantling came about in January, 1974, with the federalization of the adult categories (AB, APTD, and OAA) under the Supplemental Security Income (SSI) program. The welfare professionals in SRS lost a large portion of their work load to the insurance professionals in the Social Security Administration, with early estimates of 100 positions to be shifted from APA to SSA (U.S. House Appropriations Committee, FY 1974, Pt. 5, p. 578). When many of the problems predicted for SSA administration of the adult categories materialized (Mullis, 1975), those wanting the welfare program left in SRS felt vindicated.

Less visible, but of greater significance for overall welfare policy, was the early 1973 transfer of quality control from the Assistance Payments Administration to the Office of

Management, a unit attached to the SRS administrator (HEW, SRS, Office of Quality Control Management, 1973). This transfer has policy significance because operatives in APA, sensitive to the problem of applicants eligible for welfare but denied access to the rolls, wanted to improve quality-control methods to include examination of negative case actions. With APA not responsible for quality control, demands for review of negative case actions were ignored by appointive officials until December 1976, when, as I have said, they were prodded by a federal judge. As one APA official observed:

The goal of [SRS administrator] Mr. Dwight is to cut down on the errors picked up by quality control. In so doing, he is ignoring some other matters that are important, but APA has not been allowed the staff to pursue them (interview, March 1975).

*Expansion of Nonprogrammatic Staff.* Although Harold Seidman (1975, p. 57) notes that the Congress is "grudging in granting to department heads the resources which they believe are required to coordinate and manage effectively the programs for which they are responsible" such as additional staff, the fact is that the federal government staff units have increased faster than line units in recent years (Kaufman, 1976, p. 39). In SRS, staff providing assistance to the administrator proliferated while reorganizations and unfilled vacancies constricted the programmatic units, as the accompanying table shows.

From 1968 to 1974 in SRS, nonprogrammatic staff rocketed from 29.7 percent to 66.6 percent of total staff. (For 1975, the percentage dipped slightly, providing some evidence that, under President Ford, managerial emphasis in welfare had peaked.) In the regional offices, where the staffing figures are harder to

Table 1. SRS Staffing, 1968 through 1975 (July 1)

Year	Programmatic	Nonprogrammatic	Nonprogrammatic as Percent of Total SRS
1968	472	199	29.7
1969	705	406	36.5
1970	687	484	41.3
1971	649	505	43.8
1972	644	581	47.4
1973	482	574	54.4
1974	306	608	66.5
1975	279	480	63.2

Source: Based on unpublished data from the U.S. Department of Health, Education, and Welfare, Social and Rehabilitation Service.

assemble, the SRS policy at this time was also to install more managerial personnel at the expense of line staff, as indicated in 1973 during congressional testimony by the SRS acting administrator:

It has only been in the last year that we have attempted to put management type people rather than programmatic people in the regional offices. This has shown some good results (U.S. House Appropriations Committee, FY 1974, Pt. 5, p. 578).

At both the national and regional levels, a growing management staff monitored, analyzed, coordinated and otherwise controlled a diminishing SRS program.

As the SRS staff expanded, it absorbed many of the functions which had been performed in the programmatic units, such as public information and affairs, grants management, and most importantly, review and evaluation. As the functions went up to the fifth floor of HEW's Switzer Building in Washington (near the administrator's office), another significant change took place: the commissioners of the programmatic units moved to the fifth floor to be next to the administrator and further removed from the careerists. As one respondent recounted: "It used to be that the commissioner was on the fourth floor, surrounded by his people. After the move, it was hard to get to him" (interview, December 1978).

The increasing management staff in SRS strengthened the Nixon administration's control over policy by reducing the influence of the programmatic units which have a tradition of professionalism. With their employees having backgrounds in social welfare work and associated with the relevant professional organizations, the programmatic units have furthered professional goals and objectives. Although politically appointed, the heads of the SRS units also have been associated with the respective professions, a difficult-to-break tradition. For example, appointing a person with no experience in social welfare to be commissioner of the Assistance Payments Administration would generate considerable political opposition. Appointing the same person to a nonprogrammatic unit would pass unnoticed. Such was the case with Francis DeGeorge, named associate administrator for management in SRS in December, 1972. He came to the welfare programs with a Master of Business Administration degree, a Bachelor of Science degree in accounting, and work experience as a treasurer, vice-president, controller, and cost-accounting manager of several private firms, including divisions of Litton Industries (U.S. House Ap-

propriations Committee, FY 1974, Pt. 5, p. 269). The increase in staff units facilitated this type of appointment to SRS. Lacking ties and commitments to the social welfare profession, the army of management experts installed in these newly created, nonprogrammatic units, in response to administration desires for a more restrictive welfare policy, came to have a profound impact. DeGeorge played a crucial role in the welfare retrenchments during the height of the Nixon campaign to restrict welfare (Derthick, 1975, pp. 96-99).

In the creation of staff units in SRS and the de-emphasis on programmatic units, the Nixon administration selected an effective tool for changing welfare policy. Although the roots of the generalist, centralized control of welfare extend back into the Johnson administration with the creation of the Social and Rehabilitation Service, the Johnson administration neither consolidated control at the SRS level nor reversed the generally expansionist policy ingrained in the operations of the agency and its predecessors since the mid-1930s. Rather, the centralization of control and the reversal of the policy awaited the Nixon administration which adroitly used and further refined the organizational structure conveniently provided by the preceding administration.

*Regionalization.* The keynote of the Nixon administration's new federalism was "decentralization." In his 1971 State of the Union speech, using a language which became a familiar refrain, President Nixon declaimed:

The time has now come in America to reverse the flow of power and resources from the States and communities to Washington and start power and resources flowing back from Washington to the States and communities and, more important, to the people, all across America.

The time has come for a new partnership between the Federal Government and the States and localities—a partnership in which we entrust the States and localities with a larger share of the Nation's responsibilities, and in which we share our Federal revenues with them so they can meet those responsibilities (U.S. House of Representatives, 1971).

While Nixon was talking about decentralizing federal authority to the states and communities, he was instead regionalizing it. Proposals developed in the 1960s by the Bureau of the Budget (now the Office of Management and Budget) influenced his actions but not his words. As an agent of the president, the bureau has long sought some sort of presidential presence in the regions. Because Congress, for

obvious political reasons, will not allow Bureau of the Budget regional offices, it turned to other means in the mid-1960s when it perceived the need for better field coordination of federal programs following a rapid growth of grants-in-aid. The bureau developed plans for federal regional councils and the requisite common regional headquarters and boundaries for federal departments, as well as a proposal for granting regional directors more authority over programs in their own departments so that they would be in a stronger position to coordinate with other departments. But the Johnson administration limited implementation of these plans to modest experiments with federal councils in four cities (see Derthick, 1974, pp. 160-63).

In 1969, President Nixon accepted bureau proposals for the common regions and headquarters and the federal councils for all regions, but at least in HEW, the proposal for more authority to regional directors was largely ignored. As the management attitude of the Nixon administration came into focus near the end of the first term, however, the administration granted more authority to the regional directors much as the Bureau of the Budget had proposed to President Johnson years earlier. Despite the New Federalism rhetoric which called for decentralization of authority to the states and communities, this was regionalization. No increase occurred in participation by state and local welfare officials. Just as observers have noted the potential in regionalization for greater central control (Kaufman, 1969, p. 8), welfare programs became more subject to the influence of the Nixon administration. By the spring of 1974, HEW Secretary Weinberger openly acknowledged the redefinition of decentralization to mean regionalization—not a flow of authority to the states:

By "decentralization," we [HEW] mean a delegation of program authority from a program official in Washington, such as the Commissioner of Education, to his counterpart in a region, such as the Regional Commissioner of Education (U.S. House Appropriations Committee, FY 1975, Pt. 2, p. 71).

The result of the regionalization of welfare authority was not so much an increase in coordination (as intended by the bureau in the original proposal) as a shift of influence from Washington-based bureaucrats to regional ones, many of whom were recently hired and more receptive to the new welfare policy. In this way, the "decentralization" involved a managerial master-stroke which severed from influ-

ence in welfare decision making many Washington bureaucrats who for 35 years had pressed for increased and more progressive welfare programs. Secretary Weinberger unconsciously hinted at this shift of influence when he placated members of Congress who feared that "decentralization" would produce an extra layer of bureaucracy:

To the contrary, the purpose of decentralization is to remove a layer. Right now in most programs . . . the final decision has to be made in Washington. We are talking about putting that authority, final authority, for whatever function it may be—whether audit, technical assistance, or approval of a grant—in the regional office. . . (U.S. House Appropriations Committee, FY 1975, Pt. 2, p. 72, emphasis added).

As the secretary suggested, "decentralization" did "remove a layer"—a layer of Washington bureaucrats.

Prior to SRS regionalization, direct lines of control went from the central office units to regional counterparts. In welfare, officials in the regions involved in the administrative reviews of state programs reported to the Assistance Payments Administration. In 1969, even after the SRS regional commissioners were delegated additional program and administrative authority, the commissioners' involvement in SRS operations (as opposed to strictly administrative chores) was generally limited to "those areas of program operations involving two or more SRS programs" (HEW *Annual Report*, 1969, p. 323). In matters involving one program, guidance and control came from the relevant programmatic unit in the central SRS office.

Regionalization upgraded the role of the commissioners. Personnel in the region now reported directly to the commissioners, who deliberately severed their relationships with counterparts in the national headquarters. Regionalization changed the role of the commissioners from field representatives attempting to conciliate disputes among warring units to agents of the SRS administrator managing all SRS programs. With the administrator and regional commissioners as political appointees, the potential for presidential control of the welfare system increased.

To buttress the administrator's position further, the Nixon administration upgraded the Office of Field Operations (OFO), an SRS unit assigned staff functions but inevitably assuming line control. The administration required that all SRS communications between the regional and central offices be channeled through OFO.

Pressures on central office staff were sufficiently onerous that virtually all written communication and an overwhelming proportion of telephone communications were submitted to this prior clearance.<sup>3</sup> As a result, central office staff lost contact with the field, became less familiar with regional personnel, and were left out of the mainstream of welfare policy.

In the regions, a growing contempt for the Washington programmatic units seemed to have official sanction. One regional official said a report or recommendation goes directly to the SRS administrator: "It would get lost if we sent it to APA; we feed it right to SRS" (interview, June 1975). When asked about the value of APA headquarters staff, for assistance payments work in the region, a regional respondent reported that they are there in Washington "to help us to interpret the federal 'regs.'" But the official added, "We must make most of our decisions [about state plan compliance with federal rules and regulations] on our own; if it is not down in writing, then they are not much help" (interview, June 1975).

Although official spokespersons, for the record, dismissed variations in decisions among the regions following regionalization (U.S. House Appropriations Committee, FY 1975, Pt. 2, p. 72) despite contrary evidence (Derthick, 1975, p. 40), operatives voiced great concern. One administrator reported:

The ten regional offices don't make consistent decisions. We could have told them, when they were shifting responsibilities to the regional offices, that they would end up with inconsistent decisions (interview, March 1975).

These officials asserted the need, as one put it, for a "systematic mechanism for reviewing regional operations" (interview, March 1975). But the logical agency, the APA, lacked the authority and staff.

<sup>3</sup>Survey data confirming my argument that there was little communication between regional and central SRS staff can be gleaned from Jun's (1976) study of the SRS Operational Planning System (OPS, a pseudo-Management by Objectives system). His regional office respondents "said the central office staffs did not provide any feedback (or even call them)" about OPS reports. Jun appears surprised to learn that regional officers "in many cases did not reveal their own shortcomings to the central office" (1976, p. 50). Given the administrative effort to eliminate the influence of the welfare professionals in the central office, close observers knew of the paucity of communication between regional and central office counterparts.

During the FY 1975 budget hearings, Congressman Flood (D-Pa.) asked SRS Administrator Dwight if he planned to assign all 204 newly requested positions to the regional offices. Dwight's response evinced his lack of sympathy for the complaints from headquarters about staff shortages:

Yes. My view . . . is . . . the regions are understaffed and the headquarters may have recently been a little overstaffed. But I think that is largely a question of the past. By not filling vacancies here in Washington, we have pretty well eliminated whatever overstaffing there may be in headquarters (U.S. House Appropriations Committee, FY 1975, Pt. 6, p. 282).

The failure to strengthen the Washington office of the APA effectively deprived it of important functions until its abolition in 1977.

The ostensible decentralization employed in the Social and Rehabilitation Service has been an effective management tool to obtain policy results. Policy objectives of high-level SRS personnel clearly differed from those of the long-term career officials at lower SRS levels, including the operatives in APA. As one official close to the Secretary of HEW observed, "At the highest levels of SRS, an oppressive attitude about welfare is pervasive. Down lower in SRS there is not this oppressive, punitive attitude" (interview, March 1975). Regionalization silenced one of the significant voices of the federal-state welfare bureaucracy.

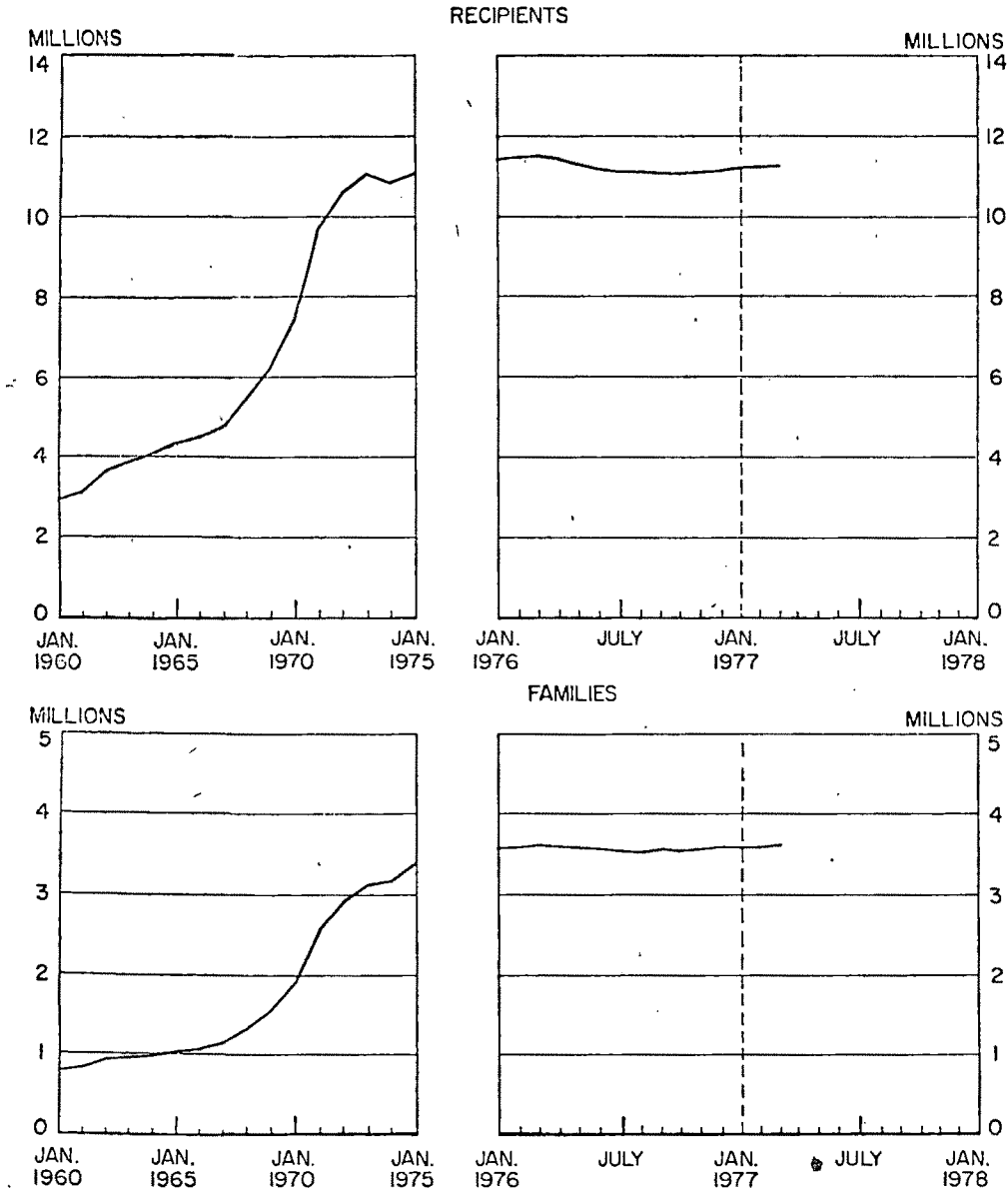
### The Policy Consequences

The overarching consequence for the welfare system of the Nixon administration was an increasingly restrictive welfare policy in the AFDC program. Management tools used by that administration had the direct effect of impeding access to the welfare rolls and the indirect effect of producing a deterioration in the federal-state relationships in welfare policy. While the former was the more visible, the latter may prove more insidious over the long run.

Compared to the states, the federal welfare establishment had long been an important force for inclusion of eligible persons on the welfare rolls. With the help of federally funded anti-poverty programs, the greatest gains came in the late 1960s and into the early years of the Nixon administration as the normally cautious state and local welfare systems acquiesced to pressures to enroll clients previously discouraged (Randall, 1976). The quality-control system and other Nixon administration mea-

asures were intended to stop the rapid expansion of the welfare rolls. They had an undeniable impact. After 1972, as Figure 1 shows, the AFDC rolls leveled off at about 11.5 million recipients, in spite of a poorly functioning economy which increased the pool of those eligible for welfare. Obviously, welfare is not

denied to the vast numbers of those eligible, as was true before the early 1960s. But, when eligibility criteria of each state were applied to the 1970 data files of the Current Population Survey of the U.S. Census Bureau, 22 percent of those eligible for AFDC were not receiving welfare; in the southern states, the exclusion



Source: U.S. Department of Health, Education, and Welfare, *Public Assistance Statistics* (October 1976 and March 1977).

Figure 1. Aid to Families with Dependent Children

rate rose to 34 percent (Boland, 1974, pp. 152–53). Considering the significant increase in the number of unemployed and the relatively stable welfare load since 1970, it would appear that participation rates decreased during the Nixon administration campaign to restrict welfare. Substantial numbers of welfare eligibles remain unenrolled who can legally claim benefits—a point of no contest to case workers who make eligibility determinations; they will confess to turning away people who probably are eligible.

When quality control shifted from the welfare professionals to the office of the SRS administrator, the means which states used to reduce their error rates became unimportant to the relevant federal officials, as long as the error rates diminished. An SRS official involved in quality-control development reflected the prevailing ideology of the Nixon SRS: “We are more interested in results than in how the states reduce the error rates” (interview, March 1975). This ideology stimulated changes injurious to welfare recipients and inconsistent with the traditional federal welfare posture. One official volunteered: “One state effectively cut down on error rates concerning diabetic diet allowances. They no longer allow special diabetic diet allowances” (interview, March 1975). Similarly, another state defined away the tubercular diet problem.

Using a methodology distrusted in many states and questioned by bureaucrats who used it (interviews, March 1975), Nixon appointees nevertheless claimed precise savings from the quality-control system. SRS Acting Administrator Don Wortman estimated “a direct saving since 1973 [to spring, 1976] of close to \$936 million as a result of the quality control program,” and that quality-control penalties against 43 states in 1977 would save \$220 million (U.S. House Appropriations Committee, FY 1977, Pt. 6, pp. 56, 31), or just under 2 percent of the federal AFDC expenditures of \$11.5 billion. An HEW press release later that year (16 December 1976) reported that 490,000 recipients or 175,000 families were “removed” from the welfare rolls because of the quality-control campaign.

Whatever the significance of the quality-control savings, the emphasis clearly displaced other actions. Planning and development in welfare within the Social and Rehabilitation Service count among the casualties. Although former SRS Administrator Dwight once asserted that “our strategy is to reshape the central SRS office into a true national policy and planning agency” (U.S. House Appropria-

tions Committee, FY 1975, Pt. 6, p. 73), few in SRS and no one in APA could point to any significant national policy development and planning, aside from quality control, which had consumed much time in SRS units. Two years later, after pressing the SRS acting director to admit to little SRS involvement in the development of welfare reform proposals then under consideration, Congressman Robert Michel (R-Ill.) questioned the advisability of ignoring experts: “I don’t know how you can ever just out of thin air dream up a reform proposal without going to those of your people who work directly on a day-to-day basis with the thing” (U.S. House Appropriations Committee, FY 1977, Pt. 6, pp. 37–38).

Another consequence of the Nixon administration welfare policy was the end of federal encouragement of the states to adopt AFDC-U, the program supplement to AFDC which allows families headed by unemployed fathers to be eligible for welfare. The putative concern of the Nixon administration to keep families together, which was one of the driving forces behind FAP (Moynihan, 1973), could have manifested itself after the demise of FAP in a campaign to extend AFDC-U to the 26 states without it. That, however, would have increased welfare rolls. Consequently the welfare system continued in most states to foster family breakup by requiring that there be no father in the home.

Over the long run, the deterioration of the relationship between federal and state welfare officials may prove to be the more debilitating blow to good administration. From the beginnings of the joint federal-state welfare system through the 1960s, “partnership” fairly characterized the relationship between federal and state officials, with federal officials pressuring for professionalization and expansion of the state programs. For 35 years, the federal welfare establishment provided guidance and encouragement to improve methods of welfare delivery, to design better organizational structures, to upgrade welfare personnel, and to enroll eligible people.

Derthick cites the federal actions which led to changes in Massachusetts. Federal pressure was generally subtle, as she observes:

Federal influence, in summary, operates mainly through the agency that receives grants, and with the agency’s self-serving cooperation. It operates by enhancing the role that the agency plays in the state political system and by shaping the agency’s values, goals, interests and actions. It operates primarily on the structures and processes of policymaking and administra-



tion rather than directly on the substance of policy. This may be a critical limitation, but if the influence on structures and processes is extensive and enduring enough, the result must be to influence policy outcomes as well—all policy outcomes, not just those in which the federal government is actively interested (Derthick, 1970, p. 207).

In one specific example, Derthick (1970, pp. 75–97) shows how Massachusetts gradually shifted to increased state control of local operations until finally, in 1967, the system was formally changed to a state-administered one, largely because of indirect federal pressure. Of course, there has always been official silence on statewide administration at the federal level, but in the 1970s pressure was no longer put on states to move in that direction. According to one well-placed SRS official:

You will not find an official SRS position on state-administered versus state-supervised systems. Some of us have our private opinions, but there is no official view. I don't think there is any pressure at all on the states to change to a state-administered system (interview, March 1975).

Recent research supports the widespread impression that state-administered systems are more professional and expansionist (Randall, 1976).

Governors, generally uninterested in the details of welfare (e.g., Weinberg, 1977, pp. 140–42), were united in opposition to many Nixon administration welfare actions. For example, Democratic and Republican governors joined in an appeal to the Congress to write the social services law in such detail that the Nixon administration would be unable to deny funding for social services carried out by the state agencies. States were discovering, after billing the federal government for its share of a given social service, that the Department of Health, Education and Welfare no longer considered it as such and thereby denied funding. In his capacity as chair of the Governors' Conference, Governor Daniel J. Evans (R-Wash.) testified that the states no longer trusted HEW and wanted specific definitions of the social services written into the law—rather than leaving the issue to the regulations written by HEW, even though he thought that under other circumstances this “would be unnecessary and undesirable” (U.S. Senate, 1973, p. 12).

Finally, the recent emphasis in federal welfare policy has greatly reduced the once-prevalent technical assistance to state and local welfare operations. After 1973, regional officials explained that technical assistance was one

of their major responsibilities. As an example of technical assistance, one regional assistance payments official discoursed on “the help they can provide states in preparing their corrective-action plans to improve the quality-control results.” When asked for a specific example of help on a state corrective plan, she said: “Well, most states don't ask for much help.” Of the state for which she was responsible, she added: “They haven't asked for help, either” (interview, June 1975).

The view from the state level does not change the impression of a breakdown in federal technical assistance. Regarding help on corrective-action plans, a high official in Ohio welfare said: “We get no technical assistance from the regional office. On the corrective-action plan, we go to the other states for help and to see what they are doing” (interview, August 1975).

Concerning potential help from the regional office, a state official with lengthy experience in two different regions observed that until 1970 or 1971

there were knowledgeable people in the regional office. Now they seem to have all new people who don't know the law or the regulations, or very much about welfare. We see a lot more use of management consultant firms (interview, August 1975).

Isolating the welfare professionals and assigning additional responsibilities to the new management-oriented employees assured a federal-state relationship distasteful to many. In pleading for a return to the old relationship, an old-line Washington bureaucrat said:

You have to understand that public-assistance program has traditionally been a joint program. It has operated as a joint program. The feds have not in the past been sneaking around spying on the states. We have worked together (interview, March 1975).

A state official succinctly characterized the change in the federal-state relationship: “From help and assistance to an adversary role” (interview, August 1975). Some SRS policy makers publicly agreed with this assessment, including SRS Acting Deputy Commissioner Mike Suzuki (1975, p. 12) who summed up social services during this period thus: “Rather than two levels of government brought together in a common enterprise, the federal-state relationship became that of adversaries.” Behind this deterioration of federal-state relations is the control exercised through the management techniques of the Nixon administration. States were under the gun to make their programs

more restrictive; the management-oriented officials in Washington and the regions held the gun.

### Summary and Conclusions

Many students of the presidency stress the obstacles to presidential control of the bureaucracy, basing their findings on the recollections by presidents, their aides and bureaucrats of presidential-bureaucratic conflict. This research approach runs the risk of letting the respondents write the conclusions. If one shifts from questions about presidential-bureaucratic relations in the abstract to analyses of the management techniques used by presidential administrations in specific policy areas, one's perspective is likely to change. Presidents do enjoy policy successes in the face of bureaucratic opposition, as did the Nixon administration in the area of welfare. When presidents fail to influence policies, it is not usually for lack of administrative tools. Rather, the explanation may be a lack of time, lack of political or personal interest, or lack of understanding of the bureaucracy and management tools at hand. Perhaps because recent presidents generally have concluded that they can best secure a prominent place in history with an impressive legislative box score, matters of management have too often been shunned (Seidman, 1975, pp. 71-73). Moreover, most incoming presidents are inexperienced in managing government programs and must learn on the job.

During the first two years of his administration, President Nixon emphasized a legislative over a managerial strategy to achieve policy goals. Faced with few legislative achievements, and concluding that "operations is policy" (Nathan, 1975, p. 62), he later switched to a managerial strategy to change policy, characterized by Nathan (1975, p. 8) as the "Administrative Presidency." A new set of management-oriented leaders replaced another set which had been implementing a traditional federal welfare policy distasteful to the administration. These new leaders wasted little time in employing management techniques to turn welfare policy in a restrictive, sometimes punitive, direction. They scuttled the old-line units and turned to more responsive ones, often newly created in the national and regional offices. They forced the atrophy of the Social and Rehabilitation Service and they dismantled the Assistance Payments Administration. Management units with no programmatic experience were given programmatic responsibilities. They developed a monitoring system which forced

case workers to become more restrictive in determining eligibility and payment levels. They refined that system to exclude from consideration the concerns of old-line professionals. They reduced the technical assistance to the states. They turned from helping hapless individuals to constricting the system.

Before generalizing from this case study, we must note factors peculiar to the welfare system which enhanced presidential control. First, the AFDC program does not enjoy widespread support among the general population. Efforts to constrict the system are aided by this low legitimacy. Second, the beneficiaries of the program are poor, not well educated, and lacking in political efficacy. They are not effective defenders of their interests, a factor which also buttressed the administration's position. A third factor which might appear to have aided the Nixon administration was its desire for a retrenchment rather than an expansion of the program. However, while retrenchment might normally be easier to achieve through presidential management efforts, a mandated program like welfare presents a special case. Because the open-ended appropriations of a mandated program automatically provide whatever amount of money is necessary, retrenchment and expansion would be about equally difficult.

On balance, the conditions peculiar to welfare should strengthen the president's managerial hand in this program as compared to others. But there remains the possibility that presidential management efforts can be handled more adroitly, offsetting in some degree the management difficulties characterizing other program areas. For example, welfare changes sought by the Nixon administration were limited by its excessive zeal in its financial dealings with the states and by Nixon's failure to serve an entire second term.

The experience of the Nixon administration with the welfare bureaucracy suggests how a president can short-circuit the legislature and put a personal policy stamp on the programs administered by the bureaucracy free from the glare of publicity, congressional control and the awareness of the general public. Yet the Nixon administration enjoyed greater success over the federal welfare bureaucracy and its policy than it did over welfare outcomes—the policy at the operating level experienced by those eligible for or claiming welfare. In obvious and not-so-obvious ways, federalism complicates efforts to control welfare policy. Because state welfare systems have always maintained considerable autonomy, control over the federal welfare

bureaucracy does not translate into a similar degree of control over the state systems. Rather, control over, or neutralization of, the former must precede control over the latter. In this sense, Nixon's mid-term resignation robbed his administration of the time needed to develop more extensive control over the state welfare systems. In fact, almost immediately after his resignation, the pressures on both federal welfare officials and the states abated. In addition, many state welfare systems were generally closer in ideology to Nixon than they were to the federal bureaucrats. In part, then, administration gains over the federal bureaucracy simply relaxed pressure on many states to do what neither they nor the Nixon administration wanted done.

But an additional and more subtle factor involving federalism is necessary to explain why changes in welfare outcomes did not match the changes in the federal welfare policy. Although the deterioration in federal-state relations worked to the advantage of the Nixon administration policy preferences, the federal system has recently undergone other changes which inhibit presidential control of domestic policy. The states themselves—through the actions of governors and other top officials concerned with overall state budgets—are becoming a different kind of element in the federal system as their fiscal solvency depends increasingly on federal dollars. Where the financial stakes are great, states have been transformed into a separate clientele of the federal government, so that they act more as powerful claimants on the federal treasury (much like many other powerful interest groups) than as partners of the federal government administering a joint federal-state program. Recall two Nixon administration ventures to reduce the flow of federal welfare funds to the states. Successful opposition came from the states; that the interests of the states, of the federal welfare bureaucracy and of welfare recipients merged was fortuitous for the latter two. First, Democratic and Republican governors descended on Congress in 1973, forcing the Nixon administration to retreat from its efforts to redefine many social services out of existence (and thereby reduce the flow of federal dollars to the states). Second, the fiscal penalties that the Nixon administration planned to impose for high error rates uncovered by the quality-control system unified the states in successful opposition through the courts.

Where the financial stakes are not so great, the states' political involvement in policy and program administration will recede, more typi-

cal federal-state administrative patterns will obtain, and governors will be relatively indifferent to presidential efforts to control domestic policy. Where financial stakes are large, however, the states become involved as a powerful class of actors both more removed from presidential control than the federal bureaucrats and with greater access to the Congress. Moreover, members of Congress have become more receptive to the pleas of the states (and other constituents) in recent years as "a lesser proportion of congressional effort is now going into programmatic activities and a greater proportion into pork-barrel and case-work activities" (Fiorina, 1977, p. 46).

President Nixon's domination of the federal welfare bureaucracy and the considerable changes he wrought in welfare policy and outcomes raise important questions about the limits to presidential control of policy. If there are limits, they are ambiguous and variable. Laws usually fail to delineate them clearly. They are reduced when a president seeks retrenchment of a program beneficial to a politically weak clientele, or perhaps when seeking expansion of a program benefiting a powerful clientele. In this study, limits seemed to emerge as presidential actions drew the states into the fray. In American politics, important issues are rarely settled permanently. Opposition from other power centers like the states not only prevents a presidential initiative from being implemented in the manner intended, but also means that changes made by a presidential administration will never be irreversible. Before taking too much comfort in this kind of conclusion, however, consider that the limits noted in this case study could have been forced back even further and welfare outcomes changed more dramatically had the Nixon administration pursued a more moderate strategy with the states and had it survived another two years. Hence, the greater the president's managerial astuteness and determination to control, the further removed are the limits. Clearly, the power over the bureaucracy and control over policy is subject to abuse, which makes the study and surveillance of presidential management efforts so important.

Clearly, management techniques should be used in any paradigm of presidential policy development and implementation. Obviously the specific techniques found important in this case study of welfare will not necessarily be important in other areas. However, analyses of other policy areas in which the national government has a significant involvement should begin with the expectation that there are great

resources available to the administratively astute president disposed to use them.

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# Generational Replacement and the Growth of Incumbent Reelection Margins in the U.S. House\*

RICHARD BORN  
Vassar College

*Virtually all congressional scholars investigating the rise of incumbent safety in the U.S. House have assumed that the responsible cause, regardless of its specific nature, is one which has affected incumbents generally. The sole exception is Fiorina, who speculates that increased overall safety results from recent freshmen's greater electoral strength. The analysis performed here confirms this generational replacement hypothesis. Much greater vote gains have resulted from the freshman term of incumbency since 1966-68, while no pro-incumbent trend for veterans has surfaced.*

**Incumbent, n.** A person of the liveliest interest to the outcumbents.

—Ambrose Bierce, *The Devil's Dictionary*

Students of congressional elections, like the "outcumbents" who must wage ever more Sisyphean campaigns, have demonstrated the "liveliest interest" in the growing electoral value of incumbency. Alternative explanations for the phenomenon have focused on redistricting following the 1964 *Wesberry v. Sanders* ruling (Tufte, 1973, pp. 551-53; 1974), expanded use of perquisites like the franking privilege (Mayhew, 1974, p. 310; Cover, 1977, pp. 536-39; Cover and Mayhew, 1977, pp. 65-66), increased case-work services (Fiorina, 1977a; 1977b, pp. 41-49), direction of additional federal funds into the district (Fiorina, 1977a; 1977b, pp. 41-49; Mayhew, 1974, p. 311), more skillful "position-taking" (Mayhew, 1974, p. 311), and the decline of partisan voting cues (Ferejohn, 1977, pp. 172-75; Erikson, 1972, p. 1240; Burnham, 1974; Mayhew, 1974, p. 311; Cover and Mayhew, 1977, pp. 66-68; and Nelson, 1978-79, pp. 665-78).

The all-but-unanimous assumption of these scholars, however, is that whatever the cause of greater reelection margins, it has had a general impact upon incumbents. Fiorina alone speculates, without attempting an empirical test, that the observed increase in safety actually results from the greater electoral strength of freshmen entering Congress since the mid-60s (1977b, pp. 54-55). This study will confirm Fiorina's generational replacement hypothesis by demonstrating that House cohorts elected since 1966

have achieved more electoral value from their incumbencies than those they replaced, while members of the pre-1966 generation have not benefited from any general pro-incumbent swing.

## Why Has Incumbent Safety Risen?

Results for the 13 congressional elections from 1952-76 will be investigated here. Incumbents running in multi-member districts, as well as those elected on an at-large statewide basis, are excluded from the calculations. On the other hand, the analysis will include districts subjected to regular decennial redistricting or to boundary adjustments necessitated by the 1964 *Wesberry v. Sanders* decision. Ample evidence has been presented by Ferejohn (1977, pp. 167-68), Cover (1977, pp. 528-31), Bullock (1975), Mayhew (1974, pp. 303-06), and Erikson (1972, p. 1241) demonstrating that the electoral effects of both kinds of redistricting have been insignificant.

Previous studies have chosen to quantify the electoral safety of incumbents in terms of the percentage of reelection bids falling within marginal vote categories. Mayhew (1974, pp. 303-04), for example, finds that regardless of whether a 55 or 60 percent cut-off point is established, the number of marginal incumbents has fallen by about half from 1956 to 1972. A somewhat less dramatic decline in marginality has been discovered by Tufte (1974), who analyzes incumbent safety by computing the percentage receiving less than 53 percent of the vote. While the marginality measure does indicate the dwindling number of districts with incumbents most vulnerable to defeat, it still ignores possible shifts among districts regularly outside the marginal range. It cannot reveal whether the movement of marginal districts into safer categories has, for instance, been

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counterbalanced by a reduction in the number of seats which formerly were won by extremely lopsided majorities. Furthermore, if many incumbent seats normally cluster around the cut-off point employed, modest swings of the national vote can produce significant fluctuations in the percentages of incumbents judged to be marginal.<sup>1</sup> The measure has limited utility, therefore, as a summary indicator of incumbent safety.

Electoral safety here will be quantified more comprehensively as the incumbent's margin relative to the mean vote received by all opposed congressional candidates of his or her party. This correction is needed to control for changes in safety which can be produced by national electoral swings when there is substantial disparity between the numbers of incumbent Republicans and Democrats. Consider, for example, the effect of a 5 percent pro-Republican inter-election swing upon the margins of 160 Democrats and 90 Republicans seeking reelection in both elections. In the absence of any real increase in the electoral advantage of incumbency, the average margin nonetheless would appear to change by

$$\frac{160(-5\%) + 90(5\%)}{250},$$

or -1.4 percent. We thus remove any biasing effects of national swings by subtracting from the opposed incumbents' percentage of the two-party vote the mean share of the contested two-party vote received by all incumbent and non-incumbent congressional candidates of their party that year.<sup>2</sup> (All election data are taken from the biennial editions of *American Votes*, edited by Richard M. Scammon.)

The last column in Table 1 indicates the average magnitudes of these corrected reelec-

tion margins from 1952-76. Clearly, the data fall into two distinct periods: 1952-64, and 1966-76. The incumbent margins for the latter period are higher than any of the margins for the years 1952 to 1964. From 1952-64, the average incumbent safety level is .114, and there is no discernible trend; the equation for safety ( $Y$ ) regressed against time ( $t$ ) has zero slope ( $Y = .113 + .000t$ ,  $r = .095$ ). Margins from 1966-76, averaging .144, increase slightly over time ( $Y = .137 + .002t$ ,  $r = .412$ ). The two most recent averages for 1974 and 1976, however, represent declines from the peak of incumbent safety attained in 1972.

Except for Fiorina, researchers who previously have uncovered increases in safety using their own indicators have attributed this development to a general pro-incumbent trend. Underlying their belief is the assumption that any changes in the overall electoral value of incumbency will be manifested approximately to the same degree across seniority classes. This is evident in Cover's recent use (1977, p. 527) of the so-called "sophomore surge" measure—the average vote gain of freshmen between their initial victories and first reelection tries—to quantify the electoral advantage enjoyed by incumbents; increases in "sophomore surge" values are assumed to parallel similar safety improvements among veteran members.

The only data actually bearing on this assumption, however, have been provided by Erikson in an analysis of congressional elections from 1960-70. Separate calculations of the "sophomore surge" are not presented; instead, Erikson chooses to quantify the incumbency advantage with a hybrid measure based upon a weighted average of the "sophomore surge" and the loss experienced by a party when its incumbent retires. Still, since freshman reelection bids in most years outnumber incumbent retirements, we would expect the values of Erikson's measure to approximate those of the "sophomore surge." Discovering that a 1964-66 increase from about 2 to 5 percent in the measure is matched by a similar 3-percent rise in the margins of veteran incumbents, Erikson states that "the electoral advantage from being an incumbent simply increased

<sup>1</sup>Similarly, Burnham (1975, p. 424) shows that substantial inter-election swings, like the aggregate 6-percent national shift to the Democrats from 1972-74, can produce very large changes in the percentage of marginal incumbents. Because of the Republican decline over these two years, the number of all northern and western incumbents receiving less than 60 percent of the vote jumped by fully 11 percent.

<sup>2</sup>In order to compute national interelection partisan swings, Tufte (1973), Mayhew (1974a), Cover (1977), and Cover and Mayhew (1977) measure a party's national strength as the percentage of the aggregate vote received by all its congressional candidates, regardless of whether they run unopposed. This would seem to overstate somewhat the strength of the

Democratic party in its most successful years, since the number of its incumbents running unopposed in these elections is higher than normal. Hence, we rely here, as well as in later calculations of national inter-election swings, upon the mean party vote in contested districts only.

suddenly with the 1966 election" (1972, p. 1240).

However, any conclusion drawn from this finding that the electoral movements of freshmen and veterans more generally are driven by a single incumbency advantage factor must be qualified by the fact that Erikson's analysis excludes the substantial number of districts which either are redistricted or located within southern and border states. In the 1964-66 analysis, for example, he is limited to including only about half (122) the total number of veteran incumbents (245) who actually faced major party challengers in both elections. But even if attention is confined to Erikson's limited data set, there is reason to wonder whether the parallel between the 1964-66 changes of his measure and the reelection margins of veterans may itself be only a temporary aberration. His own findings for the 1966-68 and 1968-70 election pairs show that while northern veterans continue to pick up an additional 1.6 percent over these four years, the measure still remains at its 1966 value of only 5 percent (Erikson, 1972, p. 1240).

The assumption by researchers that the greater safety of seats since the mid-60s results from a general increase over all seniority classes,

probably spawned in large part by Erikson's early work, thus remains to be confirmed. In contrast to this historical change hypothesis, Fiorina's generational replacement explanation suggests that members of Congress elected since the mid-60s simply may have derived more electoral mileage from their incumbencies than more senior members. If generational replacement is, in fact, responsible for the rise in safety, the average margin of Congress members in the aggregate should increase when the first class of the new generation makes its initial reelection bid, and should then continue to grow as successive cohorts replace members of the old generation in future elections. We shall test these two hypotheses across the complete cross-section of congressional districts by examining the inter-election movements of freshmen and veteran incumbents facing major party opponents in both elections, relative to the average biennial swing in all U.S. districts which have major party competition both times.<sup>3</sup>

<sup>3</sup>As above, all electoral percentages are computed on the basis of the two-party vote. Members who initially win their seats in special elections held within one year of the next general election are included with those first winning their seats in that general election when freshman inter-election swings are calculated.

Table 1. Electoral Safety of Incumbents with Major Party Opposition

Year	Number of Incumbents Running	National Democratic Mean	Average Incumbent Margin	Average Incumbent Margin Relative to National Party Mean
1952	286	.480	.622	.119
1954	313	.511	.606	.109
1956	325	.499	.608	.108
1958	285	.550	.615	.121
1960	313	.534	.615	.109
1962	315	.520	.621	.120
1964	340	.570	.619	.114
1966	340	.502	.629	.128
1968	347	.508	.643	.143
1970	324	.531	.655	.154
1972	319	.518	.656	.154
1974	316	.572	.642	.143
1976	330	.563	.659	.143
Average Incumbent Safety, 1952-64			.615	.114
Average Incumbent Safety, 1966-76			.647	.144

Source: Based on data from Richard M. Scammon (1956-1977), *America Votes*, Vols. 1-12, New York: Macmillan; Pittsburgh: University of Pittsburgh Press; Washington, D.C.: Congressional Quarterly.

Key: All margins are calculated as percentages of the 2-party vote. National party means are the mean party percentages of the 2-party vote in all contested open and incumbent-held seats. Average incumbent safety for 1960-64 and 1966-76 has been computed by weighting yearly averages by number of incumbents running.

These data, presented in Table 2, reveal that freshmen from 1952-54 to 1962-64 gain an average .015 of the vote between their initial victory and first reelection. Veteran margins over these years do not change. Between 1964 and 1966, there is a sudden rise in the electoral strength of all incumbents; the average margins of both seniority groups increase substantially beyond the mean values recorded previously.

The veteran surge, however, is dissipated over the next five elections. After a .007 drop from 1966-68 and a .013 increase from 1968-70, veteran margins then decline in each of the three remaining election pairs. By 1976, in fact, the net .025 electoral loss of all non-freshmen since 1966-68 has more than offset the abrupt .021 gain of 1964-66. In contrast, freshman inter-election gains rise spectacularly to .072 from 1966-68 and then continue through 1974-76 at levels considerably higher than those attained by pre-1966 freshman cohorts.

Across the 1952-54 to 1964-66 period, the assumption by previous researchers, that a single incumbency advantage factor similarly affects all seniority classes, seems well founded. Shifts in the electoral payoff of the first term tend to covary with shifts in the margins of more senior members; the Pearson correlation between changes in successive "sophomore surge" values and veteran inter-election vote

changes is a moderately strong .456. From 1966-68 to 1974-76, however, the corresponding coefficient is only .075, indicating almost complete independence between the electoral movements of freshmen and veterans.

The sizeable gains of first-termers from 1966-68 to 1974-76, averaging .066, are thus produced by factors which are unique to cohorts elected since 1966, rather than the result of a general electoral increase across all seniority classes. While a correspondence between the vote changes of freshmen and veteran incumbents does exist until 1964-66, it disappears thereafter as freshmen continue to achieve enhanced electoral gains which are independent of veteran performance. The general incumbent surge of 1964-66, emphasized by Erikson in his analysis of northern elections, is revealed here to have only transitory effects nationally which are cancelled by the veteran declines of succeeding years. Were it not for the ability of the new-generation freshmen to reap greater electoral rewards from their first term, the increases in incumbent safety demonstrated in Table 1 could not have occurred.

#### Generational Replacement: What Electoral Difference Does It Make?

While it is apparent that generational re-

Table 2. Inter-Election Vote Shifts of Freshmen and Veteran Incumbents,  
Relative to National Party Swing

	Freshmen	Veterans
1952-54	.014 ( 67)	-.004 ( 225)
1954-56	.019 ( 45)	-.007 ( 254)
1956-58	.015 ( 42)	.019 ( 237)
1958-60	.000 ( 76)	-.012 ( 209)
1960-62	.025 ( 56)	.006 ( 227)
1962-64	.023 ( 54)	-.005 ( 256)
1964-66	.035 ( 76)	.021 ( 245)
1966-68	.072 ( 66)	-.007 ( 253)
1968-70	.064 ( 33)	.013 ( 273)
1970-72	.076 ( 50)	-.006 ( 246)
1972-74	.058 ( 60)	-.023 ( 230)
1974-76	.062 ( 92)	-.002 ( 212)
Average 1952-54 to 1962-64	.015 (340)	.000 (1408)
Average 1964-66 to 1974-76	.060 (377)	.000 (1459)

Source: Based on data from Richard M. Scammon (1956-1977), *America Votes*, Vols. 1-12, New York: Macmillan; Pittsburgh: University of Pittsburgh Press; Washington, D.C.: Congressional Quarterly.

Key: National party swings are calculated in terms of mean party shift in all open and incumbent-held seats with 2-party competition in both years of election pair. Numbers of incumbents with major party opposition in both years of each election pair are in parentheses. Averages for 1952-54 to 1962-64 and 1964-66 to 1974-76 have been computed by weighting biennial shifts by number of incumbents running.



placement has been responsible for the greater safety of incumbent seats, we have yet to specify its exact impact in each election year. What levels of safety would have occurred in 1968 and afterwards, were it not for the greater electoral strength of new-generation cohorts? The influence of each new freshman class in raising overall incumbent margins will depend not only on the magnitude of its first-term gains, of course, but also on the number of its members. Likewise, the overall effect of inter-election movements by veteran incumbents opposed in both elections of the pair will depend upon the numbers involved. Both of these influences will be measured below, as well as the impact of a third, less obvious factor—biennial shifts of incumbents between unopposed and opposed election status. A Congress member running unopposed is excluded from the calculation of incumbent safety we have performed above for that year, but is included in the computation for a second election if major party opposition surfaces at that time. Since the two-party percentage of

the vote attained by such a member in the second election is likely to be very high, the incumbent average will be raised. The technique used here to decompose biennial changes in incumbent safety into the three components is explained in the appendix.

Table 3 presents the values of these components for each election pair.<sup>4</sup> Over the 1952–54 to 1962–64 period of incumbent margin stability, the mean electoral impact of each influence is fairly negligible. From

<sup>4</sup>There are two other minor sources of change for some of the election pairs: cases where a freshman who defeats an incumbent decides not to run again two years later, and cases where incumbents move from election in multi-member districts (which are excluded from the calculations of safety performed here) to election in single-member districts (e.g., the two New Mexico Democrats who shifted from at-large contests in 1966 to elections in separate districts in 1968). Both of these changes are of very low magnitude since they involve only a handful of cases, and they accordingly will be excluded from Table 3.

Table 3. Decomposition of Inter-Election Changes in Incumbent Safety

	Veteran Vote Shift Impact	Generational Replacement Impact	Unopposed- Opposed Electoral Transition Impact	Net Change Due to All Impacts	Incumbent Safety in Latter Year of Each Electoral Pair from 1966–68 to 1974–76 if Generational Replacement Impact Remained at 1952–54 to 1962–64 Mean
1952–54	-.004	-.001	-.006	-.010	—
1954–56	-.006	.003	.002	-.001	—
1956–58	.018	.004	-.010	.013	—
1958–60	-.010	-.010	.008	-.013	—
1960–62	.006	.009	-.005	.011	—
1962–64	-.005	.003	-.002	-.005	—
1964–66	.018	-.004	-.001	.014	—
1966–68	-.006	.023	-.001	.015	.122 (.143)*
1968–70	.012	.000	-.003	.011	.135 (.154)
1970–72	-.005	.007	-.002	.000	.129 (.154)
1972–74	-.023	.011	-.001	-.011	.108 (.143)
1974–76	-.001	.000	.001	-.001	.109 (.143)
Average 1952–54 to 1962–64	.000	.001	-.002	-.001	—
Average 1964–66 to 1974–76	-.001	.006	-.001	.005	.120 (.147)

Source: Based on data from Richard M. Scammon (1956–77), *America Votes*, Vols. 1–12, New York: Macmillan; Pittsburgh: University of Pittsburgh Press; Washington, D.C.: Congressional Quarterly.

Key: All figures are based on incumbents' percentage of two-party vote, relative to national party mean. Additional minor impacts are excluded from the table. 1966–68 to 1974–76 incumbent safety averages in the last column have been computed by weighting safety levels for latter year of each election pair by number of incumbents running.

\*Actual safety levels.



1964–66, however, it is clear that the sharp increase in overall safety which we noted above is entirely due to the sizeable .018 value of the veteran incumbent component. The generational replacement term at the same time actually is well below its previous .001 mean because so many of the freshmen in the 1966 election are switched-seat Democrats receiving lower margins than those normally enjoyed by incumbents of their party.

Subsequent improvements in safety after 1966 result almost entirely from substantial growth in the magnitude of the generational replacement component, which exerts a cumulative .041 impact on incumbent margins over the last five election pairs.<sup>5</sup> Generational replacement, though, fails to have a positive effect in 1970, when only 38 freshmen—the fewest in any of the elections considered here—run for reelection, and in 1976, when, as in 1966, a sizeable number of switched-seat, first-term Democrats attain relatively low reelection margins. The veteran incumbent component of change from 1966–68 to 1974–76 acts to depress overall safety, having a cumulative -.023 impact, while the component for incumbent transitions between uncontested and contested election status continues to exert only a slight influence.

Further highlighting the critical role of generational replacement since 1966–68, the last column of Table 3 indicates the average incumbent margin which would have resulted in the latter year of each pair if the new freshman cohorts had been no more electorally proficient than the old. Here we have substituted for the actual values of the generational replacement components the mean .001 value recorded over the 1952–54 to 1962–64 baseline period. The consequent .120 average incumbent margin which would have materialized over the five elections from 1968 to 1976 is fully .027 below the actual average and only modestly higher than the .114 average which existed over the 1952–64 period of unchanged incumbent margins. It seems unlikely that this slight additional safety which would have occurred without enhanced generational replacement effects could have stimulated much analytical concern among congressional researchers.

<sup>5</sup>The percentage of all freshman reelection bids involving those who have replaced members initially elected in 1966 or later remains low until 1976—8.1 percent in 1970, 28.8 percent in 1972, 20 percent in 1974, and 54.8 percent in 1976.

## Conclusion

This study has confirmed Fiorina's speculation that the rise in incumbent safety results from generational replacement. No general pro-incumbent trend has emerged; rather, incumbents in the aggregate have become more secure only because freshmen elected since 1966 have derived greater electoral rewards from their first term.

We have not addressed Fiorina's additional contention that the new freshmen's electoral strength stems from enhanced constituency service, nor have we investigated other techniques which they might have employed advantageously. Our results do suggest, however, that they have not merely been the fortuitous beneficiaries of behavioral change in the electorate. If decline in the salience of partisan cues alone were the cause of stronger backing for recent freshmen, why, then, would voters not have moved toward greater support for old-generation incumbents as well? It thus seems that new members themselves have induced voting shifts in their favor by more effectively capitalizing on the opportunities of incumbency.

## Appendix

The technique used to compute the electoral impacts produced by inter-election movements of veterans, generational replacement, and transitions between contested and uncontested election status may be demonstrated through the following example for the 1952–54 election pair. The average .109 margin for the 313 incumbents facing major party opposition in 1954 may be disaggregated into four terms representing the electoral contribution of the following groups: 225 veterans who previously had contested reelections in 1952, 20 freshmen who had defeated incumbents in 1952, 50 freshmen who had won open seats in 1952, and 18 veterans who had no competition in 1952. Similarly, the .119 average margin for the 286 opposed incumbents in 1952 can be disaggregated into a term for the same 225 veterans as above, a second for the 20 incumbents who lose in 1952, a third for 24 incumbents who retire after winning for the last time in 1952, and a final one for 17 veterans who then run unopposed in 1954.

The resulting equation for 1954, then, is

$$313(.109) = 225(.117) + 20(.057) + 50(.074) + 18(.159),$$

which can be written as

$$.109 = [225(.117) + 20(.057) + 50(.074 - .109) + 18(.159 - .109)] / 245.$$

Here, we have expressed the .109 average incumbent margin in 1954 in terms of the contributions provided by the 245 total districts which also had opposed incumbents running in 1952, plus the contributions relative to the 1954 incumbent average of the 68 total districts which did not have any opposed incumbents running in 1952. Similarly, the equation for 1952 is

$$286(.119) = 225(.121) + 20(-.028) + 24(.124) + 17(.256),$$

or

$$.119 = [225(.121) + 20(-.028) + 24(.124 - .119) + 17(.256 - .119)] / 245.$$

Subtracting the 1952 terms from their 1954 counterparts yields

$$-.010 = [225(-.004) + 20(.085) + [50(-.035) - 24(.005)] + [18(.050) - 17(.137)]] / 245,$$

or finally,

$$-.010 = -.004 + .007 - .008 - .006.$$

This final equation decomposes the overall .010 decline from 1952–54 in average margins into four different sources of change. The first two are straightforward, representing a .004 fall-off due to vote decreases among veterans, and a .007 increase resulting because the average 1954 margin of freshmen who had defeated incumbents two years earlier is larger than these losers' average 1952 margin. The .008 drop of the third term reflects the inter-election retirement effect; the relative contribution to 1954 incumbent safety of freshmen who had replaced retiring incumbents in open seats two years earlier is less than the relative contribution to 1952 safety of members who then choose not to run again in 1954. Generational replacement effects will be estimated by summing the second and third terms, which together measure the electoral impact caused by the replacement of veterans by new freshmen. The final fourth term is interpretable

as a .006 decline which occurs because the relative contribution to 1954 safety by members who were unopposed in 1952 is less than the relative contribution to 1952 safety by those who then move to unopposed status in 1954.

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## COMMUNICATIONS

### ON ARTICLES

#### Reply to Crosby (Vol. 73, March 1979, pp. 103-12)

**Editor's Note:** The following reply was not received in time to be published with Crosby's article. In accordance with *Review* policy, it was scheduled for publication in the next issue with available space. Faye Crosby responds. James Davies was also invited to comment on this exchange.

#### TO THE EDITOR:

A recent article in the *Review* by Faye Crosby (1979) has taken issue with our article (1977), "The J-Curve Theory and the Black Urban Riots: An Empirical Test of Progressive Relative Deprivation Theory." Crosby raises two primary and then several minor objections. Her primary objections are: (1) we fail to define relative deprivation and in so doing misrepresent relative deprivation theory; and (2) we employ inadequate measures of what she calls "felt deprivation."

We welcome Crosby's interest in our research and the opportunity to respond. We shall deal with her criticisms in the order she has established, looking at the primary criticisms first and then at the minor criticisms.

#### Relative Deprivation: A Theory in Search of a Definition

We find Crosby's concern with definition rather strange; indeed, it becomes even more incomprehensible in the light of the complete corpus of Crosby's own work (1976, 1979; Cook et al., 1977). Her criticism of our work implies that there is an agreed-upon definition of relative deprivation discernible from the literature, that it is empirically applicable to the study of collective violence, and that we sorely erred in not using it. As she and her colleagues (Cook et al., 1977, p. 308) have rather explicitly noted, "Examination of the past literature reveals no single definition common to the major theorists. Indeed definitions remained so loose that it is currently unclear as to whether relative deprivation is to be considered a hypothetical construct or an intervening variable." Frankly, we are compelled to confess that we do not know either.

Undoubtedly Crosby felt we should have resolved this elusive problem before moving ahead with our research. Aside from the fact that more agile minds have been unsuccessful in this pursuit, we are compelled to note that it was not what we set out to do. In fact, it was not even appropriate for us to have attempted it. Our concern was to test Davies' (1962, 1969) conceptualization of relative deprivation as he applied it to the black urban riots. To have used another definition would hardly prove commensurate with the requirements of validation.

Having faulted us for not engaging in what appears, from a reading of two decades of research, to have been a rather fruitless venture, Crosby further alleges that we never tested relative deprivation theory because Davies' theory is not a form of relative deprivation. It is discernible from this assertion that although no one presumes to agree what relative deprivation theory is, some know what it is not. Crosby thus alleges that whether the J-Curve is an illustration of relative deprivation, in its progressive form, is "a matter of some debate" (Crosby, 1979, p. 6).

Although Crosby does acknowledge that Gurr (1970) clearly sees the J-Curve as a form of relative deprivation, it is alleged that other sources, namely, Geschwender (1964) and Cook et al. (1977) see the matter in other terms.

A careful examination of Geschwender's (1964) somewhat questionable application of "objective" data to five hypotheses concerning what he calls "the Negro revolt" reveals quite a different story. The question of whether Davies' theory was or was not an example of relative deprivation did not concern Geschwender, for he was concerned only with incorporating under a common rubric those hypotheses he was *unable* to reject. Davies' theory was rejected in Geschwender's analysis. Consequently, he did not concern himself with the question of where Davies' theory fit.

As curious as the inapplicable citation of Geschwender's work is, it is exceeded by Crosby's citation of her co-authored work (Cook et al., 1977), for there is no mention of Davies, neither in the article nor in the references. Consequently, one is compelled to conclude that Davies' work is controversial simply because Crosby alone has alleged that it is. And the reason for this exorcism of Davies from relative deprivation theory becomes apparent

when we closely examine another of Crosby's works (1976).

The thrust of Crosby's work has been to establish a precise conceptual definition of relative deprivation. Such attempts at conceptual clarification are laudable. However, what Crosby fails to appreciate is that concepts like language mean what the users have come to make them mean through usage. Science is to no small extent what scientists do, and concepts derive meaning from their actual formulation and measurement as part of the research process. Crosby's work, until very recently, has had no empirical basis, and it comes as a myopic intrusion, ignoring the ongoing research process.

Crosby parts company with Davies for two reasons: (1) he did not posit that individuals must "feel" entitled to possess the item of which they have been deprived; (2) Davies did not deal with the issue of feasibility, i.e., did the individual feel the attainment of the desired commodity was feasible? (It will be noted that it is the absence of an adequate measure of felt deprivation which caused Crosby to take issue with our work, and we will address this concern below in some detail.)

These issues may be important to Crosby but they were obviously not important to Davies. As we were dealing with Davies' conceptualization of relative deprivation and not hers, the issues did not interest us. Crosby may feel that the avoidance of these issues should deprive Davies of his recognition as a relative deprivation theorist and similarly deprive us of any claim to having tested relative deprivation theory, but we doubt whether many will take such autocratic assertions seriously.

Equally irrelevant is Crosby's criticism that we did not properly test Davis' (1959), Runciman's (1966) and most of course her own theories of relative deprivation (Crosby, 1976). In fact, why restrict us to these? Should we have tested every version of relative deprivation theory? Indeed, we are somewhat dismayed that Crosby should require the testing she proposes. In a recent paper (Crosby and Bernstein, 1978), as far as we know, her only effort to subject her attempts at conceptual clarification to empirical verification, her work received only partial confirmation and Runciman's work was disconfirmed. On the other hand, Gurr's (1970) model, which she says we tested, was confirmed.

We are thus accused of not incorporating into our analysis relative deprivation models which were irrelevant to our concerns, which were, until very recently, only conceptual

formulations without empirical verification, and which, upon being subjected to empirical testing, have been generally found to be without much substance. This is hardly a sin of omission worthy of intellectual concern.

The crux of Crosby's quarrel with us is really that her understanding of relative deprivation differs from ours and that disconfirming Davies does not disconfirm relative deprivation theory. Her quarrel about the understanding of relative deprivation theory only brings into focus what everyone working in the area sooner or later recognizes, namely, that after at least two decades of researching, conceptualizing and theorizing, social scientists have not reached a commonly agreed-upon theory of relative deprivation or even a consensus as to what the term actually means.

Despite her attempts at resolving this issue, Crosby in her own way has contributed to the confusion. She has set standards without due regard for the ongoing empirical research, or for the realities of data which of necessity must be generated outside the laboratory setting. She has abrogated to herself the role of final arbiter of the dispute, proclaiming that relative deprivation theory is what she says it is, and she has done so while failing to acknowledge the fact that even her empirical research belies her own conceptualization.

Her quarrel is based on our not having set out to do what she has done, or, for that matter, not having seen what she had done as remotely relevant to our work. Our concern was not to fashion a new or even more precise conceptualization of relative deprivation theory but to test its application by Davies to the black urban riots of the 1960s. We were only interested in the overall conceptual problems of relative deprivation theory (with one exception which we shall note below) as they related to this specific case. It is possible when taking our language out of context and removing it from the overall focus of our concern to suggest a more general inquiry into relative deprivation theory. But we do not think this is a fair assessment of our work.

The one area where we did look at a general aspect of relative deprivation theory was in the relationship between current need achievement and future expectations. We did say that the relationship was virtually axiomatic to "all" forms of relative deprivation theory. This was too strong a statement.

What we should have said is that the relationship is important for many forms of relative deprivation theory, especially those which have been applied to civil violence.

Although we are grateful to Crosby for having pointed out this overstatement, we feel that she has confused the issue, having created an even less appropriate overstatement in the opposite direction. As Crosby notes (1979, p. 12), "In plain and simple terms, relative deprivation theory does *not* (emphasis hers) assume a correspondence between present conditions and future expectations." Crosby is wrong! She has failed to understand that the application of relative deprivation theory to collective violence has of necessity concentrated on a dynamic process. As Gurr (1970, p. 46) notes in describing this dynamic process of relative deprivation, "Dynamic analysis requires conceptual tools that take account patterns of change in value capabilities over time." In such analyses, future expectations have been either explicitly or implicitly seen as a result of current need fulfillment (Davies, 1962; Gurr, 1970, p. 74). In fact, Fierabend et al. (1969) go so far as to explicate graphically this very assumption. As we noted (Miller et al., 1977, p. 977, n. 25), Gurr thinks this assumption is so fundamental to the application of relative deprivation theory to the study of collective violence that he and Raymond Duvall undertook research to make the relationship even more precise. Crosby's interpretation of relative deprivation may not assume a correspondence between present conditions and future expectations, but this is hardly the basis for alleging that relative deprivation theory does not make such an assumption.

### The Issue of Felt Deprivation

The second of Crosby's major objections is directed against what she calls our inadequate assessment of "felt" deprivation. It will be recalled that it was this failure that she noted in her assessment of Davies, and one which caused her to excise him from the field of relative deprivation theorists. We happily followed Davies' lead in this matter, for it is, after all, his theory that we were testing.

We are unable to ascertain what is gained by Crosby's insistence on defining relative deprivation as an emotion roughly synonymous with a type of anger, dissatisfaction, resentment or grievance. This hardly contributes to the explicit and succinct definition of relative deprivation for which Crosby continues to call.

The issue of "felt" deprivation of course is not unrelated to the issue of how one measures relative deprivation. If relative deprivation is described as a cognition (as we do, following

Davies) it will tend to be assessed through perceptual measures of achievements, expectations and gratifications. On the other hand, if one conceives of relative deprivation as Crosby does—as an emotion—then it will be measured through feelings of anger or resentment. But from Davies' perspective, and incidentally Gurr's, these emotions are clearly consequences and not causes of relative deprivation.

This disagreement leads to Crosby's objection to our measures, which she describes as "clear indicators of one of the hypothesized preconditions of felt deprivation" (1979, p. 17). What she apparently is unable to comprehend is that neither we nor Davies was concerned with whether blacks "felt" deprived, resentful, or frustrated. After all, people can feel deprived, resentful, or frustrated for a variety of reasons. Our concern was to assess whether or not the black population experienced, over a number of years, a pattern of relative deprivation illustrative of the J-Curve. Our three perceptual items measured fundamental components of relative deprivation theory—expectations, perceived gratifications and perceived attainments—as a means of assessing the J-Curve pattern. Their ability to measure felt deprivation was completely irrelevant to our concerns, as they were irrelevant to Davies' concerns.

### Rejoinder to Crosby's Small Critique

The best that can be said of Crosby's small critique (1979, pp. 109–10) is that it couples casual reading with serious misunderstanding; much of the latter would have been eliminated had she paid more attention to our work. Her questions about the sampling procedure are illustrative. We plainly noted that our sample came from the University of Michigan Survey Research Center election studies. That rather simple knowledge should have answered Crosby's questions about design, comparability of sample size and grouping by region.

While Crosby's attempts at theoretical criticism are maladroit, her attempts at methodological criticism show a naivete about even rudimentary principles of data analysis. Thus her assertion that we should have aggregated our data rather than Davies' data in order to compare the differences between individual and aggregate data is astounding. Davies' (1969) aggregate data came from the U.S. census; consequently, they were far and away superior to any aggregation of sample data. To have tested the aggregate characteristics from our

sample data would have obviously been a less accurate, less meaningful and less appropriate test. Indeed, if Crosby were serious about such a test, she should have performed it. As Bross (1960) has noted, the appropriate role of a critic is not simply to cast assertions but empirically to test the validity of counter-hypotheses. Obviously, Crosby preferred not to do this.

This erroneous methodological assertion is followed by another. Crosby conducts a heterogeneity-of-variance test to determine if the pattern of our curve is other than would be expected by chance. The nature and shape of a curve cannot be tested by an analysis of the statistical significance of the individual points of the curve. This is a common methodological error but its frequency of occurrence does not make it correct. We take little comfort in Crosby's observation that our work received some modest degree of confirmation by this inappropriate procedure.

#### Some Additional Confirmation of Our Findings

We find nothing in Crosby's work to cause us to reconsider our substantive findings or our conclusions. The progressive form of relative deprivation theory does not explain the urban riots of the 1960s. Our data do suggest that the ambiguity and uncertainty that blacks perceive about their current and future financial well-being may have created the conditions for the riots.

To test further the comparative ability of these theories to explain the black urban riots, we conducted some additional testing. We view these tests as supplementary to the ones presented in our research. They are less direct and look at a static rather than a dynamic process. Consequently, they are less powerful, but their value resides in the *additional* confirmation that they provide.

We took "hostility toward whites" as a dependent variable and looked at seven composite explanatory variables. The data were derived from the 1968 Michigan Survey Research Center Election Study. The dependent variable was the SRC feeling thermometer (degree of feeling toward whites). The independent variables consisted of social uncertainty; personal uncertainty; political cynicism; personal political efficacy; system unresponsiveness; status uncertainty; government trust; and relative deprivation. The relative deprivation index consisted of a composite of three other composite variables. These consisted of: (1)

individual relative deprivation, a measure that scored relative deprivation when an individual indicated that he anticipated the future to be worse than the present; (2) an individual's present relative deprivation compared to that of blacks as a group, a measure which assesses the respondent's comparative perceptions of these two phenomena; (3) expectations of individual future deprivation relative to the future status of blacks as a group. A complete description of all the measurements and tests for item inter-correlation can be found in Bolce (1976, pp. 180-83).

It should be noted that Crosby (1979, pp. 18-19), after some misinterpretation of Davies, suggested a measure of Davies' operationalization that would incorporate two perceptions: (1) respondents who see the current year as better than the year before *and* (2) those who expect a deterioration of conditions in the future. The operationalization of relative deprivation described above incorporates the notions suggested by Crosby as well as group and individual frames of reference.

These indices were placed in a stepwise regression with hostility toward whites as the dependent variable. The data from this procedure, shown in Table 1, are presented in rank order, in terms of their ability to explain variance in the dependent variable. Not only is relative deprivation the weakest explanatory variable but also its contribution is negative. Personal uncertainty was found to explain more variance than any other variable.

These data are consistent with our earlier findings which illustrated that relative deprivation was not an explanation of the black urban riots. We stand by those results.

ABRAHAM H. MILLER

*University of Cincinnati*

LOUIS H. BOLCE

*Fordham University, Lincoln Center*

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Table 1. Coefficients for Stepwise Regression with Hostility towards Whites as Dependent Variable, Northern Black Sample, 1968

Variable	Multiple R	R-Square	RSQ Change	Simple R	B	Beta
Personal Uncertainty	.228	.052	.052	.228	.436	.235
System Unresponsiveness	.271	.073	.021	-.099	-.489	.333
Political Cynicism	.348	.120	.047	.149	.453	.218
Personal Political Efficacy	.390	.152	.032	.164	.342	.193
Government Trust	.405	.163	.011	.110	.187	.113
Status Uncertainty	.407	.165	.001	.014	.161	.080
Relative Deprivation	.408	.166	.001	-.032	-.086	-.051
(Constant)					1.186	

Source: Louis H. Bolce, "Urban Violence and the Culture of Ambiguity: The Fluctuation Uncertainty Thesis," (University of Cincinnati: unpublished dissertation, 1976), Table 5:3, p. 158.

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## Rejoinder

### TO THE EDITOR:

Thank you for the invitation to respond to the communication by Miller and Bolce. I shall divide the reply into three sections: (1) restatement of my general position, (2) examination of a series of specific issues, and (3) musings.

### General Issues

Judging from the first paragraph of their communication, Miller and Bolce did not clearly understand my central point. Allow me to reiterate it here. The point of my article (Crosby, 1979) was that Miller, Bolce, and Halligan (1977) were not logically correct in rejecting relative deprivation theory as the explanation of black urban unrest on the basis of their data and analyses. I did not claim that relative deprivation theory provides the correct explanation. Nor did I claim that the J-curve theory may not be considered to constitute one form of relative deprivation theory. Rather, I asserted that the data they presented *did not lead logically* to a refutation of relative deprivation theory. Their claim to the contrary appeared to spring from a lack of familiarity with the relative deprivation literature and from a looseness of operationalization.

My position is, I think, supported by the Miller-Bolce communication. In several instances (e.g., paragraphs 4, 11, 14, 16, and 19, pp. 818-20), Miller and Bolce announce that, in their 1977 article, they were concerned only with Davies' J-curve theory. Obviously, the J-curve is one formulation of relative deprivation theory. Because they were concerned with only one version of the theory, they were prevented, logically, from drawing conclusions



about the theory as a whole.

### Specific Issues

The Miller-Bolce letter raises numerous issues. I would like to discuss nine of them, taking them in the order of appearance in the Miller-Bolce communication.

1. Miller and Bolce state in their opening paragraph that one of my "primary objections" to their article was their failure to define relative deprivation. In fact, the definitional failure was but one example of what I took to be the broader problem: their misrepresentation of the literature. Other aspects of the problem in my view included their equating J-curve and relative deprivation theory, their ignorance of the work of Davis (1959), Runciman (1966), and myself (Crosby, 1976), their slanted view of Gurr (1970), and their false claims about the empirical research performed in the area of relative deprivation.

2. The communication accuses me of claiming that "Davies' theory is not a form of relative deprivation theory" (p. 818). In fact, I said that it is one form of relative deprivation theory. My claim was simply that Miller et al. had mistaken a part for the whole. In my article, I stated it this way: "Arguing that J-curve theory is invalid is one matter. It is quite another matter to argue that because J-curve theory is invalid, all of relative deprivation theory is invalid." (It is, incidentally, my belief that Miller and Bolce fail to demonstrate that the J-curve is invalid. Davies [1978] points out some of the ways in which Miller et al. present a "deficient" test of the J-curve hypothesis.)

3. The tenth paragraph opens with: "Crosby parts company with Davies. . ." (p. 819). It seems that Miller and Bolce are referring here to some parts of an unpublished manuscript which I sent them (Crosby and Bernstein, 1978). I am confused, however, by the reference to Davies. Bernstein and I discussed Davis, not Davies.

4. Miller and Bolce suggest that the only problem I found with their view was that it did not agree with my own. Among the sentences one could cite are these: "The crux of Crosby's quarrel with us is really that her understanding of relative deprivation differs from ours. . ." (p. 819), and, "Her quarrel is based on our not having set out to do what she has done" (p. 819). It seems ironic indeed that Miller and Bolce give me and my formulation of relative deprivation proportionately much more attention than I myself did.

My original piece did not claim that Miller,

Bolce, and Halligan ought to know my model of relative deprivation. On the contrary, I explicitly excused them for not knowing it. I did chide them, however, for not knowing the work of Runciman and of Davis. I remain dismayed by their continued silence about these scholars. Runciman and Davis deserve more than the passing glance they receive in the Miller-Bolce communication. On p. 819, Miller and Bolce characterize my concern for Runciman and for Davis, and (by implication) for Gurr as "irrelevant" and ask if they should be expected to test all the relative deprivation theorists. The answer is obvious. If one wants to make claims about relative deprivation theory in general (as they did in 1977), then one must give the theory a fair hearing. No part can stand for the whole.

5. Miller and Bolce continue to assert that relative deprivation theory assumes a correspondence between present conditions and future expectations. To support their claim that Gurr sees the present-future link as "fundamental to the application of relative deprivation theory to the study of collective violence" (p. 820), they refer to Gurr's ongoing work with Duvall. Concerning the Gurr-Duvall collaboration, the 1977 article read: "All we can say is that sometimes, and for some groups, future expectations are a function of current need fulfillment, and at other times, for other groups, this relationship does not hold" (Miller et al., 1977). Which tune are we to follow?

6. On the issue of "felt deprivation" Miller and Bolce seem off the mark. If they accorded Davis and Runciman the attention they deserve as relative deprivation theorists of long standing, Miller and Bolce would have to admit that deprivation is conceived as a feeling. Even Gurr speaks of a tension state. Davies too mentions emotions. One distinction that Miller and Bolce make is rather too subtle for me. They show no interest in "whether blacks 'felt' deprived" and restrict their curiosity to "whether the black population experienced . . . a pattern of relative deprivation" (p. 820). I do not know how one can experience a pattern. Nor do I follow how one can separate feelings from experiences.

7. In my article I suggested that Miller et al. ought not to have drawn conclusions based on a comparison of their Table 1 and their Table 2. We see from their communication that the former contained aggregated census data; the latter contained individual-level survey data. Whether the census data are superior or inferior to the Michigan survey data is irrelevant. The point is that they are different. This means that the observed differences between Tables 1 and

2 could have been owing to (a) the different levels of data (aggregate-individual) as Miller et al. claim or (b) something about the way in which the data were collected. The alternative is plausible.

8. Miller and Bolce believe that I made a mistake in my own small statistical analysis. They claim that one may not test the nature of a curve by analysis of the individual points. At least one standard statistical textbook (Hays, 1963) suggests the opposite; Miller and Bolce thought I had tested to see if the "curve is other than would be expected by chance" (p. 821). I did *not* test to see if the curve for northern blacks differed from chance variations. I did test to see if the variance in that curve differed significantly from the variance in the curve for the northern whites. Their own original uncertainty argument rested on their "eyeball" observation that the northern black curve fluctuated more than the other curves. I was attempting to provide a more precise test of their speculations than they themselves did.

Since writing my original piece, I have learned of an analysis which might be equally or even more appropriate for the issue of fluctuations than is the test for heterogeneity of variance. It is Tukey's test for smoothing curves (Tukey, 1977, esp. chapter 7). To use this test, one would have to convert the aggregate data into single points, as I did in the test for heterogeneity of variance. The advantage of Tukey's test is that it is sensitive to dips and ridges, while the standard test for heterogeneity of variance is not.

9. I am interested by the dissertation data analysis presented by Miller and Bolce at the end of their letter. They claim the operationalization of relative deprivation, described on p. 821, "incorporates the notions suggested by Crosby as well as group and individual frames of reference" (p. 821). As far as I can tell, the measure does get at two of the hypothesized preconditions of my model (Crosby, 1976). But what has all this to do with assessing Davies' (1962, 1969) rise and fall model with which it is linked in the text?

### Musings

Let me reiterate a point. Neither in my original article nor here do I wish to claim that relative deprivation theory provides the best explanation of black urban unrest. My central point is simply that the data and the analyses in the Miller, Bolce, and Halligan article and in the Miller-Bolce communication fail to refute rela-

tive deprivation. Why, then, all this fuss?<sup>1</sup>

One reason for the fuss, I think, has to do with the nature of social scientific inquiry. What differentiates scientific and logical endeavors from other endeavors is a matter of method and not of conclusion. One may conclude in the next years or months that relative deprivation theory is wrong or that it is right. Quite apart from the conclusion itself is the method one uses to reach the conclusion. The scientific method involves precision and logic. In the end, then, my concern with the work of Miller et al. rests on my conviction that in the social sciences we ought to care about how we reach our conclusions as much as we care about the conclusions themselves.

FAYE CROSBY

Yale University

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<sup>1</sup>That my own fussing has been tempered considerably is due in large measure to Robert Abelson, Travis Crosby, and especially Donald Kinder. I am grateful for their help.

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### Comment by Davies

#### TO THE EDITOR:

As the accused and admitted parent of a theory that helps explain political violence, I want to say something about how the idea was conceived, its gestation, and about its continued growth since it was born in 1962. The idea, in its original or improved versions (Davies 1962, 1969), has been published in whole or in part more than 25 times, but the occasional abuse and misunderstanding of this brainchild compels a parental defense. An earlier analysis in this *Review* (Davies, 1978) of the critiques by Miller, Bolce, and Halligan (1977, 1978) of the J-curve seems not to have established clear contact with the critics. At least, they have not very explicitly responded to my queries about what they think black people wanted in their 1960s rebellion and about when cycles begin.

#### The Ancestry and the Gestation

The J-curve's ancestry is a bit complicated but clearly traceable, at least back to the second generation. The idea was conceived about 1956, when I was trying to figure out why the 1894 strike of Pullman railway car workers took place when it did. Census Bureau data on both wages and prices for the three decades following the 1861-65 Civil War revealed a steady, slow rise in real wages among working people. Wages remained roughly constant; prices declined. During the 1894 recession, the gradual, generation-long rise in real wages was interrupted abruptly, most particularly for the Pullman railway car workers. Many people who worked in the Pullman shops and lived in the town which the company had built to house them were put on part-time schedules or were laid off altogether. Many of these people had very little to eat and were simultaneously pressed by the company to pay their suddenly delinquent rents. A nice J-curve describing the generation-long event popped out of the data, into my head, and onto the page of notes I was making.<sup>1</sup>

<sup>1</sup>The data on real wages are in U.S. Bureau of the Census (1949, pp. 66, 231-32, 235). See also Lindsey (1942, esp. Chs. 5 and 7).

The idea grew and was nourished by the writings of some rather remarkable people, whom I charge with grandpaternity: Crane Brinton's *Anatomy of Revolution*, which I had read when it was first published in 1938; the work of Abraham Maslow, whose need hierarchy (1943) is a basic component of the theory and the research of now several political psychologists (Davies, 1963; Aronoff, 1967; Knutson, 1972; Renshon, 1974; Inglehart, 1977; and Burns, 1978). And there were two remarkable others, whose most relevant work I encountered after first plotting the J-curve.

Tocqueville's 1856 analysis of what led to the French Revolution noted in grand detail how things in France steadily improved during the eighteenth century, not just culturally but also economically and socially. (From Brinton and others I'd already got information about the 1788 economic crisis, which included a critically sharp drop in the supply of grain.) To this was added rumination about Marx and Engels' 1848 *Communist Manifesto* which hammered on the ways industrialization was making the life of factory workers steadily worse. In the spring of 1960 Seymour Martin Lipset pointed out to me that Marx had written not only about increased immiseration but also about how people compare their circumstances with those of people in other classes: "Our desires and pleasures spring from society. . . . Because they are of a social nature, they are of a relative nature."

Three major components of theorizing about revolution were thus related to the writings of Tocqueville and Marx and Engels—jointly and separately. Tocqueville described an upswing before revolutions, Marx and Engels a downswing. And Marx (1849) clearly stated, in one of its forms, what Stouffer (1949), Pettigrew (1967), and Gurr (1968, 1969 and 1970), long after, called relative deprivation.

Stated most simply, the theory that was conceived about 1956 in a study of the Pullman Strike, nourished in *utero mentis* by the ideas of Brinton, Maslow, Tocqueville, Marx, and Engels, and born in 1962, states this: when a relatively long period of steady rise in what people want and what people get is followed by a short period of sharp reversal, during which a gap suddenly widens between what people want and what they get, the likelihood of revolution increases sharply.

#### The Growth of the Idea Since 1962

The Black Rebellion and the Student Rebellion of the 1960s rekindled interest among

social scientists in pursuing nonsuperficial explanations for these very significant events. The timely appearance of Ted Robert Gurr's *Why Men Rebel* in 1969 provided a solid basis for deepening the study of conflict. Unfortunately, people who undertook such new research adopted the practice of not really seeking out the origins of ideas about civil violence, in the writings either of the honored but ill-understood dead—Aristotle, Hobbes, Tocqueville, and Marx and Engels—or of more recent writers like Sorokin (1925), Edwards (1927), Pettie (1938) and Brinton (1938, 1965).

An even greater misfortune is that such recent researchers have shown little understanding of basic human motivation as theory and research about it have developed in recent decades, out of the work of James (1890), McDougall (1908), Freud (1930), Murray (1938), Fromm (1941), and Maslow (1943). Like John Locke, Karl Marx, Ivan Pavlov, J. B. Watson, and B. F. Skinner, recent researchers seem to suppose that human beings are (almost) entirely what society conditions them to be. Or that, if they do innately possess anything, it's only the animal desires to survive and procreate.

In brief, the unexamined and seemingly Marxist assumption as to human motivation has been that humans do indeed, like animals, live by food alone. Even this assumption is falsely founded in Marx, whose concern was not simply with the extreme physical deprivation which poverty produces but also with the ways in which poverty breaks the elemental social ties of family and destroys human dignity and makes it impossible for people to fulfill their uniquely human potential. Researchers in civil conflict in the 1960s and 1970s have exhibited little theoretical understanding of earlier writing on either revolution and rebellion or of basic human motivation, because they evidently have not read anything relevant that was written before the 1960s. In Arthur Koestler's phrase, they are a generation without an umbilical cord.

In addition to innocence regarding motivational psychology, there has been another deficiency in recent research in political conflict: a non- or misunderstanding of time periods and therefore, inter alia, of what is and what is not a complete cycle. This deficiency becomes particularly apparent when the theory that is being examined relates to cycles within cycles and epochs within epochs. The deficiency makes it possible to deny sequence (by starting in the middle of a cycle) and to deny cause and effect (by discarding the early part of

a cycle, during which the causes are starting to produce effects).

Let me give an example of cycles within a cycle, the ones I mentioned in the 1962 article, in discussing the Russian Revolution. The emancipation of the serfs in 1861 began what I regard as the final long upswing of expectations that peaked 44 years later, on the eve of the 1905 revolution. The long cycle ended its downturn 56 years later in February 1917 when, in the midst of war, a general strike and a revolution "spontaneously" burst forth. After this February event, the government never again was able to regain control and restore order. The 56-year J-curve cycle was complete.

Within this long cycle, I included briefer, less intense J-curve cycles, with sharp downturns following long rises. The first briefer cycle was related to the assassination of Alexander II in 1881. The second came when the futile war in East Asia was followed by the 1905 Revolution. And the third came when the solid recovery after 1910 was followed by the brief downturn which the German attack precipitated in 1914.

The three short cycles have to be understood in context of the great cycle. No one of them was big enough to effect the final overthrow of the Tsarist regime, but their effect was cumulative. Widespread rebellion burst forth in 1905, even though serfdom had legally ended 44 years before, promising to make one-time serfs better off and thereby raising their expectations. Factory workers by 1905 were very likely better off than they had been as either serfs or the children of serfs. When a variety of such improvements had accustomed people to expect continued improvement, the degrading and wasteful war against Japan precipitated the sudden downturn.<sup>2</sup>

The phenomenon of cycles within cycles can be better understood by noting its comparability to ocean waves along shorelines. Small waves have a length of a few inches and a height of less than an inch. Larger waves, up to and including those that result from severe storms or earthquakes, have a length ranging from several yards to several miles and a height ranging up to several yards. The energy carried in these large waves is enormous. Similarly, wave lengths in the electromagnetic spectrum range from tiny fractions of a micrometer, measured in angstrom units, to several meters;

<sup>2</sup>See Davies (1962, pp. 10–13) for more detailed appraisal of the waves-within-waves. (Note particularly Figure 3, p. 13.)

amplitudes, like the height of ocean waves, similarly vary greatly. And of course in both kinds of wave, there are cycles within cycles—shorter waves within longer ones. These cycles within cycles provide the means by which music is heard and, separately, the means by which it is broadcast by radio.

Theorists and researchers who fail to recognize the cyclical nature of public moods can neither understand the beginnings and turning points of cycles, including J-curve cycles, nor undertake causal analysis of civil conflict. This cyclical characteristic derives from the states of mind of people in a polity, as these mental states change during both long and short cycles and as they form a usually unpremeditated consensus to act at a particular time. Starting points of upswings are unavoidably harder to define than turning points, for reasons of the same kind that make it harder for two people to determine when they started to have a falling out than it is for them to determine when they had a real fight and ended their friendship.

#### **The Rising Expectations of Black People**

The recent analysis of the J-curve by Miller et al. (1977, 1978, 1979) in this *Review*, in the context of the Black Rebellion of the 1960s, exemplifies the deficiencies I have mentioned: the lack of understanding of theoretical intellectual history, a reduction of human motivation to material needs only, and a misunderstanding of cycles and therefore of starting points. The expectations of black people that these researchers consider do not include any expectations other than "financial" ones, and they consider these only during the period of their data base (1956–1968). They deprive black people of their history.

What is puzzling is that the authors evidently read, in the 1969 version of the J-curve, my analysis of events that led the expectations of black people to rise after 1940 (Davies, 1969, pp. 716–25). There I specifically mentioned occupational integration into the armed forces and defense industries during the Second World War (a major advance in *both* economic *and* equality expectations), a decline in lynchings, integration in housing and education, reduction of the income-education gap, and the beginning of successful direct action in the 1956 Montgomery bus boycott. And I mentioned the downturn coming after the April 1963 pitched battle between police and lunch-counter "sit-inners" in Birmingham, with the first outbreak/of

major violence occurring in the Watts area in Los Angeles in August 1965. The recent analysis ignores all this and reduces its critique to only that segment of material expectations that relate to finances, not beginning with 1940, when the final big upswing began, but 1956.

#### **When Do Cycles Begin?**

This misunderstanding of cycles is even harder for me to understand than the lack of theoretical background. If Miller et al. reject the idea that the last upturn commenced among black people in 1940 and assert that it commenced in 1956, they should explain why they ignore such expectation-raising events as the reduction in discrimination in jobs, housing, public transportation and education—all of these events commencing well before 1956. And they should explain why, aside from the fact that 1956 was the year in which University of Michigan Survey Research Center began gathering the data they analyzed, they see 1956 as the year when a rise in expectations commenced.

After listing a set of four hypotheses about the "financial" circumstances of black people from 1956–1968, Miller et al. state (1977, p. 974): "The rejection of *any* one" of their four statements about the financial circumstances of black people after 1956 "would result in a rejection of the J-curve theory as an explanation of the black urban riots." This could be true only if the final long cycle that I said commenced about 1940 actually commenced in 1956 and only if the one thing black people were concerned about was their financial condition. It is hard to imagine a clearer case of the drunk looking for his keys under the street lamp because that is where the light is brightest.

#### **The Development of Theory beyond Mere Description**

Having disposed of the J-curve, Miller et al. come up with what they call a fluctuation-change hypothesis (1977, p. 980) whose essence evidently is that civil violence is the product of rather intense ups and downs, whose meaning to those experiencing it is ambiguous. This does make sense, if the authors can establish that the intense ups and downs and the ambiguity preceded, and therefore can be said to have produced, the violence itself. Although there is a time interval between their downs of 1956 and 1962 and what I consider the first major outbreak of concerted black violence in August 1965 (the Watts riots in Los

Angeles), Miller et al. also note a down in 1966. I think what they are doing is predicting a storm by noting the big waves that indeed are the storm itself.

Other theories about civil conflict are akin to that of Miller et al., in that they do not substantially distinguish between the causes of events and the events themselves. Mancur Olson (1963) says that rapid change is a cause of civil disturbances. Indeed there is an association between rapid change and such conflict, but does rapid change occur spontaneously? Charles Tilly and associates (1972, 1974, 1978) have proposed a theory, first, that the struggle for power between various segments of society is the cause of civil violence, and, second, that the formation of groups is such a cause for that struggle. Like Olson's, Tilly's theory is more tautological than etiological. That is, the struggle for power is a definition, not the cause of political conflict—of every kind. When the struggle, the conflict, becomes violent, we call the process rebellion or revolution. And what political action—whether peaceful *or* violent—takes place *without* the formation of groups? Group formation is a cause of almost anything in social life.

A basic question in developing theory is its explanatory, that is, its metadescriptive, value: does a theory make it possible to sort out—eventually in advance—causes from consequences clearly enough so that we can distinguish one from the other? The fluctuation-change hypothesis, the rapid-change analysis, and the power-struggle theory all are almost totally situational and inexplicit about time as a dimension. They describe circumstances, and they regard the reaction *within* human beings to these circumstances as either irrelevant or unidimensional. They say, by implication, that it does not really matter what human beings want and what therefore causes tension when they do not get what they want. Or they imply that if they do want something, it is only material goods.

The J-curve was conceived as I studied the Pullman Strike of 1894. Like the Black Rebellion, it was not a full-scale revolution. In the 1962 article, I applied the J-curve to another such event: Dorr's Rebellion in Rhode Island in 1842. Neither event was a full-fledged revolution, but ruminations and readings after looking at the Pullman Strike made it seem appropriate to apply the idea to the big ones. Ruminations and readings since 1962 made it seem appropriate to re-apply the idea not only to the Black Rebellion but also to the Student Rebellion in America in the 1960s: university students

whose nonmaterial expectations of being able to prepare for challenging careers were frustrated by the threat of military service in Vietnam. It was not only physical deprivation but also deprivation of the need for self-actualization that helped to generate the Student Rebellion. The Vietnam War caused students thus to experience a rapidly widening gap between what they wanted and what they were getting.

In short, I think the J-curve, though not total, is a rather fundamental theory. It is so because it is based on psychological foundations, in basic theory of human motivations. For the same scientific reasons that nuclear physics takes the atom as a unit of analysis, the theory takes the individual human being as a unit of analysis. Theory that is based on analyzing social change in its own terms and not in terms of the intrahuman causes of social change is tautological and incapable of probing to the depth necessary for social analysis to become more adequately causal. Marxist theory is indeed a necessary part of the explanation of the activation of poor people in developing countries, but it has not worked adequately to explain violence in either developing or advanced countries. For one thing, the continuing conflict between business and labor has become less violent, less revolution-prone with trade union leaders, in many instances, in both capitalist and socialist countries, treating their "men" like commodities, in what Marx said was the capitalist manner. For another, a major component in such upheavals as the 1952 Egyptian and the 1979 Iranian revolutions was the demand for recognition of the equal station in the family of nations that is not a function of either economic or military power.

The analytical situation, in sum, is complex but not impossibly so if one does not wholly ignore human beings not only as units of analysis but also as among the originators—among the causes—of civil conflict. In addition, it helps to look for evidence that large groups of human beings in a society are at approximately the same stage of development and are likely to want to enter the next stage at about the same time. As I saw it, the Black Rebellion of the 1960s focused clearly on the newly shared desire of black individuals to pursue, as a collectivity, their equality and dignity, in American society. They could not have so acted together if they had not already achieved the minimal economic well-being that, for example, meant that they had enough money to go to a lunch counter with the ability to pay for their own lunch and enough money in Montgomery

in 1956 to buy and use enough cars to transport one another to work and thereby make the bus boycott effective.

### Some New Theory

In two recent articles, I have posited a relationship between the stages of development of individuals and the stages of development of politics (Davies 1977a, 1977b). This theory poses more grievous problems of testing than the very real but I think manageable ones relating to the J-curve. This theory elaborates not only on the general political implication of basic human needs but also on how, for large groups of people in a polity, like black people in America in the 1960s, the transition from one stage to the next takes place during the same epoch. The theory also argues that frustrations of physical, material needs are by no means the only ones that produce violent political conflict.

The revolutions in the Third World, late in the twentieth century, against colonial domination are in part an echo of the revolution against colonial domination that commenced in 1775 and was justified a year later with the statement of a first cause: "All men are created equal." In the twentieth century, the same demand is among the foremost of those that have helped to cause such revolutions as that which occurred in Iran in January 1979.

Until the social sciences develop more specific equations which more specifically include human beings as *active* determinants of change—rather than as merely acted-on products of their historic and current circumstances—theory and research will remain radically deficient. To fail to consider the full range of basic human wants—from survival, to social ties, equal dignity, and self-fulfillment—leads to ingenious and/or rigorous exercises which confirm only the obvious: that there is a relationship between instability and violence.

JAMES CHOWNING DAVIES

University of Oregon

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#### Comment on Dobel

(Vol. 72, September 1978, pp. 958–73)

#### TO THE EDITOR:

Patrick Dobel's ambition in "The Corruption of a State" (1978) is to contribute a general positive theory of corruption. He builds his theory upon the references to corruption he was able to find in the writings of Thucydides, Plato, Aristotle, Machiavelli, and Rousseau. However, while such sources can give his theory academic respectability, they cannot, by themselves, give it scientific credibility, especially since they are quoted outside their historical and, to a large extent, philosophical, contexts. Only internal consistency and external verifiability can give Dobel's theory scientific credibility.

It is often said that corruption is the second oldest human trade, the first being prostitution. But no matter how widespread the practice, the

pattern of corruption and prostitution varies in time and space. There is a common general idea of what corruption is; but the precise definition of corrupt behavior differs historically and geographically. Indeed, it also differs within the same historical and geographical context. Thus, dealing with corruption, one must refer to two patterns of behavior: the actual behavior of the corruptor and/or the corrupted, and the accusations of corruption that others address to either or both of them in connection with their behavior. For instance, an Italian politician might solicit a bribe from a foreign corporation and be accused by his rivals of allowing his policies to be shaped by foreign concerns; likewise, an American corporation might bribe a foreign politician and be accused by American politicians of jeopardizing American policies abroad. What is corruption?

Dobel proposes an objective definition of corruption: namely, the process of internal decay and disintegration undergone by a society. It is an observer-invented definition and as such must be judged as to whether it throws new light upon the phenomena to which it is applicable and/or withholds potential light from other phenomena to which it can no longer be applied.

Dobel, however, does not label as corruption all processes of social decay, but only those characterized by disloyalty. To call such processes corruption is by itself unlikely to throw light upon them. Unless. . .

Underneath Dobel's explicit dissertation runs another theory, based on the popular concept of corruption. According to this other theory, disloyalty and corruption are not the same but different behavior patterns, and the second one, corruption, is caused by the first, disloyalty. In other words, Dobel would suggest approaching from a perspective of corruption the study of the processes of social decay characterized by the disloyalty of the members to the group. This is not a banal suggestion and could indeed be a useful one.

Dobel himself, however, deals a blow to the attractive simplicity of this theory when he argues that disloyalty does not necessarily lead to corruption. For such development actually to take place, another factor, inequality, must intervene. Dobel's theory could thus be stated as follows: a social group characterized by inequality among its members and members' disloyalty to the group, sooner or later becomes corrupt and eventually disintegrates.

This is, however, just one interpretation of Dobel's never-too-explicit train of thought. Dobel continuously reshuffles the relationship



among his concepts. Sometimes, for instance, he establishes the direction of the causal link among them rather differently, as follows: when the members of a social group are unequal among themselves, they are bound to behave disloyally to the group, *that is*, to behave in a corrupt way. Eventually their behavior will lead to the disintegration of the group. Within this logical framework, corruption again becomes another name for disloyalty and thus loses all its explanatory significance.

A new reversal of logical connections in Dobel's theory of corruption occurs when Dobel draws yet another behavioral pattern into it, factionalism. He then argues that factions result from corruption, which in turn results from inequality; factions, in turn, destroy the members' loyalty to the group which thus eventually disintegrates. Indeed, the study of factionalism from a perspective of corruption could be rewarding and the hypothesis that factionalism is a function of corruption worth testing. The hypothesis, moreover, would not require a redefinition and consequent monopolization of the concept of corruption, which would thus remain available for the study of other unrelated phenomena. Indeed, the traditional concept of corruption could benefit from this new application; since its definition would probably need to be broadened and formulated in terms such as the following: the use of ideological and strategic power (i.e., political power in its widest acceptation) to the furtherance of private (i.e., personal and/or factional) ambitions.

Unfortunately, Dobel does not seem fully to appreciate the analytical power of his hypothesis. Taking a moral rather than a scientific viewpoint (corruption is bad, factions are bad), Dobel uses the hypothesis linking factions to corruption as a stick with which twice to beat politicians. Moreover, in his crusading zest, Dobel loses sight of the causal direction of the relationship corruption-factions, and sometimes he writes as though corruption leads to factionalism, sometimes as though factionalism leads to corruption.

The conclusion of the above discussion must be that Dobel's theory of corruption as a whole lacks internal consistency, although it contains some interesting analytical suggestions. Dobel's theory, however, also pretends to be a *positive* theory of corruption and, to this extent, the test of its external verifiability becomes all the more crucial.

Insofar as Dobel defines corruption as the process of internal decay and disintegration undergone by a "just, equal, and stable socie-

ty," he raises serious questions about the possibility of empirically applying his theory, let alone verifying its validity, since it is arguable that the phenomenon which Dobel's theory is meant to explain has actually never existed and probably never will exist.

A case could, however, be made to the effect that Dobel's theory sets forth the reasons why a "just, equal and stable society" has never come into existence, or, by implication, the patterns of behavior that would need to be stamped out in order that such a society can come into existence. In this respect, Dobel's propositions are not only justifiable but thoroughly commendable. They, however, do not constitute a positive, but a *normative*, theory of corruption; or, better, a normative theory on how to stamp out corruption, stated in a roundabout way. As such, their doctrinal value is probably high, but their scientific value is nil.

F. D. MARENGO

*University of Bath*

### Reply

#### TO THE EDITOR:

Marengo's comments focus on two criticisms: he alleges, first, that the theory of corruption I have formulated is either tautological or internally inconsistent; and second, that the theory as formulated has normative but no scientific value. I shall address these charges in their order of relative importance.

Marengo's progressive reformulations show a basic misunderstanding of the theory. The confusion lies in his insistence that in my view corruption consists in "disloyalty" or "disloyalty of members to the group." My theory, however, never uses the word "disloyalty" nor does it speak of "groups" in any generic sense. I speak, instead, of "states" or "factions" and attempt to present an interpretive theory of historical phenomena. Marengo raises the theory to a level of abstraction never intended and in my view inappropriate. His simple focus on "the group" presumes a closed and exclusive set of loyalties by which disloyalty becomes easily defined and enables him to charge that for me disloyalty becomes synonymous with corruption. His misappropriation of the concept of "disloyalty" and charge of tautology can best be answered by a recapitulation of the role of loyalty in the theory.

Loyalty is a precondition of civic virtue, and civic virtue encompasses the commitments to

and dispositions towards other citizens, symbols and institutions which enable a state to resolve conflicts with a minimum of violence and a maximum of justice and equality. Insofar as a just, stable and equal political order defines the functions of the state and serves as an ideal type in the theory, it is the theoretical background by which other conditions can be evaluated and understood. Civic virtue is a necessary theoretical component of the type; therefore, any realistic expectations of approaching the ends of the state would require the approximation of civic virtue.

Corruption of the state, on the other hand, results as the minimum civic virtue in the state breaks down and leads either to privatization of moral concerns dissociated from public loyalties or factionalizations of loyalty. The importance of loyalty in the theory stems from its status as a necessary condition for civic virtue and, therefore, from its absence or refocusing. Corruption and disloyalty do not collapse into a tautology.

Marengo charges the theory with inconsistency on two counts. First, he argues that in my theory there are unnecessary and reshuffled relations among corruption, inequality and factions. Second, he argues that I confuse the causal direction of corruption and factions, sometimes arguing that corruption leads to factionalism and sometimes the reverse.

I answer that it is necessary to sacrifice "attractive simplicity" to do justice to the phenomena. My theory presents two necessary conditions for the decline in civic virtue through the destruction or refocusing of loyalty and thus, the emergence of the patterns of politics which characterize the corruption of the state as opposed to other forms of corruption. First, by far the most important, is systematic inequality over time (usually intergenerational) in access to wealth, power or status. The second, also significant, is the factionalization of the state. A faction constitutes a specific type of group which possesses internal moral and symbolic coherence and autonomous power. Theoretically, inequality and factionalization are the necessary interrelated conditions and together constitute the sufficient condition for beginning the process of undermining civic virtue and the corruption of the state.

It should be clear that, as with inequality and factionalization, the relation between factions and corruption of the state is an interactive process. As coherent symbolic and moral entities, factions provide alternative foci of loyalty to the state and the common good. The

faction's success in using the state for private purposes leads to a decline in the efficacy of the state's symbols and institutions which leads more people to abandon their civic commitments for privatized or factional concerns. These systematic interactions are vital. The theoretical interpretation of historical and political actions or situations (paradigmatically the United States) is not reducible to billiard ball causality (even billiard ball interactions are not unidirectional) except at the expense of trivializing the issue.

Although I appreciate Marengo's comment that the doctrinal value of the theory is high, I do not agree that its scientific value is nil. His claim that the scientific value of a theory rests upon its external verifiability rests upon an overly narrow and simplistic view of science and the role of theory within it. As most contemporary philosophers of science are well aware, theory performs a number of functions in science, and concern with verifiability and strict causality occurs at a fairly low order of analysis. One purpose of my theory and of similar enterprises is to reconceptualize the problem of political disintegration and provide a general language in which to make sense of the phenomena. The theory brings together a number of seemingly unrelated phenomena and reintroduces other aspects, such as the role of civic virtue. It links together moral aspects stressed by conservative critics with the more structural aspects of radical critics. Thus, it creates a new framework of concepts within which to understand the phenomena of political disintegration and it suggests new linkages and multiple interrelated causes in political life which have been ignored up to this point. This reconceptualization is both prior to and distinct from the generation of hypotheses subject to verification. The latter always arise only within the context of a general conceptual theory. The general theory, like many theories even in the natural sciences, is not intended to be "verifiable" in the positivistic sense Marengo seems to want.

Finally, Marengo's attempt to sunder the normative value from the scientific value oversimplifies the nature of a general framework in political theory. The theory could be read as normative discourse upon an everpresent limiting factor in the attainment of a just, stable and equal political order, and it could be judged, with its accompanying prescriptions, upon its consistency, plausibility and fit with one's moral agenda. But a prescriptive political theory makes little sense unless it identifies a real-world problem and why it exists as a

normative "problem" rather than merely an issue of academic interest. In my theory, the just, stable and equal state simultaneously serves as an ideal type to help locate political phenomena and as a goal by which we can evaluate the desirability of an actual situation. The normative discourse, therefore, must inform the conceptual framework if the theory is to identify areas which generate the most harm and is to provide a more coherent ordering of prescriptions. I do not expect (alas) to "stamp out" the corruption of the state, but my theory may help us cope with an everpresent tendency.

J. PATRICK DOBEL

*University of Michigan, Dearborn*

Comment on Ferejohn  
(Vol. 71, March 1977, pp. 166-76)

TO THE EDITOR:

In the absence of any response from Edward Tufte in defense of his argument (1973) against the criticisms of John Ferejohn (1977), I feel compelled to become a participant in the expanding debate in this *Review* over why competition in congressional elections appears to be declining (or, as David Mayhew and Morris Fiorina prefer to label it, "the case of the vanishing marginals"). While various others, e.g., Robert Erikson, Warren Kostroski, and Walter Dean Burnham, have all been very much involved in this debate, I would like to restrict my comments to Ferejohn's article and, in particular, to his objections to Tufte's thesis that redistricting has made an independent contribution to reduced competition and, hence, increased incumbency of House members.

Ferejohn's objections to Tufte's argument are twofold: (1) it is implausible because there were fewer legal restrictions on gerrymandering before the Court's rulings than there were after its decisions; and (2) Tufte failed to look at changes in the number of competitive districts in states where no reapportionment occurred. If he had, Ferejohn argues, he would have found that the drop in the percentage of competitive seats following reapportionments was not due to redistricting, since the decline occurred in unredistricted areas as well. Ferejohn therefore concludes "that redistricting has had no influence at all on the swing ratio," that "the decline in the number of marginal districts is a general one. . ." (1977, p. 168).

Concerning the first point, I feel obliged to point out that while it is true the courts have

somewhat restricted the ability of state legislatures to manipulate congressional district boundaries, it is important to note that this restriction has extended primarily to the number of people that may be included in a district, and has not affected the shape or compactness of a district. I would contend that the ability of state legislatures to gerrymander districts has rarely been impeded by equal population requirements. Equally as important, however, is the fact that reapportionment/redistricting decisions of the Court in the 1960s awakened a large number of "sleeping dogs." In many states the issue of redistricting was largely dormant prior to the *Baker* and *Wesberry* decisions simply because such states refused to redistrict their House seats. For example, at the time of *Wesberry* there were 13 states that had not redistricted since the 1930s or '40s, or even prior to that. Moreover, rather than undergo the acrimony of redistricting after every decennial census, some states that gained a congressional seat chose to leave their existing district configuration intact, and instead chose merely to elect the new Congress member at-large. The point here is that the Court forced the issue by its actions such that nearly every state had to review and reshape its congressional district scheme in the light of equal population requirements, rather than rely upon piecemeal changes, or, as in many cases, the inertia of inaction.

Ferejohn's second point would lead one to believe that since a decline in the proportion of competitive districts occurred in unredistricted states approximately equal to the decline in competitive districts in states that underwent redistricting, the competitive effects of redistricting were nil. No so. If one is to assess the impact of the independent variable "redistricting," the best way to go about the task is not to use such gross comparisons as the proportion of "competitive seats in states that have and have not been redistricted" at two points in time, as Ferejohn has done. What he has tended to confirm, as have others previously, is that there has been a general decline in competitive seats. But what he has not done is to examine the competitive nature of districts in those states that *have* undergone redistricting, and compare the competitiveness of those districts largely altered in the redistricting process (usually those districts severely over- or underpopulated) with those that were not altered at all, or were only slightly altered.

In an article published at approximately the same time as Tufte's (Noragon, 1973), I employed two research strategies in a study of 32

states and 333 districts in an effort to discover what, if any, trace influences could be detected as a result of redistricting in the 1960s. First, through linear regression I sought to identify those districts most politically (read "competitively") affected through mapping changes from among "recognizable" districts, i.e., those occupying much of the same land mass after redistricting as they did before. I designated 35 districts that had been severely affected. The competitive nature of those districts, based on an adaption of Lewis Froman's index of party competitiveness, both before and after redistricting (through 1970) is instructive:

	Before Redistricting	After Redistricting
Very competitive or competitive districts	66%	29%
Semi-competitive or safe districts	34	71
	100%	100%
N = 35		

While almost two-thirds of these districts were competitive before redistricting, less than 30 percent were competitive afterwards—a rather dramatic reversal. Six "nonrecognizable" districts accrued from the redistricting process (new districts created from largely overpopulated districts). Of these six, four could be termed "safe" on the basis of subsequent election results.

My second strategy was to sort the 333 districts included in the study (minus five at-large seats) into two groups: those that were most malapportioned prior to redistricting, i.e., whose populations varied  $\pm 15$  percent or more from the state norm; and the remainder, which were fairly well apportioned before redistricting. The first group of districts, numbering 158, were those most often, and most radically, carved by the legislative scalpel, and thus constituted the most likely place to seek the effects of redistricting. The competitive make-up of the two groups before and after redistricting is as follows:

	Malapportioned Districts		
	Before	After	Difference
Very competitive or competitive districts	46%	27%	-19%
Semi-competitive or safe districts	54	73	+19
	100%	100%	
N = 158			

	Well-Appportioned Districts		
	Before	After	Difference
Very competitive or competitive districts	37%	31%	- 6%
Semi-competitive or safe districts	63	69	+ 6
	100%	100%	
N = 170			

A comparison of the two groups shows that, although a decline in the proportion of competitive districts is evident in both (with an overall reduction of 11.6 percent), the decline in the more malapportioned subset is much greater: three times as many districts fell from the competitive categories in the malapportioned subset than in the "well-apportioned" subset.

To me, this evidence points to the independent influence of redistricting as a factor helping to account for the decline in competition in House districts, and suggests that Ferejohn was incorrect in dismissing Tufte's argument.

JACK L. NORAGON

*Ohio Legislative Service Commission*

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## Reply

### TO THE EDITOR:

I am somewhat confused by Jack Noragon's comment on my article, which appeared in this *Review* in March 1977. There I concluded that, since the level of competition for House seats

declined in those states that had not been redistricted as well as in those that had, the explanation for this decline probably did not reside in redistricting. Noragon presents two tables apparently intended to call this conclusion into question. First, he shows that in the 35 districts he designates as "severely affected," the level of competition declined after redistricting. Second, he shows that the drop in competition following redistricting was larger in the severely malapportioned districts than in those which were relatively well apportioned prior to redistricting.

Do the data in either of these tables affect the conclusion presented in my article? The data in Noragon's first table are entirely consistent with my argument, while those presented in the second table suggest merely that among the seats that were redistricted there is some systematic variation in the effects of the redistricting. Although there may in fact be such systematic variation, this is irrelevant to the claim advanced in my article: the decline in competition occurs in those districts *that were not redistricted* as well as in the others. Thus, while some of the data presented in Noragon's comment are interesting, I fail to understand in what way they warrant a modification of my conclusions.

I suspect that the questions of interest to Noragon, especially in his earlier article (1973), are somewhat distinct from mine and that this fact has caused some confusion. In his first article he was interested in seeing whether redistricting had an effect, first, on the competitiveness of House seats and, second, on the partisan distribution of these seats. Third, he investigated various factors that might alter the effects of redistricting.

To investigate these issues, Noragon examined the competitiveness of districts before and after redistricting without using a control group of unredistricted districts. As should be clear, I object to the use of this method when the natural competing hypothesis is that all districts underwent changes at the same time. But

perhaps this method retains some validity if one is interested in studying the effects of various factors on the impact of redistricting—Noragon's final set of questions.

Noragon implies in his comment and in his earlier article that two variables had a measurable effect on the impact of redistricting on the level of competition for House seats: the "degree of change in the district" (which might be measured by the malapportionment variable in the second table in his comment), and the partisan conditions surrounding the redistricting process. I believe the data which he reports in his original article do not support firm conclusions on these points and that, more likely, the general decline in the level of competitiveness dwarfed the impact of the other variables that Noragon chose to study.

The following table compares the effect of redistricting on those districts which were, by Noragon's criteria, "politically affected" by redistricting. Those districts were divided into two groups according to how much change occurred in their boundaries; he labeled these districts as "recognizable," or "nonrecognizable" (Noragon, 1973, Tables 3 and 4). Table 1 reports the change in safeness before and after redistricting in the politically affected districts. The data suggest, to me anyway, that, if anything, the effects of redistricting may be contrary to those suggested by Noragon. That is, among the districts "politically affected" by redistricting those districts which were substantially changed by redistricting exhibited an increase in the proportion of safe seats which was small compared to the corresponding increase in safeness in the districts less altered. Of course, the number of cases in this comparison is quite small and one would be unwise to conclude too much from the table. I would think a more prudent course would be to regard the impact of "degree of change in the district" on the decline of competition as ambiguous.

Perhaps more interesting is Noragon's classification of districts according to whether the redistricting had occurred in states in which the

Table 1. The Effects of Redistricting on Recognizable versus Nonrecognizable Districts\*

	Before Redistricting	After Redistricting	Number of Cases
Recognizable Districts	.20	.57	35
Nonrecognizable Districts	.46	.50	26

\*Entries are the proportion of safe districts.

legislature and executive were controlled (1) by a cohesive majority party; (2) those states in which two fairly cohesive parties divided control over the branches of government; and (3) those states in which redistricting took place under conditions in which the parties were relatively weak or in the one-party states. Noragon argues that if a cohesive majority party controls the executive and legislature it will try to increase the size of its congressional delegation by utilizing its supporters more efficiently, and thereby diminish the proportion of safe seats. In either of the other cases, the effect of redistricting should, according to Noragon, increase the proportion of safe seats.

The data presented by Noragon only permit us to observe the percentage of districts that moved out of the marginal category following redistricting. In the states which exhibited control of the executive and legislative branches by a cohesive majority party, there was a net change of 11 seats (of 69 total seats) or 16 percent out of the marginal category. In the other states, a net total of 17 of 183 seats, or 9 percent, switched out of the marginal category. In other words, the decline in competition was, if anything, more pronounced in the states whose legislature and executive were controlled by a cohesive majority party than in the other states.

This result is, it seems, inexplicable by Noragon's own theory and should, I think, cause him to suspect that the phenomenon of the decline of competition for House seats is indeed a general one, and that it had little to do either with whether or not a seat was redistricted or with the conditions under which the redistricting, if any, took place.

JOHN FERREJOHN

*California Institute of Technology*

*Reference:* Noragon, Jack (1973). "Redistricting, Political Outcomes, and Gerrymandering in the 1960s." In L. Papayanopoulos (ed.), *Democratic Representation, and Apportionment: Quantitative Methods, Measures, and Criteria*. *Annals of the New York Academy of Sciences* 219:314-34.

## ON BOOK REVIEWS

Comment on Kahan's Review  
(Vol. 72, December 1978, pp. 1444-45)

### TO THE EDITOR:

The most serious fault with Michael Kahan's (1978) review of my book *Power and Ritual in*

*the Israel Labor Party: A Study in Political Anthropology* is his obvious failure to understand the main conceptual framework which informs the analysis of the work. For example, Kahan states that "most public or quasi-public political gatherings are, by nature, ritualistic; and Aronoff gathered his information almost entirely from such party gatherings." In fact, I devote significant parts of my theoretical introduction and the chapter dealing specifically with ritual in the Labor Party to making a careful analytic distinction between the imprecise everyday usage of the term "ritual" as used by non-specialists (exemplified by the above quotation from Kahan's review) and the more precise anthropological concept which I refine, adapt, and apply in my analysis. I clearly distinguish the unique ritual aspects of the highly secretive closed forum of the Standing Committee (which I was the first and only "outsider" ever allowed to observe), which was characterized by the controlled and bracketed social setting in which it took place, and by the fact that the outcomes were determined in advance. (Cf. p. 75 for an elaboration of the ten major characteristics of ritual in this forum.) I stressed the difference between the ritual character of the Standing Committee and the ceremonial aspects of more open forums such as the national party conference, which lacked important characteristics essential to ritual. Kahan completely ignored and/or failed to understand such important analytic distinctions, which are the basis of the conceptual framework.

Rather than criticize the appropriateness of the conceptual framework I employed, Kahan criticizes me for having failed to employ a more "dialectic" and "metaphysical" approach, which he evidently would have preferred. Yet, by failing to deal with the conceptual analysis I employed and by pushing his own (neo-Marxist?) approach, he distorts and misrepresents my findings. He falsely accuses me of having failed to appreciate the degree to which Labor has shaped Israeli society, and of having treated the party as a "surrogate for the culture" (whatever that means). That this is untrue is indicated in one of my concluding statements: "However, it is clear that in a political system in which the dominant political party has had such a great role in the shaping of the way in which the society has developed, the policies, procedures, and example set by the dominant party and its leaders have been an important factor in shaping and effecting as well as reflecting and articulating the values of the society as a whole" (pp. 176-77). Kahan failed

to understand that my analysis of ritual and other forms of symbolic action were vehicles for focusing on the cultural dimension as it interacts with the major sociopolitical processes which shaped the development of Israeli society and the Labor party. In fact, I did stress the complexity of the mutual influence of social factors on the party, and of the party on the society.

Among the broad fundamental contradictions in Israeli society which Kahan falsely accuses me of having either ignored or ineffectively treated, he stresses the relationship between religion and the state. I quote from my conclusions to a lengthy analysis of the subject: "The *inherent contradictions* between the values of religiously orthodox and secularly liberal ideologies about the very nature of the society and the state are less amenable to 'solution,' even by any expenditure" (p. 93, emphasis added).

Because I hope to have this reply published, I will cite only one more case among the many inaccurate accusations made by Kahan: his claim that I did not go beyond the conclusions of Medding's *Mapai in Israel* (Cambridge University Press, 1972). In fact, I spent a considerable part of chapter 6 and my concluding chapter analyzing why Medding and I came to fundamentally opposite conclusions about the role of democracy in the party. Medding concluded that Mapai was almost a classic case of a pluralistic democratic party which disproved Michel's "iron rule of oligarchy"; I concluded (on this point) that it was in fact very close to Michel's model. Joshua Sinai wrote in the Fall, 1977 *Middle East Review* (10:71): "Dr. Medding's assumptions about the party have recently been challenged by M. J. Aronoff's *Power and Ritual in the Israel Labor Party*. . . . Both books are necessary reading for anyone interested in the organization of Mapai/Labor Party, but Dr. Aronoff's study is the more penetrating." Regardless of whose conclusions one may find the more convincing, it is impossible to understand how any unbiased and conscientious scholar who had read our two books could have made a charge like Kahan's. I leave it to the objective readers to draw their own conclusions.

MYRON J. ARONOFF

*Rutgers University*

Comment on Freedman's Review  
(Vol. 72, December 1978, pp. 1531-32)

TO THE EDITOR:

When Robert O. Freedman wrote his first review of my book, *Red Star on the Nile: The Soviet-Egyptian Influence Relationship Since the June War*, for the *Middle East Review* (Spring 1977), I directed myself to his distortions, inaccuracies, and confused line of analysis in a letter to the editor (*MER*, Summer 1977). I shall not burden readers of these columns with a repetition of those remarks, though they are as germane now as they were then, since his second review (*APSR*, December 1978) is, with the exception of a few phrases and sentences, deleted possibly for reasons of space, the same one that he published earlier.

The submission of the same review to two scholarly journals raises fundamental questions concerning Freedman's professional judgment that say more about the nature of the review than any detailed comments I could provide.

ALVIN Z. RUBINSTEIN

*University of Pennsylvania*

Reply

TO THE EDITOR:

Alvin Rubinstein's ad-hominem attacks aside, here are the facts in the matter. When I was asked to review Rubinstein's book by the *APSR*, I wrote back informing the journal that I had already reviewed the book for the *Middle East Review*, a small journal dealing with contemporary Middle Eastern affairs. I indicated, however, that I would be happy to review the book for the *APSR*, given its different readership, if the *APSR* wished me to do so. The response from the *APSR* was in the affirmative and I prepared the review accordingly.

My approach to preparing book reviews is straightforward. In looking at the audience to which it is addressed, I seek to evaluate the book in question with a view toward helping the reader make a decision as to whether or not he or she should read the book, and/or order it for his or her college (or personal) library. In the book review which I wrote for the *Middle East Review*, which totaled some 1800 words—three times the length of the *APSR* review—I dealt in detail with a number of areas of particular interest for Middle Eastern specialists, such as Rubinstein's analysis of the Israeli

response to Nasser's "war of attrition," Rubinstein's evaluation of the abortive coup d'état in the Sudan in 1971, and his chronological treatment of Sadat's preparation for war in the 1972-73 period. By contrast, my remarks in the *APSR* review were restricted to areas I thought would be of general interest to political scientists, regardless of their specialty within the discipline.

Rubinstein might have done a better service to *APSR* readers if he had responded to my substantive criticisms of his book instead of engaging in attempts at character defamation.

ROBERT O. FREEDMAN

*Baltimore Hebrew College*

#### Editor's Note

The Rubinstein-Freedman exchange requires editorial clarification. When we asked him to review the Rubinstein book in February, 1977, Professor Freedman replied that he had reviewed it already for the *Middle East Review*, but would like to "review it more extensively" for the *APSR*. Our normal policy is to seek another reviewer where our first choice is reviewing the book for another journal. Because I assumed that the *MER* review was to be short and felt that Freedman was the appropriate reviewer for this particular book, I made an exception to this policy. Of course, I expected quite a different review—covering some of the same points perhaps but certainly in different language and, as Freedman had offered, more extensive.

A comparison of Freedman's two reviews shows that my expectations were unfulfilled. The only thing that prevents the two reviews from being identical in all important respects is that the *MER* review is longer. Apparently Freedman simply cut words out of the original review to produce the *APSR* version. Only 68 words (out of about 1150) in the *APSR* review were not in the earlier review, and our manuscript editor was responsible for changing some of them. Furthermore, in my judgment, there is little substantive difference between the two reviews.

No author should be exposed to the same review in two professional journals. For our part (however unintentional) in this affair, we owe Professor Rubinstein our deep apologies. Beyond that, we feel it necessary to reiterate the obvious: our reviewers are expected to submit original work to us. We cannot check reviews to determine their originality, but have

to depend instead upon the professional responsibility of our reviewers.

PAUL ALLEN BECK

*Book Review Editor*

Comment on Liu's Review  
(Vol. 72, December 1978, pp. 1443-44)

#### TO THE EDITOR:

It would be a pity if Byung Joon Ahn's fine book (*Chinese Politics and the Cultural Revolution*) were deprived of the wide readership it deserves as a result of Alan P. L. Liu's unaccountably critical assessment. That review notwithstanding, Ahn's book is quite simply among the best two or three books available dealing with the policy conflicts of the late 1950s and mid-1960s in China. The book is based on thorough and meticulous research in all available documentary and emigre sources and must be considered the most basic and comprehensive account of that period. Not only does the book clarify our understanding of the substantive issues dividing the Chinese leadership, but it also offers a probing and convincing analysis of how decisions are reached in the bureaucratic arena of Chinese politics. It may be true, as your reviewer points out, that many of Ahn's conclusions have now become part of scholarly conventional wisdom about this era. But credit should be given where it is due. It is Ahn's fine scholarship on this period that has contributed most substantially to shaping that current scholarly consensus.

VICTOR C. FALKENHEIM

*University of Toronto*

Comment on Rich's Review  
(Vol. 72, December 1978, pp. 1421-23)

#### TO THE EDITOR:

A recent review faults Robert L. Lineberry for using San Antonio as the site for testing hypotheses about inter-neighborhood service differences, on the grounds that San Antonio is a very poor site for learning about *metropolitan* service distributions. Rich (1978) argues that the arena of competition for life style advantages is now the metropolitan area rather than the central city, and in most large urban areas locational strategies for acquiring desirable



combinations are pursued as often among jurisdictions as within them.

If the question is how to make the distribution of urban services more equitable for citizens of all income groups within a metropolitan area, it is possible that San Antonio may be a very good site for testing hypotheses about service distribution because such a large proportion of the SMSA population lives within a single urban jurisdiction. Thus, Lineberry's site may be representative of the single jurisdiction metropolitan area, which seems to be the goal of many reformers.

San Antonio for decades has ranked among the top two or three metropolitan areas in the United States in the proportion of both total and urban SMSA population within the central city. In 1970 it ranked third among all large (over 500,000 population) metropolitan areas in the percentage of SMSA population living within the central city (76 percent) and percentage of urbanized area population living within the central city (85 percent). As the result of some major annexations in the 1970s, these percentages are even higher in 1979.

Thus, San Antonio seems to be a good approximation of the prototype of a metropolitan area with a high proportion of the population within a single urban jurisdiction. This proportion is attainable for most of the metropolitan areas of the United States. Outside of New England, about 87 percent of the central cities of the 264 SMSAs recognized by the Department of Commerce at the end of 1972 have more than three-fourths of their city boundary adjacent to unincorporated territory, and less than 15 percent of the population of the SMSA is within suburban municipalities whose boundaries adjoin the central city. So, for most metropolitan areas in the U.S. it is still possible to develop the central city into a jurisdiction containing a very high proportion of the SMSA population, thus avoiding the jurisdictional problems allegedly hampering equitable distribution of urban services to all citizens.

This solution is, of course, not attainable for many of the largest SMSAs because they already contain a central city surrounded by large numbers of suburban municipalities. But this is no reason to fault Lineberry's choice of location to test his methodology, which can be applied in the majority of SMSAs.

If future applications of the methodology in such cities as Houston, San Diego, and Jacksonville shows results similar to those found in San Antonio, the case for a single urban government for a metropolitan area will be strengthened. If

the distribution of services in other large cities is as equitable as was the case with San Antonio, we can place more confidence in the view that decentralization is undesirable and that further centralization of urban governments may be desirable.

C. J. HEIN

*University of Missouri, Kansas City*

**Comment on Miller's Review**  
(Vol. 72, September 1978, pp. 1029-30)

**TO THE EDITOR:**

The review by David Miller (1978) of *Justice, Human Nature, and Political Obligation* and *Alienation and Identification*, although intelligent and subjectively fair, contains major inaccuracies in the presentation of my positions, and in particular grossly misrepresents what Miller agrees is a key element in my argument concerning the objectivity of values.

Because of space requirements, I shall merely allude to, rather than discuss, Miller's awkward position on objectivity—which he seems to believe requires agreement among observers—and which I believe a careful reading of my discussion of objectivity in *Justice* could have clarified for him. Miller seems to denigrate hypothetical counterfactual analyses. Yet, for instance, the assertion that Pasteur was objectively correct in his "germ" theory and the French Academy wrong is not dependent upon agreement or its absence either then or now, but upon an assessment of the demerits of the academy's treatment of evidence and its understanding of the conditions of proof. Nor can it be divorced from hypothetical considerations. As Hempel has shown so convincingly, explanations (and, I would add, assessments) are acceptable only if they can sustain counterfactual subjunctive conditional tests. There are no simply "factual" elements in nature. My discussion of Toulmin in the area of epistemology was merely one example of how the methodology of employing counterfactual conditionals clarifies the reifications of common language.

Let us now turn from this preliminary clarification to the point of my letter, Miller's serious distortion of a central element in my defense of the objectivity of values: "[Kaplan's] 'test in principle' presupposes that moral disagreements arise only from imperfect information and/or personal bias on the part of the people making the judgments. It seems likely that there are value differences that cannot be

accounted for in this way." I think it is unlikely that it is accidental, despite my clear and repeated statements to the contrary, that Miller attributes this absurdly simplistic position to me. If his position on objectivity reflects, as it seems to, a position that objective statements require a strong, or even unique, order in the world, then he likely jumped to the conclusion that I must be defending that position in the realm of values.

Perhaps Miller is not sufficiently aware that the concept of a critical experiment—which is intimately related to the concept of a strong order—has been discredited. Many scientific "truths" are so strongly established that they seem to imply a strong order everywhere in the world. However, it is a serious mistake to conflate a few very important areas of science with all of science or logically derived theorems with all of scientific reasoning. For instance, there may be competing but objectively weak solutions to some problems; e.g., whether dinosaurs were ectotherms or endotherms, whether a crystal is living, the implications of the red shift in astrophysics, the first-order disagreement and second-order (symmetrical) agreement concerning which system is moving with respect to which between observers on different inertial systems, the optimal way to design an air-warning system (optimal systems being distinguishable from non-optimal systems even when there are more than one of the former), and so forth. In some cases more information would produce a unique answer, whether with respect to a proposition or competing theories. In other cases, common first- but not second-order symmetrical agreement is ruled out. In some cases, there may be solutions that objectively are logically or empirically equivalent and not in other cases. Yet all these positions are objective.

Note how my argument on values fits into this "continuum." Apart from the fact that I sharply distinguish between first-order value judgments, where I deny the probability of consonance, and second-order moral judgments, where I assert its plausibility, the following points are relevant and are clearly stated. On p. 92 of *Justice*, I state: "Objectivity in the only sense in which it is meaningful, is possible in that realm [of values] also. . . ." I do not say that it is omnipresent, but only that it is possible. On p. 100, I point out that judgments concerning the comparative value of different societies are "weakly ordered" and that "because our judgments concerning values are often low-confidence judgments, they come closer to what the *ancients* [italics added]

called opinion than to [what they called] science." On p. 101, I analogize that because a "color-blind person cannot distinguish some colors . . . does not mean that he can distinguish none." Apart from the fact that it is not clear whether Miller is talking about first- or second-order value differences—a distinction in the absence of which one cannot begin to understand my position—in the realm of values (and in the realm of colors as well) there are neighboring positions that may be indistinguishable or equivalent, although in the realm of values the "scale" is very complex; and differences concerning comparative evaluations of entire value systems and of alternative values within particular systems require different treatment. On p. 103, I ask whether there might be substantial biological differences among individuals such that they have no common second-order framework of values or whether there may be a test in principle for a best physiological type, in which case there would be a common second-order standard of values. Furthermore, I refer (p. 102) to possible areas of evaluation that are "personal or idiosyncratic," and thus that in principle are not objective. (I nowhere call this a "bias"; it would be an irreducible difference.)

This use of objectivity, which I believe conforms better to the actual praxis of science than Miller's, makes objectivity meaningful in the analysis of values. The extent to which human beings inhabit a common (or symmetrical) second-order moral universe is, however, open to inquiry.

There is no space to cover other important topics. Therefore, I can say only that Miller's other critiques are somewhat less than obviously relevant.

MORTON A. KAPLAN

*University of Chicago*

### Reply

#### TO THE EDITOR:

Reviewers are necessarily condensers and simplifiers of the material they discuss, and Morton Kaplan should not complain if I have overlooked some of his qualifications and retractions in order to present his main line of argument as forcefully as possible. Many people will be interested in a defense of the view that value judgments are objective; rather fewer will be riveted by the thesis that perhaps they are objective and perhaps not. Yet to be absolutely

fair to Kaplan, this second thesis is the one I should have to attribute to him.

Distortion, however, is another matter. Kaplan charges me with having simplified his argument for the objectivity of values to the point of absurdity, and with having neglected essential distinctions such as that between first- and second-order value judgments. I believe that the argument is basically defective and cannot be salvaged by any such devices. My reasons follow.

The central issue between us is whether there is sufficient similarity between empirical and moral judgments for the latter to be described as "objective." I hold that objectivity should be predicated on classes of judgment and that a necessary condition of the predication is practical agreement among those making judgments of a particular kind. A judgment such as "The boiling point of alcohol is lower than that of water" can be classed as objective because observers concur in it, or if they do not, they can agree on a procedure for resolving their difference. A judgment such as "Chocolate ice cream is nicer than vanilla" must be classed as subjective, on the other hand, since it neither commands agreement in practice nor is there any way forward for those who disagree.

I do not infer from this, as Kaplan supposes, that the truth of scientific theories depends upon the consensus of opinion among scientists. The connection between scientific theory and empirical evidence is sufficiently indirect to make it an open question at any time which of two competing theories offers the better explanation of a body of evidence. If science as a whole is to be called objective, however, it must be because its theories are ultimately verifiable by reference to a set of agreed-upon empirical judgments, namely, those of common-sense observation. Although Pasteur and the French Academy may have differed over the germ theory of disease, they agreed at least about which patients had the measles, and thus provided the evidence against which the theory could eventually be tested.

The question is whether we can find an analogous foundation for moral judgments. Kaplan appears to concede that there is unlikely to be agreement in what he calls "first-order" judgments—presumably the judgments we make as we go about our everyday business. He thinks agreement may be possible in second-order judgments, made under the conditions of the "test in principle." This involves thinking ourselves empathetically into the various roles offered by different social systems, and then making comparative judgments about the over-

all goodness of each system. How will this alter our first-order judgments? As far as I can see, it eliminates the effects of personal interest and of restricted information, but nothing more. If my favorable assessment of system *A* is governed by my privileged position in *A* and/or my ignorance of the satisfactions offered by alternative system *B*, then undergoing the test may lead me to revise my judgment. But suppose I am a dedicated and well-informed egalitarian who ranks systems solely on the basis of their degrees of equality; then the test will change nothing.

Kaplan suggests that we may at least be able to agree on a weak ordering of systems using the test in principle. Supposing this is so, how does it bear on the objectivity of our judgments? Compare the following two cases: (1) A group is asked to place very similar pieces of wood *A . . . . Z* in order of length; they find they can agree that *A* is longer than *W*, *P* than *D*, etc., but cannot give a complete order. (2) A group is asked to rank varieties of ice cream *A . . . . Z* in order of taste; they find they can agree that *A* is nicer than *W*, *P* than *D*, etc., but cannot give a complete order. Despite the formal similarity of the two cases, their implications are very different. In the first we have judgments that are objective but incomplete; the incompleteness is due to limited powers of sensory discrimination and could be remedied, e.g., by supplying the group with a micrometer. In the second case, the judgments made are subjective but partially convergent. There is no way of converting the weak ordering to a strong ordering, and no guarantee that an individual joining the group wouldn't weaken the order still further by preferring *W* to *A*. Now which kind of weakness is involved when moral judgments about the relative worth of social systems are made? In my view, clearly the second. Moral judgments differ because each person uses a slightly different combination of criteria in making them. Since, however, the number of criteria is in practice finite, it seems likely that we shall be able to describe hypothetical societies *p* and *q*, such that in everyone's book *p* is preferable to *q*. But there is no way in which this can be extended to give a complete ordering of all actual or possible societies; there are no moral micrometers.

Mention of criteria for moral judgments shows that the analogy with judgments about ice cream is not complete. Reasons can be given for our moral assessments and to the extent that others share these reasons, moral argument may be possible. If Kaplan would like to say that this constitutes a degree of objectivity in

moral judgment—and he extends an olive branch with his mention of a “continuum” (I presume between objectivity and subjectivity)—I am happy to accept the compromise. But if lines have to be drawn, I must reaffirm my conviction that such judgments lack the basis in agreement that makes it possible for us to describe empirical judgments as objective,

and thus fall much closer to avowedly subjective judgments of taste. The problem about moral judgments is not merely that they are complex, as Kaplan suggests, but that there are irreducible variations in the criteria used to make them.

DAVID MILLER

*University of East Anglia*

## EDITORIAL NOTE

Paul Allen Beck is leaving the University of Pittsburgh to assume a post at Florida State University. He has served as Book Review Editor since 1976. Professor Beck's service to the Association in this important position is best exemplified by the resolution of commendation passed by the APSA Council:

The APSA Council hereby commends Paul Allen Beck for his superlative service to the Association as Book Review Editor, 1976–1979. Professor Beck's management of this important section of the journal, his judicious selection of books and reviewers, and his advice and counsel in policy matters merit the gratitude of the entire membership.

At one time it was simple enough to manage the book review section from a different location. The sheer volume of work associated with this position now makes it imperative that the book reviews be assigned and coordinated from the journal's main office. I am pleased to announce that Holbert N. Carroll has accepted the appointment of Book Review Editor, effective July 1, 1979. Professor Carroll has served on the faculty of the University of Pittsburgh since 1946, chairing the department, 1960–1968. A person with wide interests in political science, Professor Carroll has written works on Congress and American foreign and defense policy. He served on the APSA Council 1967–1969. The Association will be well served by this acknowledged scholar and administrator.

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With the backlog issue resolved, it has been possible to focus on other matters associated with managing the *Review*. One of an editor's

responsibilities is to point to areas which researchers have neglected and to encourage scholars to venture into new directions. Despite the considerable interest shown by political scientists in public policy research during the last decade, relatively few such manuscripts are submitted to the *APSR*. I recently wrote to a number of policy scholars about this situation and several reasons for it were offered: e.g., not all that much quality work is being done, such research is often supported by government grants and published in reports, many other publishing outlets have recently been developed, and the *APSR* is perceived as not being hospitable to policy research. As editor, I can do little about the first three, but find it unfortunate if the last reason applies. And yet I can understand how such an attitude might develop, given the scope and variety of what is called “policy research.” A useful purpose can be served, therefore, by identifying the type of policy-related study seemingly appropriate for the *APSR*. I prepared the following statement (now slightly revised) for a *Policy Network Directory*, to be published by the Policy Studies Organization. It is offered here to provoke thought, not to establish an orthodoxy.

Articles directed to conceptual and theoretical development and refinement would certainly be of interest to our general readership. Comparative analyses of public policies, major issues, political institutions, policy processes, etc., would also be welcome because they provide the empirical basis for theory and conceptual growth. And single-issue studies too, might be of general interest if designed to test propositions drawn from the work of other policy scholars. Strictly substantive policy analysis—i.e., work designed to evaluate the substantive effects of existing programs and/or

recommend programmatic change—is probably best placed in the several journals now inviting such work.

I also sent this statement to a number of policy scholars for their reaction. For the most part their reaction was positive. Several had constructive comments and as a result I made some revisions in the statement. Two responses were particularly contributory and deserve special mention here. Duncan MacRae, Jr., University of North Carolina, noted that: "There is one type of 'policy analysis' that does . . . belong clearly in political science and this is the choice among political institutions—constitutions, federalism, party systems, electoral systems, administrative structure, legislative institutions, judicial systems, and the like." He also observed that while articles dealing with conceptual and theoretical development are relevant, "It might be useful to specify some of the major lines of development. . . ." Here is what he suggests:

1. Criteria for policy analysis [are needed]. This topic overlaps somewhat with political philosophy, especially when choices of regimes or constitutions are involved. But the region of overlap of criteria, between constitutional choice and program choice, seems to me to be among the most interesting areas: how can similar criteria be formulated for these large choices and for choices among specific programs? Can detailed choices among political institutions be assessed in economic or utilitarian terms? Can criteria, dealing with human development or perfection, be expressed in researchable terms?

2. The role of policy analysts in political systems is another large topic that deserves discussion. It can be generalized in one respect to deal with the proper "politics of expertise," and in another to deal with the evaluation of proposed new institutions, such as "science courts," to make use of policy-relevant expertise. An increasing variety of such institutions is being proposed and requires systematic analysis.

3. Normative analyses of how problems are formulated might be appropriate. Political scientists have contributed a great deal to the study of how issues *are* redefined; but could we take the further step of analyzing how issues *should* be redefined? At first glance, this seems to be mere assertion of the writer's political preferences; but if redefinition could derive from general philosophical systems, take a long view, or contribute to policy analysis, conceivably it could be a contribution to the *APSR*. I suspect that this sort of discussion has occurred more often in "great debates" on foreign policy or international relations than in domestic policy; but I wonder whether this restriction is necessary.

4. Can comparative policy studies be placed in a context of explicit evaluation of alternative choices currently facing a single polity? I have found "comparative public policy" to be a very frustrating field on the whole because of its *lack* of direct contribution to policy evaluation and choice. Explicit attention to policy choices, rather than to non-manipulatable variables alone, might help to remedy this.

5. Careful analyses of "political feasibility" are still needed. I don't mean mere case studies of feasibility; important as they are, I don't think the *APSR* should invite them. Rather, I mean analyses of the scientific versus intuitive or personal status of this topic. Not only the scientific status of feasibility assessment, but also its ethical status should be assessed. Should political science be training feasibility experts as "hired guns?"

This is a particularly rich and stimulating, though by no means comprehensive, list of research issues and is reprinted here for illustrative purposes. It encourages the political scientist to move beyond tightly formulated empirical work to consider its significance in the world of policy choice. It asks as well that scholars draw upon their training in philosophy for identifying the nature and consequence of decisions.

Theodore J. Lowi, Cornell University, asks why the criteria in the statement should not also be applied to other research areas. He writes:

I would urge you to take it one step further and to formulate some of the same criteria on a more general basis for every area in which political scientists write articles and submit them for publication in the *APSR* which were perfectly adequate and acceptable from the standpoint of the subdiscipline in question, but which were too specialized and too narrowly focused to be in the *APSR*. If you had applied the criteria suggested in your September 12 memo to those areas, many such articles would have been sent back to the author with strong encouragement to divert them to the more specialized journals. Why, for example, should the *APSR* be more interested in an article on voting behavior in Congress or in the electorate if it is not "directed to conceptual and theoretical development and refinement?" Why should the *APSR* be receptive to an article on any subject if the article does not "provide the empirical basis for theory and conceptual growth?" You are correct in your position toward public policy papers, and you would be precisely equally correct in applying such criteria to all other areas of our discipline.

Whether a single statement is applicable to all subfields is not something I am prepared to

judge at this point. I was moved to develop a statement for public policy for the reasons stated in my opening remarks. It is my belief, however, that the editor must become as knowledgeable as possible about developments in the many subfields of the discipline. Accordingly, it is my intention to correspond with a

representative group of scholars in various fields to determine how they see the *APSR* serving them, as well as what they judge to be important developments in their particular corner of the discipline. In the meantime, I invite members to write to me or members of the editorial board on this important set of issues.

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The following articles have tentatively been scheduled to appear in the December, 1979, issue:

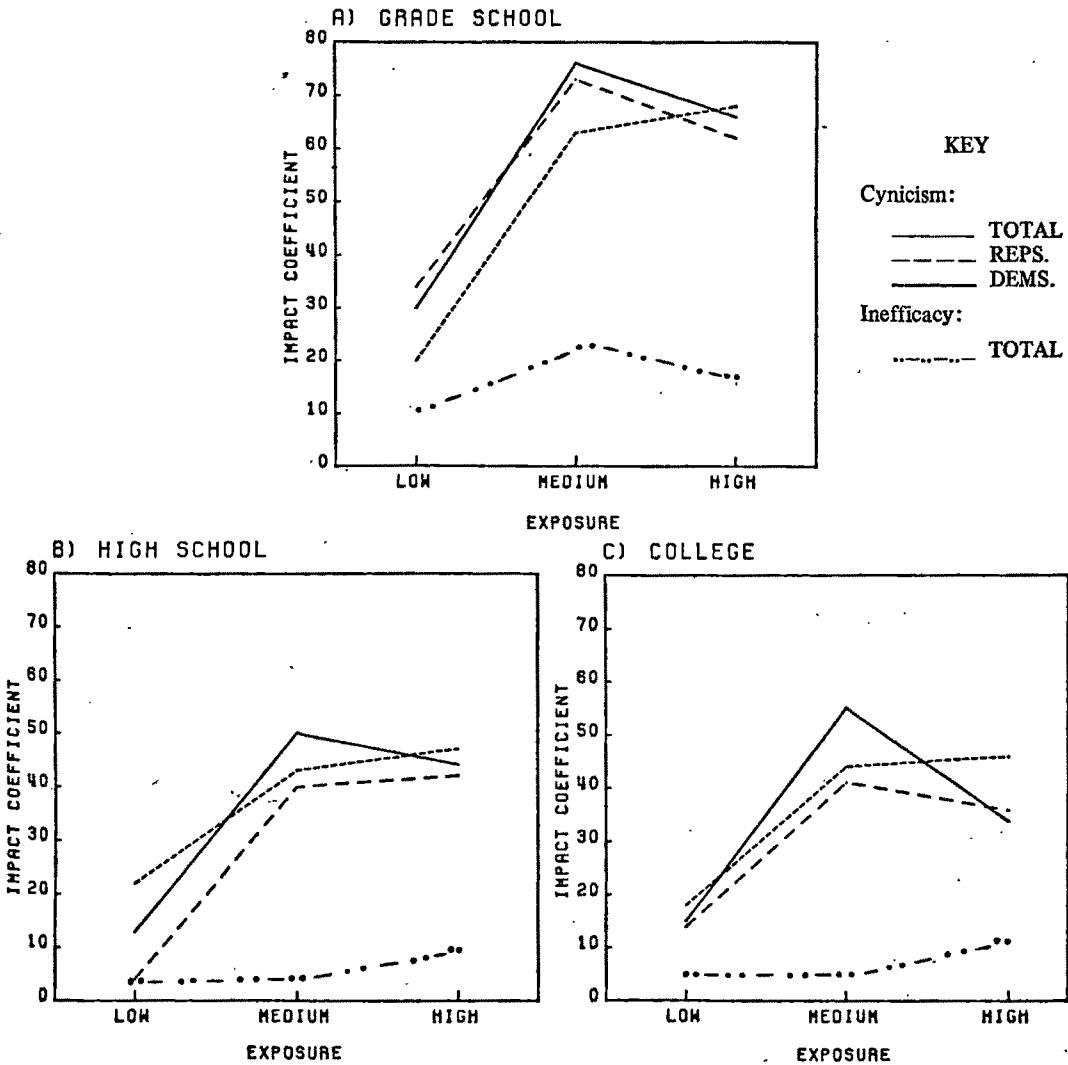
- Ruth Berins Collier and David Collier, University of California, "Inducements versus Constraints: Disaggregating 'Corporatism'"
- Enrique A. Baloyra, University of North Carolina, "Criticism, Cynicism, and Political Evaluation: A Venezuelan Example"
- Robert B. Albritton, Virginia Polytechnic Institute and State University, "Social Amelioration through Mass Insurgency? A Reexamination of the Piven and Cloward Thesis"
- Frances Fox Piven, Boston University, and Richard A. Cloward, Columbia University, "Electoral Instability, Civil Disorder, and Relief Rises: A Reply to Albritton"
- Dale C. Nelson, Fordham University, "Ethnicity and Socioeconomic Status as Sources of Participation: The Case of Ethnic-Political Culture"
- W. Phillips Shively, University of Minnesota, "The Development of Party Identification among Adults: Exploration of a Functional Model"
- Gregory B. Markus and Philip E. Converse, University of Michigan, "A Dynamic Simultaneous Equation Model of Political Choice"
- Benjamin I. Page, University of Chicago, and Calvin C. Jones, National Opinion Research Center, "Reciprocal Effects of Policy Preferences, Party Loyalties and the Vote"
- James H. Kuklinski, Indiana University, and John E. Stanga, Wichita State University, "Political Participation and Government Responsiveness: The Behavior of California Superior Courts"
- Fred Thompson, Economic Council of Canada, "American Legislative Decision Making and the Size Principle"

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#### Erratum

The key was inadvertently omitted from Figure 3 of Arthur H. Miller, Edie N. Golden-

berg, and Lutz Erbring, "Type-Set Politics: Impact of Newspapers on Public Confidence," Vol. 73 (March, 1979), p. 77. The figure, with key, is reprinted on p. 845.



Source: Center for Political Studies, National Election Survey and Media Content Analysis Study, 1974; available through the University of Michigan, ICPSR. Not to be cited without full bibliographical reference to the present article.

<sup>a</sup>The scale for the impact coefficients must be multiplied by .001 to equal the unstandardized regression coefficient. See Appendix B for the complete equations.

Figure 3. Interactive Impact of Newspaper Criticism and Exposure Level on Political Cynicism and Inefficacy<sup>a</sup>

## BOOK REVIEWS

### Political Theory and Methodology

#### Political Philosophy and the Issues of Politics.

By Joseph Cropsey. (Chicago: University of Chicago Press, 1977. Pp. ix + 329. \$22.00.)

This book collects 20 essays written for various occasions between 1955 and 1976. Yet, from beginning to end, there is a unifying perspective bespeaking a settled disposition. The disposition is constituted in "a lengthening experience with the texts [which] persuades me that, however consequential the gap between antiquity and modernity may be, it is not so radical as to have made philosophy itself impossible" (p. viii). That is, Joseph Cropsey finds a persistency in human experience about which it has been possible to think intelligently in every generation, and, further, the result of that reflection in each generation has not been inaccessible in succeeding ones. Consistently with this profession of confidence in political philosophy's constant implication in human experience, Cropsey provides an effective response to the repeated announcement of its demise demonstrating in each essay "benign, sober, incisive, and wise investigation" of the problems for which no age has found unproblematic solutions. To him, trying to dispense with philosophy will only intensify our difficulties to the extent we lose "that skepsis that looks beyond the horizon of modernity" (p. 154), leaving us unable to distinguish founded revulsion at aspects of modern life from unfounded (self-contradictory) efforts to revolt against "human life as men must live it."

The 6 essays comprising the fourth part of the book are particularly striking attempts to test the moderns and ancients against each other. Paying careful attention to the details of the works he discusses, Cropsey gives convincing evidence that, for example, Aristotle anticipated Machiavelli's exploration of mastery; and Hobbes did not so much abandon the thoughts of his great predecessors as revise them in such a way as to inaugurate that series of revisions which constitutes the story of modern philosophy's love-hate relation to what preceded it. An excellent elaboration of this ambiguity is provided in the author's recurrent comparisons of Rousseau's *Emile* to Adam Smith's *Wealth of Nations*.

But the subject of this book is also "issues of politics": conservatism, liberalism, radicalism, modernization, justifying force in the nuclear age, foreign aid, the fate of the family and property. What unites the discussion of each of these is Cropsey's desire to know whether the peculiar crisis of confidence in our time, reflected in policy making that is either guilty about, or disgusted with, "acquisitive egotism," is best diagnosed in terms of "alienated man" (selfish and claiming rights) seeking "identification" (collectivization in the experience of compassion or altruism); or in terms of "stunted man" whose "avid pursuit of wealth evinces a deficiency in the dignity, self-regard, or true self-sufficiency of the man who practices it as a way of life, the man who personifies the moral habit of treating small things as if they are important and is not ashamed to manifest his passion for things wanted. If the judgment of capitalism were undertaken from the standpoint of magnanimity rather than alienation, the verdict would be that acquisitive egotism is offensive to self-regard rather than to altruism" (p. 216).

Nor is there any doubt about Cropsey's inclination in the matter: "It seems to me that mere observation of our contemporary circumstances supports the judgment from the standpoint of magnanimity better than it supports the judgment from the standpoint of alienation" (p. 216). He distinguishes "competitiveness," which is spirited if not "noble," from "acquisitiveness" which is "more objectionable as a sign of deficient than of excessive self-esteem" (p. 216). He concludes: "On a moral question pertaining to the way of life where private ownership is the rule, the weight of presumption is against the less demanding solution of the question. By the less demanding I mean that solution which favors either the principle of individual preference or the supremacy of the antialienation criterion rather than a prescription favoring the moral fortitude of the individual or community" (p. 217).

This conclusion arises from the reflection that alienation is not first produced by competitiveness but by "otherness" without which no one could identify oneself (pp. 44-45). Identifying oneself through the experience of other-



ness leads to assessment of the various experiences of otherness and a discrimination between just and unjust distancings. The question will be, what is a just distance between one and another?—not, is it pleasurable to live in a world of “others?” Insofar as starting from “alienation” obscures the difference between these questions, it is either misleading because it holds out the hope that the just and the pleasant must converge, or useless because it symptomizes a problem without advancing our knowledge of the distinction between the just and the pleasant (p. 49). Needless to say, the inevitability of this issue, both in antiquity and modernity, is manifest in all important debates over both foreign and domestic policies.

In the end, the reader is impressed by the fact that Cropsey's modesty with respect to conclusions is amply compensated for by his elucidation of what is to be thought about, and by his quiet but unswerving refusal to compromise the injunction to go where the argument leads.

TIMOTHY FULLER

*Colorado College*

**Political Language: Words That Succeed and Policies That Fail.** By Murray Edelman. (New York: Academic Press, 1977. Pp. xxii + 164. \$12.50.)

Murray Edelman continues in this new text to explore the ways in which public language is deployed to manipulate belief, evoke emotion, and shape conduct. A vocabulary which one constituency, typically a professional or political elite, applies to “client” constituencies, operates to reassure the latter or temper their grievances or translate their troubles into personal illnesses. To be unaware of the political uses of the vocabularies of science, medicine, education, law, and bureaucracy is to be treated, handled, controlled and confined in manipulative ways. The vocabularies Edelman studies generally depoliticize subordinate populations. Particularly when the vocabulary is based on the ideology of “helping” people with problems, the language, and the treatment it sanctions, implicitly legitimize the conditions which subordinate the populace.

This is an absorbing book. It is carried by its examples. But it also seriously flawed. Edelman refuses to provide theoretical support for this particular interpretation of our public language. He says, “I am concerned with how beliefs

come to be evoked and with their consequences, not their tenability.” But this won't do. Concern with the evocation and consequences of beliefs is also and necessarily a concern with their tenability. If a vocabulary is used to express tenable beliefs and the believers have good reasons for holding them, we do not need to explain further why people express those beliefs through that vocabulary. One example may be helpful here. Edelman (p. 60) says that the habit of labeling the activities which mental patients engage in as dance “therapy,” recreation “therapy,” etc., “treats a common activity as though it were a medical one”; it “superimposes a political relationship on a medical one” by legitimizing the subordinate status of those receiving the therapy. But this reading requires a prior conclusion that the participants are not correctly or sufficiently categorized through these medical terms, that their condition is one which does not require “therapeutic” relationships. For if such a relation is necessary, if it is legitimate, then there would be no necessary political relation hidden within the medical vocabulary. It is because Edelman believes or suspects that medical terms are often used to control a populace which is inadequately comprehended through them that he exposes the political relation inside the medical. But in this instance, as in others, he does not defend an interpretation to support this suspicion. It is merely insinuated into the expose. By exposing all vocabularies and endorsing none, Edelman implicitly endorses the cynical view that all uses of language are thoroughly manipulative. But he cannot accept that thesis thoroughly, or else it would undermine the credibility of his own thesis. To complete his project Edelman must endorse and defend a political interpretation which sanctions the characterization he has offered of professional vocabularies.

I think this is a significant flaw in an otherwise admirable volume. The admirable part is that Edelman has provided new illustrations, rich in detail, for those who would arm themselves against the language games prevailing in the professions and the polity. It is a valuable handbook, especially for those who seek to reappraise the role of the “helping” professions in American society today.

WILLIAM E. CONNOLLY

*University of Massachusetts, Amherst*

**The Future of Technological Civilization.** By Victor Ferkiss. (New York: George Braziller, 1974. Pp. 369. \$12.50.)

In *Technological Man* (1969), Victor Ferkiss concluded that a new human type capable of using technology for humanistic purposes was not only desperately needed, but could be realized. In his new book, he undertakes a very ambitious, but welcome, effort to delineate the foundations for a political philosophy that he believes is needed for the construction of a social order based on a new world outlook that he terms "ecological humanism."

Ferkiss agrees with those that see mankind threatened. In general, he accepts that critique of American society which sees it permeated with alienation, mechanization, and bureaucratization.

The enemy against whom Ferkiss wields his theoretic lance is none other than Liberalism (chapters 1-6). The failure of liberalism lies in its ideology, the Lockean heritage of possessive individualism, and its rampant self-centered aggrandizement. Locke's teaching "bestowed on us a ravaged continent and a disordered society, and threatens us with the loss of our humanity in enslavement to technology" (p. 31). Modification or reform of liberalism, whether masquerading under the form of state capitalism, monopoly capitalism, or interest-group liberalism, will not work. Ferkiss sees little hope in conservatism and socialism, for the alternatives which they offer, he concludes, lead to similar dead ends.

Ferkiss' attack on liberalism provides an impressive synthesis of the increasingly large literature on this topic. Granted, much of his criticism is valid, yet the problems that he points to are common to all industrialized societies, east and west. A major part of our problems may stem from the fact that the very foundations of our modern, technological-scientific world order are intrinsically power-oriented. The drive for power and dominion over nature (and over other people)—part and parcel of the modern outlook—owes as much to Descartes and Bacon as it does to Locke and Hobbes. Ferkiss appears to be setting up a straw man, and in doing so he fails to come to grips with the inner logic of the technological society that has emerged. It is the dynamic, if not demonic qualities of parts of the technological society that Ferkiss will not confront. This is understandable, since he is such an outspoken believer in the positive attributes of technology.

"Ecological Humanism" (chapters 7-14) is the heart of Ferkiss' effort to lay the ground-

work for a political philosophy for the future. He argues persuasively that political philosophy must become deliberately normative, universally relevant, and capable of being brought to bear on the emerging political and social realities in concrete ways. He makes a concerted effort to integrate knowledge of science and ethics, philosophy and politics, economics, history, and ecology—in addressing some of the basic questions revolving around the concepts of human nature, nurture, freedom and values. On the whole, this is the most original and strongest part of the book. Ferkiss takes a positive view of politics. His perception of human nature is basically optimistic, and essentially follows Rousseau. The good life and the good society are possible, and given the right value orientation, can be implemented (p. 127). Ferkiss rejects the argument that the concept of the public interest or the common good is relativistic and basically empty (chapter 13). He maintains that there is a substantive public interest and that its realization centers around the needs of subsistence, order, and purpose. Ferkiss considers the distinction between compulsion and consent arbitrary (p. 53) and maintains that the distinction between human beings as political and social animals is largely meaningless. Freedom is seen not as an end in itself but as a mode of activity, whose purpose is to increase the number of choices open to human beings and the ability to decide what those choices shall be. According to Ferkiss, the primary problem areas to which ecological humanism must address itself are those pertaining to the relationship between humanity and nature, on the one hand, and between humanity and the machines we have created, on the other. Noticeably missing is any overt attention to the category of problems concerning humanity's relationship to humanity.

Two chapters are given over to an explication of what Ferkiss calls "necessary utopia." Ferkiss will not specify the particular means and structures for accomplishing the goals that he specifies. Nor does he provide more than a set of rather general guidelines of what is needed: leadership, planning and political will, political intelligence, and political power. All this is discussed in so cursory and general a fashion that the conclusions give the appearance of being rather obvious, and even trite. Ferkiss' utopian vision lacks the concreteness and imagination that would make it more compelling. He depends upon a persistent and continuing series of hortatory admonitions of what "must" or "should" or "ought" to be done, which turns out to be a poor substitute

for careful utopian thinking.

Two short chapters deal with how to bring about the new political order. In essence, Ferkiss puts his faith in a nonviolent "immanent revolution." Though he denies it, his appeal for a changed consciousness comes at times very close to *The Greening of America*. Think peaceful revolution and you will enter that kingdom! Ferkiss sees the emergence of a new elite, or of great individuals, as the harbingers of the transformation of consciousness. He suggests that those who oppose ecological humanism are beatable (p. 163). As he puts it, "War, pollution and dehumanization could not exist unless it was in the interest of powerful individuals and groups that it be what it is. Obviously then if there is a door leading to escape the keys will have to be wrenched from the hands of humanity's jailers. This means that some sort of political action to free man from the megamachine will be necessary" (p. 79). But no real guidelines are provided to assist in this particular task.

Ferkiss believes that technology is completely controllable by human will and rationality. He fails to understand that every technology has unforeseen consequences. Ferkiss' assumption that "the future is completely open to human decision-making, whether for good or ill" (p. 7), reflects precisely the kind of hubris that has got us into the predicaments to which he addresses himself. His optimism at times strikes one as naive. The ecological humanistic revolution "will be an 'open conspiracy,' an unstructured, unorganized movement for reform" (p. 270). Shades of the progressive movement! The immanent revolution will take place because it must (i.e., ought to) take place. Given our knowledge of human history, such optimism really seems unfounded. We are offered a politics of hope, and in these gloomy days, that is most welcome. Nonetheless, one needs to be careful not to be ensnared by the siren song of misdirected optimism. We will need a more rigorous and radical critique to chart us through the stormy waters ahead.

JOSEPH HABERER

Purdue University

**John Locke and the Theory of Sovereignty: Mixed Monarchy and the Right of Resistance in the Political Thought of the English Revolution.** By Julian H. Franklin. (New York: Cambridge University Press, 1978. Pp. xi + 148. \$15.95.)

If this volume were intended for physical rather than mental consumption, its labeling would surely be controversial; it is not primarily about John Locke's theory of sovereignty and resistance. Fewer than eight pages are predominantly devoted to explicating Locke's views on these topics (pp. 91-98). Although this non-comprehensive treatment of the subject is supplemented in the course of comparisons between others and Locke, the result remains an incomplete study of Locke's pertinent views. Indeed, the protagonist of the work is George Lawson, not John Locke. Ultimately, however, rather than being a study of any one theorist, this book is a study of the development of the concepts of sovereignty and resistance throughout the seventeenth-century Parliamentary/Whig tradition. It focuses on the problem of resistance to sovereignty in a mixed constitution.

Franklin frames the problem as it was inherited from sixteenth-century constitutionalists. The monarchomach tradition advanced a right of resistance to monarchs who flagrantly violated the conditions upon which they received the consent of the community, the source of all legitimate authority. This popular constituent power, however, was viewed as resting in the governmental body by which the people were represented. Logically, though, such a locus of the constituent power is incompatible with mixed monarchy, for the king's sphere of independent power within such a government is thus made potentially subject to revocation by the Estates. Resistance is preserved, but mixed monarchy implicitly falls. Nevertheless, circumstances permitted ignoring this implication.

The English Civil War ended the possibility of such obscurantism, since the king's opponents found themselves in the position of justifying a war against him while insisting on his independent authority. Parliamentary publicists Henry Parker, Charles Herle, William Prynne, William Bridge, Jeremiah Burrough and Philip Hutton are each shown to involve themselves in implicit or explicit contradictions, as they inevitably assert both parliamentary supremacy as constituent power on behalf of the people, and some version of independent royal power in a mixed monarchy. (I say "inevitably" because most,

perhaps all, theories are presented as retrospective attempts to rationalize an already established set of goals. Indeed lacunae in positions are filled on this principle: "It is hard to believe, then, that Lawson would have held a principle that was of little value to his cause and universally repudiated by persons of his social rank" [p. 128].)

George Lawson solves the Parliamentarians' dilemma. The constituent power lies in the community. This entity is created by the consent of its members to form a government. The community retains its constituent power even after it has established a government. Thus resistance to royal tyranny by the community became compatible with royal independence within a mixed government.

What of Locke? "He had found the solution ready-made in Lawson. But he put it in a form and language that made it impossible for his own and later generations to ignore" (p. 123). Franklin's discussion of Locke on sovereignty and resistance is limited in scope and form to supporting the thesis that it was taken over from Lawson. Locke's treatment of the nature and limits of the right to disobey a bad—as opposed to a tyrannical—law is ignored (*A Letter on Toleration*, in the Tegg edition of *The Works of John Locke*, Vol. 6, pp. 43–44). Yet this, rather than his right of revolution, is the basis of his concept of civil disobedience.

Great stress is placed on the concept of the community in Locke, for this is essential to identifying his position with Lawson's. While this concept figures in Locke's explanation of the formation of government, the preponderance of his consideration of the dissolution of government suggests a return not to community but to the "loose state of nature" (*The Second Treatise*, in *Works*, Vol. 5, pp. 464, 465, 469–70, 473, 476). Franklin deals with this difficulty for his thesis only by referring to "two or three misleading phrases in Chapter XIX" while comparing William Atwood to Locke. Could Locke's thoughts on the matter be more complex than a mere borrowing from Lawson? The possibility deserves serious consideration.

The value of this work lies in its development of the Parliamentian position on sovereignty and resistance, especially that of George Lawson. It should be read for this. But no one should be deceived by its title into expectations of intensive Lockean scholarship.

FRANCIS EDWARD DEVINE

*University of Southern Mississippi*

**Die Krise des Amerikanismus: Revolutionäre Ordnung und gesellschaftliches Selbstverstaendnis in der amerikanischen Republik.** By Jürgen Gebhardt. (Stuttgart: Ernst Klett Verlag, 1976. Pp. 337. DM 68.)

Jürgen Gebhardt, a West German political theorist, has written a remarkable book about the American civil theology. Bringing his knowledge of the history of political philosophy to bear upon the problem of American political self-interpretation, Gebhardt has produced an original and sympathetic account of the American political style. Particularly refreshing is Gebhardt's appreciation of the intellectual and spiritual quality of the political thinking of the Founders. There is here none of the ignorant contempt for American ideas and institutions apparent in the writings of, say, Martin Heidegger. At the same time, Gebhardt's book reflects the philosophical depth we have come to associate with the best of German thought. Like de Tocqueville, Gebhardt helps us to see ourselves more clearly by virtue of the fact that from his cultural distance he can make out the extent to which Lockean contractualism permeates our thinking with at times seriously distorting effects. Gebhardt does not make the mistake of labeling Lockeanism as our only political theme, however. Like Robert Bellah he also stresses the importance of the classical republican and biblical motifs.

For Gebhardt, the American Revolution was inspired by a genuinely revolutionary consciousness, itself the result of a merger of classical republicanism and Christian millenarianism. At the same time, the Revolution did not derail into what Eric Voegelin has called metastatic fanaticism; it kept its balance through a strong admixture of "common sense" as understood by the Scottish Enlightenment thinkers. Gebhardt's thesis—that, as purification, awakening, and reformation, the American Revolution was genuinely revolutionary—is, of course, controversial and deserves to receive considerable attention and serious debate. Some may wonder at the extraordinary emphasis he places upon John Adams—to the comparative neglect of James Madison and Thomas Jefferson, for example—but whatever particular reservations one may have, there is much to learn from Gebhardt's fresh and arresting interpretation of Adams.

According to Gebhardt, the "crisis" of Americanism is the emergence of the atomized self and the gradual disintegration of the "public philosophy." The Lockean, individualistic, acquisitive mentality gains the upper hand

over republicanism with its commitment to the common good and biblical religion with its commitment to "one nation, under God." The remarkable economic and material advance of our country has been bought at the price of almost destroying the very social fabric which made such an advance possible, he contends. Periodic attempts to "return to the origins" of the Revolution and inspire a new ethical-political awakening are discussed as well as the difficulties they encounter.

Although Gebhardt's study will be of particular interest to students of American history and politics, it also deserves to be read by political scientists concerned with the general problem of the role of a society's "civil theology" in undergirding its political system. Richly interdisciplinary in his approach, Jürgen Gebhardt has succeeded in writing a book about the United States which is both politically concrete and philosophically profound. There is here none of the philosopher's abstractness: Gebhardt is highly knowledgeable about American history and institutions. Let us hope it will be translated for a larger public. At that time the typographical errors in the English quotations can be corrected.

DANTE GERMINO

*University of Virginia*

**Reason and Morality.** By Alan Gewirth. (Chicago: University of Chicago Press, 1978. Pp. xii + 393. \$20.00.)

*Reason and Morality* is an ambitious effort to reunify the "is" and the "ought," and to reunite analytical metaethics with traditional normative discourse. Alan Gewirth seeks to formulate a general theory of action, and to deduce therefrom both a rational justification for morality, and a universally applicable moral imperative. In attempting so huge a task, he has produced a system so abstract and obscure that its impact risks being limited to the stoutest adherents to analytical philosophy. Yet its content is inescapably political; Gewirth's system synthesizes many of the traditional issues of political philosophy, including rights and duties, obligation and dissent, the nature of the state and of voluntary associations, punishment, and welfare policy.

Gewirth asserts that nothing more than a definition of action and the rules of logic are needed to derive both a justification for morality and its specific normative requirements. To do so, he develops what he (rather ponderous-

ly) calls the "dialectically necessary method," which involves examining the logical implications of claims and judgments that are necessarily attributable to every agent. The key to this process is his definition of action. Action has two generic features, voluntariness or freedom, and purposiveness or intentionality. These features alone logically commit all actors to accept the moral principles derived from them. Behind this claim lies the conviction that the structure of action, because it involves the voluntary pursuit of purposes, is inherently normative. By this definition, and the dialectically necessary method, Gewirth claims to have bridged the gap between the "is" and the "ought," by demonstrating that the "ought" is implicit in the "is," the concept of action. He has not made an empirical bridge from the "is" to the "ought," but an analytic one.

Every agent, by virtue of engaging in action, is logically committed to accept the moral imperative on pain of self-contradiction. The reasoning is as follows: (1) Every agent judges his own purposes to be good, and hence judges as good the freedom and well-being that are the necessary conditions of his acting to achieve his purposes. (2) Every agent necessarily makes a deontic judgment claiming the right to freedom and well-being. (3) Since he makes this claim by virtue of nothing more than being an agent, he must accept the generalization that all prospective purposive agents have rights to freedom and well-being.

The controversial rights claims contained in the second and third steps merit further explanation. Since the agent considers his freedom and well-being good, he concludes that others ought not interfere with them. This "ought" is understood to mean "others have a duty not to interfere with them." This duty implies a correlative right or entitlement. This right does not rest on the existence of any community, government, or deity, nor on any particular attribute of the agent. Hence, any agent has the right to freedom and well-being by virtue of nothing more than being an agent. This last step allows Gewirth to universalize the rights claim. If one acknowledges that all agents have the right to freedom and well-being, one commits oneself to the Principle of Generic Consistency. This PGC requires that the agent refrain from harming or coercing any recipient of his action, and that he assist those who need assistance in achieving freedom and well-being if he can do so at no comparable cost to himself. This, then, is an egalitarian universalist moral principle, since it requires an equal distribution of the most generic rights. The PGC is both logically

and normatively necessary, and is the supreme moral principle.

The final two chapters work out applications of the PGC for individual and group interactions. For political scientists, the most interesting aspects will be the indirect applications of the PGC developed in the final chapter, reflecting Gewirth's long-standing interest in political philosophy. Indirect applications are those in which the PGC is used to evaluate social rules and institutions governing multiperson activity. If the rules are thus morally justified, they are then binding on persons to whom they apply even when they burden freedom and well-being. Social rules may be justified either as extensions of freedom (procedural justifications) or as contributing to well-being (instrumental justifications). Procedural justifications apply to institutions based on consent, such as voluntary associations. The state, in contrast, is justified instrumentally, as an institution necessary for well-being; the existence of the state need not be based on consent. However, to be consistent with the PGC, a state must have a constitution which ensures consent in the selection of officials and of public policy. Gewirth argues that the body of criminal law ("the minimal state") merits different justifications from laws concerned with welfare functions ("the supportive state"). The former is logically mandated by the PGC and does not require consent; the latter are not strictly derivable from the PGC and thus must be based on consent. Readers are likely to find the political applications appealing, although some conclusions are counter-intuitive, such as Gewirth's insistence that to violate the criminal law is to contradict oneself. Overall, the insights of this chapter finally reward the persistent reader.

Persistence is indeed required to struggle through this dense book. Even the reader who enjoys metaethics will not find *Reason and Morality* pleasure reading. Gewirth's style is so turgid that it would bring tears of mirth to the eyes of Jeremy Bentham. He invents more principles than even John Rawls dared; in his hands, the Golden Rule becomes "the principle of appetitive-reciprocal consistency." Nothing is said clearly if it can be said awkwardly, ponderously, and with multiple noun clauses that assault the English language.

The content of Gewirth's argument will provoke considerable controversy, especially the logic by which he derives rights claims. Whether the logic ultimately stands or falls, it should promote serious thinking about the nature of rights and obligations. In summary, this forbidding book offers a significant contri-

bution to political thinking. If its import is not recognized, the discipline will be the poorer, but at least part of the fault will lie with an author who intimidates rather than invites his reader to follow his difficult path.

BETTE NOVIT EVANS

*Creighton University*

**Social Science and Utopia: Nineteenth-Century Models of Social Harmony.** By Barbara Goodwin. (Atlantic Highlands, N.J.: Humanities Press, 1978. Pp. ix + 220. \$22.50.)

The subject of this book is four thinkers who believed they knew the social arrangements that would make people happy. To my mind, the idea that happiness is a condition, and furthermore, that it is a condition that can be established once and for all, approaches the ludicrous. One would have thought that happiness was simply not like other ends for which the institutional preconditions or embodiments could be known or plausibly conjectured, for example, freedom, national power, or the absence of poverty. Happiness comes as it can, and quickly goes. Surely there is something humorless—perhaps, even, sinister—in laying down detailed prescriptions for happiness, for happiness as a more or less steady state. Yet Barbara Goodwin sees Godwin, Saint-Simon, Fourier and Owen in this light, and tries hard to prevent us from judging their enterprise as intrinsically foolish.

Goodwin is not especially interested in showing that these thinkers' practice-minded utopianism sets them apart from most earlier writers commonly considered utopian. After all, it is one thing to imagine a happy world, but quite another to offer the unhappy world, in a completely literal-minded way, a blueprint for a happy world, with the hope that at least a few will start to live the happy life someone else has imagined for them. These literal utopians were not content merely to expand the feeling of possibility or to sting the world with their reproaches. They were out to change the world by force of argument. Goodwin only touches on this distinction between her four subjects and most of their predecessors. Failing to take fuller notice of this point, she therefore cannot account, in a thorough enough way, for her subjects' overall distinctiveness in relation to the body of utopian speculation they inherited. The four writers, children of the Enlightenment, as Goodwin rightly says, were also witnesses to the beginnings of industrial

capitalism. That means they were possessed by the sense that unless something radical was done, the great benefits deriving from the growth in human capacities would be accompanied by great horrors. Perhaps the horrors would be greater than the benefits. (I do not mean to say, of course, that this sense was present with equal vividness in the four.) Desperation thus was mixed with hope; each intensified the other. The upshot was blueprints. For us, the upshot must in truth approach the ludicrous. A bit of historical empathy could help to stifle our derision; once we are past derision, however, is there a reason for spending some time with these writers?

Goodwin's announced strategy is to take these four seriously because their enterprise was intimately related to the growth of modern social science. It is an odd strategy, even though Saint-Simon had a large role in this ambiguous endeavor. Still, even if the endeavor were not ambiguous, and even if all four had played a large role, that would not guarantee that we now should take them seriously—that we should read and ponder them.

There must be a better reason to read them than their possible link to modern social science. What may we learn from them, even if we reject the aim of happiness? They may be full of thought about human nature and the relations between social institutions; they may contribute to our sensitivity to injustice; they may energize the wish to change and reform; they may even give hints about the direction which change and reform should take. Best of all they may give impressive examples of utopianism's most valuable effect: an enhanced understanding of the actual world which is brought about by the typical utopian perspective of estrangement from it. If we forgive them their passion to prescribe pedantically for happy lives, we may allow them to instruct us as non-literal and ironic utopians do—like, say, Plato or More or Swift or Samuel Butler.

It turns out that Goodwin, despite her title, is mostly interested in the utopian wisdom of the four writers. The link to modern social science is discussed, but not at much length. Her basic concern is with what Godwin, Saint-Simon, Fourier and Owen have to say about the human condition. She does not see their commitment to the project of human happiness as barring them from achieving some insight into human experience. She suspends her disbelief. She says, "These utopians were materialists, naturalists, perfectibilists and neo-Newtonians: they believed in material causes of social events, believed that society and men were natural

objects to be studied by methods analogous to those of the natural sciences, believed in human perfectibility, and longed to unearth a principle of universal harmony operating in society with the efficiency and totality of Newtonian attraction" (p. 11). The usefulness of this book consists in showing how on such a morally unattractive and philosophically crude base four thinkers helped to form a new sensibility. They challenged individualist liberalism (itself still only partly formed) in the name of hope for a life more fraternal or collectivist or, at least, less competitively self-seeking. That sensibility establishes their claim to the attention of posterity. The project of human happiness inflamed by an awful dose of scientism should defeat such a claim, but does not quite.

GEORGE KATEB

*Amherst College*

**Marx's Social Ontology: Individuality and Community in Marx's Theory of Social Reality.**

By Carol C. Gould. (Cambridge: MIT Press, 1978. Pp. xxvi + 208. \$15.00.)

In her introduction Carol Gould claims to present Marx "for the first time as a great systematic philosopher in the tradition of Aristotle, Kant and Hegel" (p. xi). Though this claim is obviously exaggerated, *Marx's Social Ontology* is certainly the first work by an American to deal systematically with the *Grundrisse*, the voluminous notebooks translated into English in 1973. Gould's essay is coherent, well written, and should be accessible to the non-philosopher.

The first chapter, "The Ontology of Society," shows persuasively that for Marx the fundamental entities of society are individuals in social relations, that it is a mistake to treat society as a simple composite of individuals or as a complex network of relations. Gould shows the importance of the Hegelian dialectic in Marx's understanding of social development and does a masterful job of reducing great quantities of material to a few pages. Though with Hegel Marx speaks of various stages of history, his "projection does not have the force of a logical deduction or of a historical prediction" since the transition from one set of social relations to another depends on human choices and actions (p. 21). By developing these themes in terms of "internal relations," Gould successfully corrects earlier kindred accounts of Marx and points to the latter's appreciation of Aristotelian ontology.

In her second chapter on "The Ontology of Labor," Gould reconstructs Marx's view of labor as social self-creation and explains how in capitalism the objectification of the self common to all social systems becomes an alienation in which individuals lose control of their creative powers. In the *Grundrisse*, Marx makes clear that this change is to be explained in terms of political economy (the necessity of selling one's creative capacity, one's labor-power) and that a psychological or anthropological account of the phenomenon will mistake symptom for underlying disease. Gould shows how Marx's model of alienation and objectification, initially drawn from Hegel, is again modified by an Aristotelian emphasis on the reality of the individual and the independence of the object.

Chapter 3, "Toward a Labor Theory of Cause," is perhaps the most technical, but also the most important for social scientists. Gould shows Marx to have an original understanding of social causation which makes any mechanistic interpretation of his work totally inadequate. Marx locates causality not within the domain of natural objects or events, but in the realm of human activity, with labor again serving as the active mediator between form, purpose, agency and the objective conditions for the actualization of our intentions. Since the forms of human activity are historically variable for Marx, the ways in which causality appears and is understood vary from one historical stage to another, until finally under capitalism it appears as an external relation producing invariable patterns of behavior. Gould's analysis is especially important in that it could provide a link between continental thought and contemporary analytical work in the natural and social sciences (von Wright's analysis of human activity and the discovery of causal principles comes to mind).

In chapter 4, "The Ontology of Freedom," Gould distinguishes Marx's view from the traditional view of freedom as the absence of external restraints or as a property inhering in the subject's will. Freedom becomes both a presupposition and a product of our activity, the source of value and the highest value as an end in itself, "a creative, self-transcending, historical, self-realizing" social process, which accounts for newness in history (p. 127).

In the final and longest chapter, "The Ontology of Justice," Gould argues that Marx's theory of social reality presupposes a conception of justice understood in terms of concrete forms of social relations. Though Marx insists on the historical character of our norms, he is

neither an historicist, nor a relativist. In analyzing relations of domination and the "mutuality" characteristic of a just society, Gould adeptly uses the category of reciprocity, so increasingly central to contemporary social theory. Capitalist relations of production presuppose free and equal agents, who exchange equivalent values in the market (wages for labor-power), but they disguise non-reciprocal relations of exploitation in the sphere of production, where workers surrender control of their creative capacity and ownership of the products generated when that capacity is put to use. The just society would allow for positive freedom, the fullest self-realization of social individuals who recognize each other as equal not only in a formal and instrumental way, but as ends in themselves.

Questions can be legitimately raised about any of several notions found in Gould's work: non-reciprocal, internal relations or ex post facto necessity, for example. But to ask them in a short review might leave the impression that we were speaking of analytical shortcomings. *Marx's Social Ontology* is a philosophical reconstruction of Marx's entire system. Questions will inevitably arise about its adequacy, but answers to these questions will provide insight into that system and gratitude to the person whose work has provoked them.

GEORGE ELLARD

*Dickinson College*

Leon Trotsky. By Irving Howe. (New York: Viking Press, 1978. Pp. viii + 214. \$10.00; New York: Penguin Books, 1979. Pp. viii + 214. \$2.95, paper.)

Leon Trotsky has fascinated generations of non-communist Western intellectuals for half a century partly because he seems to embody both the triumph and the tragedy of the Russian Revolution. The early opponent of Lenin helped plan the Bolshevik insurrection, create the Red Army and win the Civil War; the vacillating head of the "August bloc" became Foreign Minister, architect of "war communism" and Lenin's preferred successor; the cultured intellectual who stood for workers' power and inner-party democracy defended the accomplishments of the revolution against a usurping party-state bureaucracy; the outsider who had arisen to the pinnacle of power became the hunted exile; the "superindustrializer" relentlessly exposed Stalinist totalitarianism while urging his scattered and isolated



followers to extend their "critical support" to the Soviet Union. All told, Trotsky is often presented as one of the towering heroes—and one of the most poignant victims—of history.

But "history is a bitch," remarks Irving Howe ironically at the end of his latest contribution to the voluminous literature on Trotsky—a sophisticated "political essay" whose occasional superficiality is more than offset by the kind of insights one has come to expect of him. The "Modern Masters" format does not cramp his style. Howe manages to say what he wants to say.

He presents Trotsky as a man of great intellectual abilities and considerable personal courage whose fundamental, if understandable, tragedy was rooted in his obstinate refusal to reexamine—and renounce—his fidelity to Bolshevism. Too doctrinaire for his, for Russia's and for the world's own good, Trotsky is sharply criticized by Howe on four grounds: his defense of the October insurrection itself (whose very success made inevitable the systematic application of terror by a minority party suddenly risen to state power and determined to transform the face of Russia and the world); his refusal to share power with other parties in an all-socialist government on the questionable presumption that only the vanguard Bolsheviks represented the "long-run" interests of the workers; his strict adherence to democratic centralism even while he was advocating freedom to form factions and defending what remained of inner-party democracy; and his stubborn defense of the Soviet Union whose socialist foundations remained intact despite their bureaucratic "deformations." Howe argues that it was this tragically inappropriate fidelity to Leninism which paralyzed whatever Trotsky might have been able to do in defense of the Revolution and democracy.

But this, the core of Howe's analysis, is precisely his most vulnerable claim—and the book's value resides chiefly in its presentation of a popularly held, if highly questionable, position. To take one example, Howe says that the theory of "permanent revolution," the "going-over" of the bourgeois to the proletarian revolution, was a decisive contribution to Lenin's own thinking. But there is abundant evidence that Lenin was not indebted to Trotsky on this score and that the related projections concerning the relationship between the peasantry and the proletariat and the possibility of building socialism in Russia in the absence of successful European revolutions had little, if anything, to do with Lenin's thinking. The same might be said for such issues as the

Brest-Litovsk treaty, the role of the trade unions, the New Economic Policy, the nature of the Communist party, the meaning of the phrase "hegemony of the proletariat," the "national question," and others.

All the leaders of the modern communist movement have tried to answer the challenge of constructing a Marxism which remains international in content even as it takes national forms. That they have not all done so in the same fashion or with the same success is clear. Trotsky proved no more equal to the task than most others, and Howe's portrayal of him as an overly rigid Bolshevik engaged in heroic combat with Stalinism—the very incarnation of evil—does not help the reader approach the deeper issues of Trotsky's work. It is an interesting work for all that; and, more than an examination of Trotsky, it also illustrates the dilemmas which have confronted anti-communist socialists since 1917. In this sense, *Leon Trotsky* is about the theoretical problems of its author at least as much as those of its subject.

JOHN EHRENBERG

*St. John's University, New York*

**Pride and Solace: The Functions and Limits of Political Theory.** By Norman Jacobson. (Berkeley: University of California Press, 1978. Pp. xv + 166. \$10.00.)

How does one review for an academic journal a poetic, frequently metaphorical essay, a work filled with startling images and sometimes outlandish linguistic constructions? One cannot look exclusively for logical consistency, because, as Norman Jacobson claims, poetry is not logical. Nor can one deal with the accuracy of the sometimes novel and imaginative interpretations of Machiavelli, Hobbes, and Rousseau, for, again, the author states that he is looking for meaning rather than truth, presumably as much in his readings of the works of others as in his own life as a political man. Political theory for the author is persuasion mixed with intimations, intimations "of what persists within the political consciousness of Western humanity" (p. xiii). And it is this which the author tries to offer us in his rapid and sharply focused survey of Western political theory from Machiavelli's state to Camus' rebel. It is a persuasive and powerful plea to reject the solace of uncertain truths, while accepting the uncomfortable solace found in the writings of Orwell, Arendt and Camus. Theirs is not the solace of truth and order, but the solace of

those who have experienced and put into words the tensions and the nightmares of being human, but who nevertheless refuse indifference and who retain compassion.

Machiavelli, Hobbes, and Rousseau do not satisfy this craving for solace which Jacobson feels. The solace they give is tainted and one-sided. They give people the solace of order, but take away their humanity. Machiavelli's order is based on the conquest of inclinations to be good, Hobbes' on the amputation of private consciences, and Rousseau's on the deception of the legislator. In Jacobson's analysis of what he describes as three founders of the modern state, his interest focuses frequently and most importantly on the relationship between subjects and rulers, between the Italians and their prince, between the fearful members of Hobbes' civil society and their all-powerful sovereign, between the natural man made citizen and the legislator. It is the legislator, the Moses of political thought, who provides solace for his subjects, solace in the form of order and security, and, in Rousseau, having the additional support of a sense of justice and equity. Jacobson's thesis, though, is that the nineteenth century destroyed this relationship and left political beings in a pre-Machiavellian condition, "deprived of the solace of mystery, authority, or pre-ordained destiny, alone, groping [their] way through a labyrinth of uncertainty" (p. 129).

The argument is broad and consequently ignores or does not deal with contradictions or questionable details. In order to emphasize points, to paint with striking chiaroscuro, Jacobson oversimplifies and obscures tensions which would undercut the power of the picture. Can we really compare post-Rousseauian man with pre-Machiavellian man and simply ignore the impact of both Christianity and the Reformation? With regard to individual authors, broad generalizations again dominate. *The Prince* is a grand plea to Lorenzo, or really the seduction of the politically ambitious man; the seriousness or the scope of Machiavelli's plea remains unquestioned. *The Leviathan* presents Hobbes as the Creator, creator of a new man bound to the sovereign through language and a fear of himself, but oblivious of the concepts of obligation and representation. Rousseau's general will is the union between the state of nature and civil society, but is there not a far more radical and complex transformation entailed in the move to civil society than simply an uncertain and insecure unity between the two? But then again, one is called back; what else is the role of art but to focus on one

element of human nature, to the exclusion of the rest, in order to sharpen one's understanding and awareness of what we ordinarily do not see in the mix of complexity? On this level the book is a jewel.

On a lesser scale there are minor inaccuracies which offend but do not necessarily detract from the overall effect. For instance, Hobbes did not write a work entitled *Body, Man and Citizen* to which Jacobson refers on p. 67. The major quotation which he uses comes from part I, chapter 9 of *The Elements of Law*. Furthermore, Hobbes' first literary project was not a translation of Homer (p. 91); it was a translation of Thucydides. This does not destroy Jacobson's claim that Hobbes was a lover of heroic poetry and as aware as Plato of the power of poetry, but it does create some concern in readers who, while being pulled along by the poetry, do not want totally to relinquish their hold on history.

This is not a scholarly book in the limited academic sense of having accurate footnotes (which are largely absent until the last chapter and the discussion of the twentieth-century authors); nor is it a scholarly work in the Greek sense of the term—something which emerges from one's leisure time. It is a passionate book written by one who feels the need to confront, deal with and then put into words the dilemmas, as he and Camus see them, of modern political beings who must set limits and retain their humanity in a chaotic world not in the least conducive to these goals. The quibbles which one may have about particulars in the book often pale before its rhetorical and poetic power.

ARLENE W. SAXONHOUSE

*The University of Michigan*

**The Study of Politics: A Collection of Inaugural Lectures.** Edited by Preston King. (London: Frank Cass, 1977. Pp. xiv + 322. \$27.50.)

Inaugural professorial lectures are British occasions without an exact American analogue. Nevertheless, we have ceremonial events for which statements of intellectual faith are expected. Participants in such events will be sympathetically interested in the 16 inaugural lectures delivered by British professors of politics and collected in this volume. Others may find the book revealing as academic history. Newly appointed British professors ordinarily present their plans for future study and devel-

opment. Such plans are collegial as well as personal when professors become departmental heads.

Preston King, the teacher of politics who edits the volume, has included certain frequently cited and controversial essays, notably Michael Oakeshott's "Political Education" and Isaiah Berlin's "Two Concepts of Liberty." Harold Laski, Ernest Barker, Denis Brogan, G. D. H. Cole, K. C. Wheare, and Max Beloff, established British scholars who became well known in the United States during the second quarter of this century, are also represented, as are the slightly later figures of J. C. Rees, Howard Warrender, J. H. Burns, W. H. Greenleaf, M. M. Goldsmith, A. H. Birch, Maurice Cranston, and Bernard Crick. Of the 16 lectures, 1926-1971, four come from Oxford, three from the London School of Economics, and two from Cambridge. Thus the three places with earliest politics chairs are well represented in the collection. There is no suggestion that the other seven lectures represent British political studies, or even British professors, at all of the remaining three dozen or more universities where politics is taught. Conspicuously absent, for example, are lectures from Strathclyde and Essex, where (along with Nuffield College, Oxford) something like American behavioral political science has developed. Perhaps professors at these universities have not given inaugural lectures.

Whatever the reason for the omission of the now-familiar American style, much can be said for a volume that emphasizes an approach to the study of politics that is primarily philosophical and historical. Neither the exponents nor the restive critics of that approach are likely to deny that it has been dominant in Britain during almost all of the first half-century of politics as an academic subject. Its ascendancy may slowly be drawing to a close. Certainly many British scholars have now forsaken the old essay-writing tradition for rigorous quantitative analysis as well as for other empirical research methods. But good reasons remain for reading work in the traditional mode. Several of the philosophical lectures are important in their own right; Oakeshott's and Berlin's, and also Cranston's "Politics and Ethics" lecture of 1971, are full statements of important ideas. Crick's lecture in 1966 is a characteristically lively response to Berlin's. Moreover, the collection is itself a useful contribution to intellectual history. It tells us something about the British reluctance to treat politics outside of a humanistic tradition. The very name "political science" is usually

avoided, or, now and then, treated as an inappropriate American usage. Max Beloff reacts explicitly against behavioral methods; otherwise such methods tend to be ignored.

The editor's introduction directs attention to a change over time in the nature of the inaugural lectures. The early ones, before 1951, try to mark the boundaries of what is still a new academic discipline, and the later lectures, taking the boundaries more or less for granted, are devoted to the professors' own particular subjects. Accordingly, even within a strongly humanistic tradition, there are signs of the growing specialization that has become so familiar within American political science and indeed within all of our established academic disciplines.

It would be wrong to conclude this review without noting exceptions to the generalization about the volume's concentration on the philosophical and historical tradition in the study of British politics. K. C. Wheare's lecture is one of a few contributions reflecting a concern with the study of governmental institutions. More striking, however, is the scope of intellectual interests displayed in Harold Laski's inaugural lecture at the London School of Economics in 1926. Although he declared his own main interest to be the history of political ideas, Laski also asserted, in discussing what was known about the House of Commons, "that in the place of large-sounding generalizations that have never been tested we badly require quantitative answers..." (pp. 11-12). Premature British behavioralism?

LEON D. EPSTEIN

*University of Wisconsin, Madison*

**The Essential Tension: Selected Studies in Scientific Tradition and Change.** By Thomas S. Kuhn. (Chicago: University of Chicago Press, 1978. Pp. xxiii + 366. \$18.50.)

It is now over 16 years since Thomas Kuhn's *The Structure of Scientific Revolutions* was first published. During this period Kuhn's views have been the subject of a continuing, often virulent debate, but in spite of the hostility it has aroused in some quarters, the book has had a deep impact on our understanding of "science" and what is involved in the scientific study of politics. His latest book, *The Essential Tension*, is a collection of essays, 12 of which were published between 1959 and 1976, and 2

unpublished essays written in the same period. Together these essays provide an overview of the development of Kuhn's ideas, several studies in the history of science, and an amplification and clarification of some of the ideas developed in *The Structure of Scientific Revolutions*.

Judging from the reception of Kuhn's earlier work, the essays that will be of greatest interest to political scientists may well be his restatement of the concept of a paradigm in "Second Thoughts on Paradigms," his clarification of the role of values and "subjectivity" in theory choice in "Objectivity, Value Judgment, and Theory Choice," and his critique of Popper in "Logic of Discovery or Psychology of Research." These essays, together with his "Postscript" to the second edition of his *Structure of Scientific Revolutions*, show that Kuhn's account of science and scientific revolutions does not have the irrationalist implications often attributed to it. In other essays Kuhn continues to argue that significant changes in a scientific theory, including the rejection of one theory and its replacement by an entirely new one, cannot at the time be fully controlled by the evidence, or completely determined by any rules of rational scientific procedure. But this does not mean that scientific change is a matter of "mob psychology" or anything of the sort. On the contrary, such decisions must be justified: "Scientists may always be asked to explain their choices, to exhibit the bases for their judgments. Such judgments are eminently discussable, and the man who refuses to discuss his own cannot expect to be taken seriously" (p. 337). The bases for these decisions are scientific values such as accuracy, consistency, scope, simplicity, and fruitfulness. These values are not so well-defined that they can dictate the choice between two theories, but this is a virtue rather than a defect, because the practice of science requires the possibility that rational people may disagree about what theories ought to be accepted, permitting work to proceed on competing theories. By this means, premature closure is avoided, and the consensus that results as one theory increasingly comes to prove its worth vis-à-vis its rival can be seen to be a rational consensus.

In "Second Thought on Paradigms" Kuhn provides much-needed clarification of what has come to be one of the most overworked terms in our metascientific vocabulary. Here Kuhn argues that "paradigm" should not be used to refer to the whole set of elements which a discipline (or school) shares, and which accounts for the relatively unproblematic nature

of communication within it. This set should be thought of as a "disciplinary matrix," and Kuhn offers a valuable discussion of three crucial elements of any disciplinary matrix, including symbolic generalizations, models, and exemplars. "Exemplars . . . are concrete problem solutions, accepted by the group as, in a quite usual sense, paradigmatic" (p. 298). Thus, Kuhn proposes to return to his original, and basic sense of "paradigm," and offers a stimulating and important discussion of how such exemplars function in scientific practice.

Although Kuhn's more philosophical essays are apt to attract the most attention from political scientists, his reflections on and his studies of the history of science should also be of great interest. In his preface and in the historical and historiographic studies, Kuhn stresses the hermeneutic aspects of his field, the need for historians to understand the theories they are studying in terms of the concepts and principles possessed by the scientists who originally formulated them. In these essays we see Kuhn as a fellow social scientist seeking to understand a form of human behavior—the practice of science. As political scientists attempting to understand the political actions of men and women, our position is not unlike that of the historian of science, for our first task must also be an interpretative one, to grasp the sense these actions make in terms of the actors' own self-understandings. Unfortunately, although suggestive, Kuhn's discussion of what such interpretation involves is very brief and sketchy. In particular, he does not relate the problem of interpretation faced by the social scientist, or historian of science, to the problem of interpretation faced by the natural scientist. Where social scientists must make sense out of the behavior of their subjects, natural scientists must understand theories that have been formulated in terms of an alternative "paradigm" (in the broader sense of the term). On the face of it, both of these situations appear to raise similar issues of mutual understanding and intelligibility—problems of meaning, translation, incommensurability, and so forth. These issues have long been of concern to social scientists, historians, and philosophers who have studied the problems of the social sciences and humanities, and so it can be hoped that there is room here for valuable cross-disciplinary work. Indeed, after a long period in which social scientists have turned to the philosophy of natural science for guidance, it may well be time for them to contribute to the solution of problems within that field. The essays collected in this volume are a contribu-

tion to that enterprise.

J. DONALD MOON

Wesleyan University

**The Spirit of Liberalism.** By Harvey C. Mansfield, Jr. (Cambridge, Mass.: Harvard University Press, 1978. Pp. xii + 130. \$13.50.)

This book contains six essays. Three are primarily book reviews: one of Lowi's *The End of Liberalism*, one of Reich's *The Greening of America* and Dahl's *After the Revolution?* and one of Rawls and Nozick. One was a bicentennial *pièce d'occasion* on "The Right of Revolution," while the remaining two lament the decline of contemporary liberalism.

Mansfield speaks of the reader "catching his tone" (p. ix) and this is indeed distinctive: "feline" is I think the word. ("Waspish" would be too gross a pun.) Although a constant diet of irony and innuendo becomes cloying, some of Mansfield's effects are worthy of A. E. Housman himself, the master of us all in the gentle art of academic bitchery. For example, in Mansfield's review of *After the Revolution?* he sets things up by observing that the book is a "Yale fastback" and quoting the publisher's flackery for this concept. He then notes that Dahl crosses "the boundary between empirical political science and political philosophy . . . thus finding himself, so to speak, in the ladies' room" and continues: "It may merely be that he who writes a Fastback has no time for the fact-value distinction. . ." (p. 62).

Occasionally, the mask slips, and Mansfield starts to sound like a character invented by Kingsley Amis, as in his nice thought that busing will merely speed up the process by which the Negroes (as he calls them) acquire the middle-class virtues while "the white middle-class suburbanites, especially the young ones, . . . adopt some of those ways of the Negro which derive from his former status as slave—a slouchy posture, a shuffling gait, shiftless habits, gaudy attire, throbbing music, and the various addictions" (p. 36). Or is this another mask? Having sent up everybody else, is Mansfield sending himself up here? Who knows? Does even he?

Reviewing the book is a hazardous business because of the element of what Nixon and his friends called "deniability": any rendering of its content must take account of what is

insinuated as well as what is stated, but how does one then reply to the charge of "I didn't say that?" Having admitted that risk, I shall try to summarize the main contentions by saying that Mansfield combines the Agnew diagnosis of the media with the Kristol diagnosis of the academics: for Mansfield both are guilty of biting the capitalist hand that feeds them, and they are joined in Public Television, which excites his special contempt. The enemy is "West Coast university and New York literary activism" (p. 50), or more concretely, the New Left and the radical feminist movement (pp. 22–27). Members may be readily identified as men trying not to be gentlemen (p. 14) and women pretending not to be ladies (p. 26).

But what is Mansfield actually in favor of? He is, he says, "a friend of liberalism" (p. vi). Failing a revival of the thought of "the ancient writers who [by denying that all authority is artificial] constitute the true alternative to liberalism" (p. 50), one can still choose between its "moralistic" and its "mundane" tendencies. Mansfield prefers the latter and lauds the tough-minded liberalism of Locke and Madison for its frank recognition of the force of self-interest in human affairs, and for its analysis of large-scale societies as necessarily divided into an active elite and a passive mass. Thus, Lowi "does not face the necessity of social aristocracy in our liberal society and hence does not appreciate the necessity of disguising it in our liberal democracy" (p. 38). Tocqueville rides again: the egalitarian "spirit of democracy" must ever be opposed to the elitist "spirit of liberalism."

How did contemporary intellectuals go so astray? Mansfield, missing the point of Rawls' theory, which uses the notion of constrained maximization to make a moral argument, accuses them of being concerned with the lot of the disadvantaged out of a self-deluding fear of being among the worst-off (pp. 100–01). The "unassertive 'apathetic' many" (p. 9) have rather simple desires for material goods and "living as they please" (Earl Butz put it more vividly) and demands for equality or participation made on their behalf are merely a bid for power by radical elitists (pp. 14–15). Intellectuals "have been more democratic in their demands than the democrats" (i.e., the untalented mass) and have "lost their sense of community with businessmen" (p. 14). But are freedom to accumulate and freedom to publish indissolubly linked? Mansfield offers no serious argument for this. Copyright is, indeed, one form of property (p. 13), but any reasonably spry intellectual of leftist inclinations should be

able to dispose of that little debating ploy.

BRIAN BARRY

University of Chicago

**On the Social Contract, with Geneva Manuscript and Political Economy.** By Jean-Jacques Rousseau. Edited by Roger D. Masters and translated by Judith R. Masters. (New York: St. Martin's Press, 1978. \$12.95, cloth; \$4.95, paper.)

Roger and Judith Masters, who have already placed us in their debt with a fine edition of Rousseau's *First and Second Discourses*, now increase that obligation with a new version of *The Social Contract*, to which they have appended the *Discourse on Political Economy* and (more importantly) the first complete English translation of the "First Version" or "Geneva Manuscript" of *Du Contrat Social*. The translations are extremely faithful, though they perhaps abandon a little of the grace and elegance that Frederick Watkins managed to preserve in his equally reliable 1953 rendering of *The Social Contract*; and Roger Masters' introduction is admirably wide-ranging and comprehensive for so brief an essay, though it is perhaps, on occasion, a little elementary for those already acquainted with Rousseau ("Rousseau . . . represents the generation which rejected the principles of the old order without being able to overthrow it . . . this general movement is often called the 'Enlightenment'"). This is offset, however, by a most judicious treatment of Rousseau as popular-sovereignty theorist, and still more by a skillful elucidation of that central but elusive Rousseauian notion, the general will—a notion that Roger Masters rightly calls the "most notable" of Rousseau's enlargements of our political vocabulary.

It is because the "notability" of Rousseau's *volonté générale* is so clear to Masters that he has seen fit to include the *Première Version* or "Geneva Manuscript" of *Du Contrat Social*, the significance of whose second chapter he properly insists on. In that second chapter, which Rousseau suppressed, together with the rest of the *Première Version*, Rousseau takes up Diderot's argument (from the *Encyclopédie* article, "Droit Naturel") that there is a "universal" general will of and for the entire *genre humain*—a universal *volonté générale* which "never errs," and which is based on "reason" in "the silence of the passions" (a phrase that Diderot borrows from Malebranche). In the "Geneva Manuscript" Rousseau flatly rejects

this universality: "There is no natural and general society whatever between men; . . . it is certain that the word *human race* presents only a purely collective idea to the mind, which presupposes no real union between the individuals who comprise it." And Rousseau, having insisted that what is *général* for Sparta or Geneva is *particulier vis-à-vis* the rest of the world, finishes by complaining of "pretended cosmopolites" (e.g., Diderot) who, "justifying their love of country through their love of the human race, boast of loving everyone, to have the right of loving no one." Roger Masters gives full weight to this important manuscript in his introduction, and fleshes it out with valuable "editor's notes."

It is in one of these notes that Masters makes his only seriously disputable claim—namely that the notion of "general will" was used before Rousseau, not just by Diderot, Malebranche and Fontenelle (as is true), but also by Pufendorf. In his tenth "editor's note" to the "Geneva Manuscript," Masters, apparently following de Jouvenel and Hendel, suggests Pufendorf as an early shaper of *volonté générale*; but here Masters, like Hendel, appears to have been misled by Barbeyrac's French translation of Pufendorf into thinking that the notions of *volonté générale* and *particulière* are really to be found in *De Officio Hominis et Civis* (1682). What is unmistakably plain is that Masters, like Hendel, cites Pufendorf neither in the original Latin nor in an accurate English translation, but only in Barbeyrac's French version of *De Officio Hominis*, which was published in 1718 as *Les Devoirs de l'Homme et du Citoyen*. Now Barbeyrac used the notions of *volonté générale* and *particulière*—developed, initially in theological writings of Pascal (*Écrits sur la Grâce*), Arnauld and Malebranche—in "rendering" *De Officio Hominis* into French; his version is a transmutation of Pufendorf, not a mere translation. Thus Pufendorf's plain phrase, "if each subjects his will," becomes in Barbeyrac "if each subjects his *volonté particulière*"; and Pufendorf's straightforward "the will of all, collectively and singly," is turned by Barbeyrac into "the positive *volonté* of all *en général* and of each *en particulier*." So Barbeyrac's filtering of Pufendorf's quasi-Hobbesian thought through the prism of French theology makes him look like a precursor of Diderot and Rousseau—but only in French.

This, however, is comparatively unimportant; what matters is that Roger and Judith Masters have appreciated, and given to us in a fine English version, one of Rousseau's most important and neglected political writings. For

this one can only be grateful.

PATRICK RILEY

University of Wisconsin

**The Critical Theory of Jürgen Habermas.** By Thomas McCarthy. (Cambridge, Mass.: MIT Press, 1978. Pp. xiii + 466. \$19.95.)

Jürgen Habermas must be accounted one of the most suggestive and interesting thinkers currently at work in the social sciences. Yet understanding him is no simple matter. His thought grows out of an exacting tradition. A systematic and speculative thinker in the style of German idealism, he assumes his program and methodological convictions from neo-Marxism, particularly the Frankfurt School critical theory of Max Horkheimer and Theodor Adorno. His prose is often opaque, joining the inscrutable allusiveness of Adorno with a jargon-riddled verbal eclecticism that is uniquely his own. His system is imperial in ambition and encyclopedic in scope, recasting other theories in its own mold, and requiring of the reader some acquaintance with not merely Kant, Fichte, Hegel, Marx, Dilthey, Weber and Freud, but also C. S. Pierce, H. G. Gadamer, Talcott Parsons, Wittgenstein, J. L. Austin, John Searle, Noam Chomsky, Jean Piaget and Lawrence Kohlberg. Constantly assimilating new theories and ideas, largely through a steady stream of critical essays, Habermas is always expanding, refining and reformulating his system, which he has come to call, with misleading modesty, a "research program." This vast—and growing—program has already generated a vertiginous superstructure of propositions that are *prima facie* implausible: for example, the claim that the relations within groups pursuing political enlightenment ought to be analogous to the therapist-patient relationship in psychoanalysis, or the hypothesis that the history of the species passes through universal stages of cognitive and moral development analogous to the stages of individual development observed in advanced industrial societies. Still worse, the avowedly programmatic status of the system makes evaluation treacherous. Faced with an unfinished "research program," criticism runs the risk of seeming premature, interpretation the risk of seeming one-sided, if not obsolete.

Despite these formidable obstacles, Thomas McCarthy has managed to write a relatively lucid book that is unquestionably the most cogent account in any language of Habermas'

work to date. His professed aim is "to provide a reliable framework," from a "standpoint internal to Habermas's thought," for "the critical reception of [his] work in the English-speaking world" (pp. x, xi). But what we get is something more than a loyal and sophisticated apologetic. We also get a tacitly authorized "rational reconstruction" of Habermas' system—a restatement that, in virtually every respect, stakes its claims far more clearly and plausibly than anything Habermas himself has ever written.

This painstaking reconstruction is not light reading. For those who have never struggled through the original, it will not be of much help (they might start instead with Richard Bernstein's account in *The Restructuring of Social and Political Thought*). For those already acquainted with Habermas' work, however, McCarthy's book is indispensable.

What began as an attempt to justify and extend the critical theory Habermas inherited from Adorno has become an ambitious effort to show the existence of universal standards of truth and right. The foundation of this effort is the theory of communicative competence. It claims that "the human interest in autonomy and responsibility is not mere fancy, for it can be apprehended *a priori*," by elucidating the necessary structure of language (p. 287). To this "quasi-transcendental" theory, he conjoins (among other things) an account of ego development, a "rational reconstruction" of cognitive development in the species, and an analysis of capitalist "legitimation crises" designed to disclose latent social tendencies to redeem the claims of rational autonomy and discursive validity implicit in every speech act.

If one swallows all this whole, it would appear that both moral and epistemological relativism have been refuted, and that something like a neo-Marxist critical theory has finally been justified, without recourse to that discredited *deus ex machina*, the proletariat. Certain formal yet substantive concepts of truth and right are embedded in the very structure of competent speech, and this structure is unambiguous enough that "there is only one correct theory" of its phylogeny and ontogeny (p. 265). Accepting this account allows the comforting thought that history is a progressive "learning process" writ large, with a rational moral *telos* immanent in the language through which learning occurs. At the end of history (at least as a counterfactual "narrative foil") stands "the ideal speech situation," where uncoerced consensus emerges through unlimited dialogue about fundamental norms.

Clearly, these are bold claims that merit serious consideration. This McCarthy has given them. He convincingly dissects the early theory of "deep-seated anthropological interests," deftly untangles the subsequent theory of communicative competence. If these theories still do not carry conviction, it is not McCarthy's fault. One could not ask for an abler or more informed philosophical defense.

My main reservation, indeed, concerns McCarthy's tendency to mute his own misgivings. Although he is clearly uncomfortable with Habermas' surprisingly uncritical treatment of systems theory and Kohlberg's scheme of moral development, he confines himself to several sketchy and scattered paragraphs of comment. A little more historical context would also have helped: since the work of Horkheimer and Adorno is only discussed in passing, the false impression is left that Habermas virtually invented the idea of a "critical theory."

Minor criticisms aside, McCarthy has handsomely accomplished his principal aim. Moreover, the glosses on Habermas' critiques of behaviorism, functionalism, role theory, phenomenology, ordinary language philosophy and hermeneutics should be of value to anyone concerned with the philosophy of the social sciences. At the very least no one interested in critical theory can afford to ignore McCarthy's book. It is an impressive achievement.

JAMES MILLER

*University of Texas, Austin*

**Game Theory and Political Science.** Edited by Peter C. Ordeshook. (New York: New York University Press, 1978. Pp. xii + 627. \$28.50.)

An abstract gold medallion, designed by the French artist Georges Mathieu in 1944 and entitled "Game Theory," is imprinted against the blue background on the cover of this volume. What would a medallion for political science look like? We are not given much of a clue. Eleven of the 19 essays in this volume have the word "game" in the title; none has any direct form of the word "political." My overall assessment of this volume is that the appropriate logical connective for the title is "or."

So much for my particular concern for the lack of any systematic focus on political (or even social) problems throughout the volume. Since we do not have a book which attempts to make game theory more relevant to politics through a reorientation of the former (Schelling

is virtually alone in having broken this barrier) or to develop a unified approach to model building, we must evaluate this collection of essays through piecemeal evaluations of each article. Most of the "big names" in both formal political science and applied mathematics are here. Their primary contributions are generally reserved for journals, including this one. Thus what we have, for the most part, is a set of "remaindered" essays.

The articles in the first section, "Descriptive Theory," are either summaries or elaborations of the authors' other work in this area. Ordeshook and McKelvey and Harsanyi offer new solution concepts for cooperative and noncooperative games, respectively. The former article presents little that is new and the utility of the competitive solution still has to be demonstrated; Harsanyi's formal development is interesting (an extension of Bayesian decision theory to  $n$ -person games), but is so heavily dependent upon the notion that one or more players will make a strategic *mistake* that its applicability is unresolved. The essays of Lucas and Maceli and Schofield are also more relevant to game theory than to applied social science, although the former's discussion of the different types of domination may prove useful in the analysis of coalitional behavior. Robert Rosenthal offers a "model of cooperative games with binding commitments," a fascinating applied piece which unfortunately does not consider the prospect that a group may fail to provide a collective good. The Ferejohn et al. article is a direct application of game theory to legislative behavior, with supporting experiments. This is the most impressive piece in the first section, but it is marred by the authors' unnecessary assumption that a majority rule equilibrium within a legislature must be equal to an electoral equilibrium for each of the candidates; have they forgotten about patterns of districting?

Experimental studies (second section) often are so preoccupied with the details and procedures of the experiments that the underlying problem is almost lost. This problem is particularly acute in Vernon Smith's article, which finds some support for the optimal provision of public goods regardless of group size. Laing and Olmstead report a complex series of experiments which finds reasonable support for the McKelvey-Ordeshook competitive solution; their own ad hoc model does remarkably well, however, and it makes fewer predictions on outcomes. Isaac and Plott also employ a spatial approach to committee decision making under a "closed rule" (no amendments allowed); they



offer a useful critique of bargaining set predictions and find that their subjects tended to select the outcome which was in the core of the voting game. Cores generally do not exist, but the Ferejohn et al. article in the first section suggests that this may be more of a problem for game theory than for committee decision makers.

The most interesting contributions are in the final two sections of the book, "Applications" and "Value Theory and Applications." The most ambitious and controversial article is an extended essay by Brock which employs supergames to "derive" a "new theory of social justice." This is not completely novel, as Taylor has tread this ground in *Anarchy and Cooperation*; however, Brock maintains that game theory can prescribe a set of ethics—and a rather conservative one at that. The other contributions on value theory are concerned with power indices. Owen (in apparent ignorance of the other contributions) and Roth develop rather abstract axiomatizations of the Banzhaf and Shapley power indices. Straffin, like Roth, also follows the lead of Dubey in axiomatizing these power indices, but provides much more straightforward interpretations in terms of key issues in democratic theory and gives most useful examples. This essay is a compendium of his results published in many places and should have served as a model for many other contributions. Brams and Lake present a slightly different axiomatization, also with political illustrations, but their model assumes that each outcome in a voting situation is equally likely and its utility is thereby limited. The other contribution to this section, by Shubik and Young, develops the nucleolus as a noncooperative game solution. Its primary interest in this volume is to provide a basis for Young's model of lobbying presented five chapters earlier. The latter contribution opens new ground in a field not sufficiently developed (although entrepreneurs have been studied). The present model needs to be extended to more than two lobbyists to be of interest to most students of politics. Young should concentrate more on developing the model of lobbying than in comparing the power of legislators who have been lobbied with those who have not (the Shapley and Banzhaf indices reappear).

The "Applications" section, in addition to the Young article, contains three important articles. Hinich demonstrates that the assumptions we make about voters' preferences (whether loss functions are quadratic or computed by absolute values) and their probabilities of voting at all determine whether the mean

or the median will be the equilibrium point for a slightly modified spatial model of voting. Kramer presents a more difficult argument in establishing a theorem that maintains that under quite general conditions both candidates in a two-candidate election will have optimal *mixed* strategies and that when candidates are advantaged, optimal pure strategy (for the candidate or for the opponent) may exist. Kramer does not discuss how the optimal mixed strategy is to be operationalized. McKelvey and Howard Rosenthal develop a rather appealing model of electoral coalitions in French *apparetements*, one of the most politically relevant models to employ game theory, but then test the model (albeit too briefly) only with predictions from classical game theory. Not surprisingly, the results are mixed. The work of DeSwaan should motivate future testing of the coalitional model.

The text of the volume is set by photo-offset on inexpensive paper. There are typographical errors throughout the book and Figures 1 and 2 in the Ferejohn et al. article are nowhere to be found. Only in the printed preface is there any concern for margins and, of course, there is no index. These cost-saving devices reduce the price to \$28.50, which is still considerably less expensive than other books which use far fewer mathematical symbols. Will esthetic considerations turn away potential purchasers? The press seems to have gambled that they will not. Yet I doubt whether I would purchase the book myself. In contrast to some of the more expensive volumes, it is not only in the margins that this book is uneven.

ERIC M. USLANER

*University of Maryland, College Park*

**The Golden Lands of Thomas Hobbes.** By Miriam M. Reik. (Detroit: Wayne State University Press, 1977. Pp. 239. \$15.95.)

The Golden Lands of Thomas Hobbes is a biography, or rather, an intellectual biography; for, as its author points out, Thomas Hobbes did scarcely a thing throughout his 91 years besides think, write, and converse. With so little of an active life to record, Miriam Reik devotes the weight of her study to the impact of Hobbes' mature work on such contemporary types as jurists, political figures, moral philosophers, scientists, churchmen, and the English public at large. What that mature work consisted of she explains in three chapters on Hobbes' contributions to science, politics, and literary criticism. Finally—and for Hobbes

scholarship most importantly—in a chapter analyzing Hobbes' introduction to his Thucydides translation, Reik makes the argument that no unbridgeable distance separates Hobbes the humanist from Hobbes the scientist.

It is somewhat customary among Hobbes scholars to take the publication of Hobbes' Thucydides as closing one stage in the philosopher's life, and his subsequent discovery of Euclid as opening another. Reik contends, to the contrary, that even the admittedly humanistic translation and its introduction reveal a concern for those very problems that are to characterize Hobbes' more scientific researches. The central issue raised by Hobbes in his introduction is the role of history in the conflict between reason and passion, or, pedagogically speaking, in the conflict between dialectics and rhetoric. Thinking that the proper use of language is essential for the apprehension of truth and for the undertaking of deliberate action, Hobbes commends Thucydides for his recognition of the linkage between language and political well-being. Hobbes also expresses approval of Thucydides' rigorous attention to the laws of causality and of his device of fictive orations. Here Reik astutely observes, making her point of continuity, that the historical method of hypothetical constructions corresponds exactly to Hobbes' later method in the physical sciences (p. 49).

It is to this scientific method that Reik next turns and to the consequences of Hobbes' encounter with Euclid. Hobbes endeavored to place politics on as sure a foundation as science, and he learned from Euclidean geometry the methodology of deriving necessary propositions from a few terms correctly defined. Having explained Hobbes' indebtedness to Euclid, Reik proceeds to analyze: his implacable resistance to innate ideas, or separate essences; his epistemological contention that human knowledge is limited to what human beings construct (with the attendant notion that politics can be scientific because the state is artificial); his doctrine of motion and its implications for sense perception and for mechanistic materialism more generally; his theory of the subjectivity of secondary qualities; and finally his running battle with Descartes over the question of dualism.

On the subject of Hobbes' new science of politics, Reik mainly confines herself to a discussion of the state of nature and the origins of civil society, the theory of sovereignty, and the friction between that theory and the legal traditions current at the time. She explains how

for Hobbes natural right (self-preservation) supersedes any duties that can be ascribed to the laws of nature (seek peace) and how justice, obligation, and the concepts of good and evil are wholly conventional, owing their vitality to an independent power responsible for their enforcement. She points out further that because of the close alliance between justice and power, Hobbes cannot countenance the division of sovereign power, as seemingly occurs in mixed regimes. Sovereignty, whether it resides in a multitude or in a single person, must be, and in practice is, absolute. But by the same token sovereign power is instituted to serve a specific purpose, and its failure to do so is grounds for revolt. Thus sovereign power is simultaneously accountable to no one and is subject to review and condemnation by everyone. As if to embody this ambiguity, Hobbes fled London at the time of the Long Parliament, Reik informs us, because he feared the consequences of his absolutist view; and a dozen or so years later he fled Paris where the English court was in exile because he feared being construed an enemy of the monarchy.

The radical nature of Hobbes' political philosophy made him a famous but much-decried man in his later years: jurists attacked his theory of indivisible sovereignty; clergymen took umbrage over his portrait of human beings as incorrigibly egoistic; and members of the Royal Society belittled his pugnacious offerings in mathematics and science. Of this reception by Restoration England in general, not to say of the substance of Hobbes' thought, Miriam Reik provides an able, well-written, and oftentimes insightful account.

PATRICK COBY

*Kenyon College*

**Bentham's Theory of the Modern State.** By Nancy L. Rosenblum. (Cambridge, Mass.: Harvard University Press, 1978. Pp. xv + 169. \$15.00.)

Believing either that confession is good for the soul or that one should make one's biases clear, Nancy Rosenblum begins with a candid admission: she is "by political preference an enthusiast of the modern state." The object of her enthusiasm was "born struggling" and is today "under attack." Its enemies include "parochialism," "Enlightenment universalism and its principal heir, socialism" (pp. 1–2; what, one wonders, of modern state socialism?). She is an "enthusiast" in the older,

religious meaning of the term. Upset by "the loss of faith in the state idea," she turns for solace to Jeremy Bentham, who, as "a theorist of the modern state," has "proselytized more fiercely on its behalf" than anyone else. She notes with approval that "Bentham shared and even encouraged" the idea of "reason of state," or "Machiavellism," which she defines as "an enduring political attitude that takes the state as an ideal and accepts the imperatives this ideal generates" (p. 4). This paean to Machiavellism and the modern state brings to mind Henry Kissinger's remark about power being the best aphrodisiac. Combining Kissinger's dictum with Acton's, we might say that power thrills, and absolute power thrills absolutely. This gives us an approximation of Rosenblum's view. She waxes ecstatic about the "thrill of power," "the appetite for intrigue," "the pleasurable calculations of diplomacy"—in short, the manifold satisfactions experienced by those who bear "the proud title [of] political 'insider'" (p. 5).

If one didn't know that this was Rosenblum on Bentham, one might almost think one was reading Kissinger on Metternich (or Kissinger on Kissinger, for that matter): "The mark of modern political practice, he insisted, is absolutism," and no one has given "more thought to the necessity and justification of absolute power . . . or cared more how to organize and exercise it than he." Absolutism requires a "higher rationality" which "ought to govern the actions of governors even though it is bound to conflict with the traditional demands of morality, with rights at positive law, and with the unrestrained inclinations of private men." And since "there is no good without evil, . . . he directed rulers to prefer the lesser evil. . . ." No wonder, then, that "the extent of his democratic sympathies is still debated" (pp. 6–7).

But is this absolutist, authoritarian, Machiavellian figure the real Bentham? What kind of case does Rosenblum make, and how good is it? Ironically, this enthusiast of the modern state—whose political sympathies are anything but Marxist—has, in effect, reiterated and substantiated Marx's reading of Bentham. But where Marx was savage in his criticism (see *Capital*, Vol. I, pp. 609–10), Rosenblum is uncritical in her admiration: Rosenblum's meat is Marx's poison. Both are agreed that Bentham was a theorist of the modern state; that the modern state is a sovereign, comprehensive political entity whose monopoly on the means of coercion is constitutionally justified and accounted just or unjust on the basis of efficiency; and that Bentham's political vision is

predicated upon a metapolitical model of the human being as rational calculator. Such individuals will be served best, i.e., most efficiently, by a certain kind of political system, namely, one that is suited to their "nature." And since people are hedonists by nature, they are subject to "two sovereign masters, pain and pleasure." The state serves the latter by having recourse to the former; it promotes the general happiness by threatening to inflict pain upon individual deviants. The law, or more precisely its makers and enforcers, are the means to this end. Armed with a "social psychology" adequately reflecting human nature, the legislator devises laws the overriding aim of which is to secure our expectations (Ch. 2). Good laws are those which, when properly enforced by responsible public officials (Ch. 6), permit us to plan and carry out our private projects. "The state [as] a legal entity" with "its ethical basis [in] individualism" is, as Rosenblum rightly insists, "a peculiarly modern conception of political order" (p. 151). To this view Marx assented, and added: it is a conception of political order that serves the ideological function of justifying the actions of an emerging class of rational self-interested calculators; its conception of the individual and of human nature is the conception which that class has of itself. It is for this reason that Marx referred to Bentham as "that insipid, pedantic, leather-tongued oracle of the ordinary bourgeois intelligence." Unkind but not untrue.

Unlike Marx, however, Rosenblum comes not to bury Bentham but to praise him. Her book is a concise, if uncritical, compendium of Bentham's views on a host of topics relating to the modern state. Concerned as she is to describe and explicate, she is not interested in the sources of Bentham's ideas, nor in exposing contradictions within them, nor yet in answering Bentham's critics. She is content to describe Bentham's views and his reasons for holding them, leaving it up to the reader to judge the cogency, coherence, and adequacy of those reasons. Within these limits I should say that Rosenblum has written an interesting book from which all—even those who do not share her enthusiasm for the modern state—may profit.

TERENCE BALL

Nuffield College, Oxford

**Abraham Lincoln and American Political Religion.** By Glen E. Thurow. (Albany: State University of New York Press, 1976. Pp. xiii + 133. \$10.00.)

A simple truth too often ignored in political philosophy is that a good result depends upon a sound beginning. It makes a considerable difference—perhaps all the difference—whether one approaches political behavior from the position that it can be understood in its own terms, or whether one assumes that it can only be understood as the result of determining events and opinions outside itself. Thurow's analysis of Lincoln is based on the assumptions, once widely held but now seldom adhered to, that (1) a statesman's political opinions and convictions may be known to himself, and (2) may be known to others through public speeches and writings. The elucidation of Lincoln's understanding of the founding principles of this nation, and the relation that his own thought and action bear to them is the subject of this book.

Thurow proceeds by means of a close textual analysis of Lincoln's Temperance and Lyceum speeches, followed by the Gettysburg and Second Inaugural Addresses. His interpretations, especially of the former, are heavily indebted to Harry V. Haffa's *Crisis of the House Divided* (Doubleday, 1959).

For some time now it has been a disputed point whether Lincoln's repeated appeal to the Founders is to be understood as indicating his complete acceptance of their principles or whether, in appealing to those principles, he did not transform as well as invigorate them. In precise and forceful prose Thurow argues that Lincoln did indeed find fault with the Founders. The political and moral crisis that eventuated in the Civil War has its roots in the inadequacies of the Declaration of Independence.

The American Revolution required for its success a rejection of traditional allegiances that culminated in a love of experimentation and novelty. Only such an openness to the unknown could have turned rebellion into revolution. During the Founding period these passions were naturally directed toward establishing the new republic. But revolutionary passions are ill suited to the preservation, as distinguished from the establishment, of republicanism.

The Declaration cannot offer a countervailing force to these passions, Thurow argues, because it is based on a self-evident truth, and truth is notoriously weak when confronted with passion and interest. "Passions can only be

controlled by counter passions, not by a mere statement of the truth" (p. 107). Lincoln's moral and geometrical solution was to convert the *truth* of the Declaration into a *proposition* to be demonstrated. Henceforth, "the principle becomes the end or goal towards which the country must work, rather than the beginning from which all other political principles are derived" (p. 76). Thus is created a new passion for human equality or justice, accomplished by "uniting Christianity to the work of the Fathers" (p. 36). The injustice of inequality is now to be understood as a transgression of God's law, to be rectified only by a recommitment to the principle of human equality.

Without denying the force of this argument, one may ask whether Thurow has not gone too far. Is it altogether accurate to say that "although our fathers brought forth a new nation, they turned a child loose whose final form is not determined by them" (p. 80)? Examining the Declaration afresh, would we not find it to be a bit less Lockean than is implied by this statement? Or, to put it another way, could it not be said that the originating principles of this nation contain far more of Lincoln's position than is admitted by Thurow's thesis? And then one must always bear in mind the changing political requirements of statecraft. It may well be that what sound principle required in the period preceding the Civil War differed markedly from what sound principle—the same sound principle—required in 1776.

However this may be, Thurow has written a tightly argued and thought-provoking book. It should be welcomed by anyone interested in Lincoln or the Founding.

JEFFREY D. WALLIN

*Arkansas State University*

**The Tragic Sense of Political Life.** By Michael A. Weinstein. (Columbia: University of South Carolina Press, 1977. Pp. x + 189. \$9.95.)

Michael Weinstein is an avatar, passing through. He is in agony: a renegade, an anarchist who wears his privilege like a hair-shirt, a rationalist who has stretched his *logos* beyond its conventional tether, has relativized himself through multiple psycho-social self-analyses and has embraced what he calls the Hellenism of our times, marked by the diversity of competing belief systems and alternative histories concomitant with a crisis of imperialism. Here he develops a political philosophy based on the

"irreducibility of conflict, the cultivation of ambivalence, and the tolerance, if not the embrace of ambiguity" (p. 16).

Most of his companions are dead: Kierkegaard, Pascal, Nietzsche, Heidegger and Unamuno are in him, grafted onto the American roots of Elijah Jordan and Josiah Royce. In this, his twelfth book, he plays Kierkegaard to Talcott Parsons' Hegel. He views the Parsonian lexicon as a pathetic, banal synthesis of nineteenth-century idealism and positivism, descriptive of life in today's "conglomerates" (superorganizations that unite a number of subunits, performing tasks which may contradict one another, competing to accumulate the means to make history: wealth, power, influence, loyalty). Weinstein argues that the essential tendency of conglomerates is toward totalitarianism, that ours will be remembered (if we survive) as the century of the concentration camp, that Parsons is its most complete theorist, Hobbes institutionalized.

This book is a call to recognize and accept yourself as a crisis and an agony, or in other words, a "person." If you look with pride on your academic "vita," if you confidently proclaim yourself a Marxist or a realist, if you find meaning in concepts like "history, society, nation, state, race, civilization, organization," if you take comfort by defining yourself in terms of such categorical additions, substitutes for a lost absolute, the old *logos* peddler will try his best to bring you to hour knees: up against the blackboard, infra-man, last man, thinking you have invented happiness, free from guilt and *ressentiment* through behavioral conditioning. Ha! You can't be a person and work in a bureaucracy, and how can you *not* work in a bureaucracy?

Weinstein's "agonic anarchism" is the project of voluntarily committing oneself to appreciating and participating in the oppressive context. Child of the secular city, he digs it. As a "renegade" he stands opposed to all forms of social control (coercion, inducement, flattery and mobilization of guilt). Tenured professor, unwilling to be a saint or martyr, he recognizes that he must use the modes of instrumental social coercion in his rebellion, perhaps even strengthening and reinforcing the organizations against which he rebels, from which there is no exit.

The "tragedy" of contemporary political life is the contradiction between the "will to historical meaning" (like most political scientists Weinstein knows the yearning for world-historical identity, the Henry Kissinger wet-dream of our Machiavellian temptations) and

the "relativism which follows from careful examination of one's context through multiple perspectives." In this book, "anarchism" is primarily an epistemological category, the choice of Pascalian lucidity (doubt in order to doubt again) in the face of our dominant Cartesianism (doubt in order to affirm). Look closely, he says, and you will see your will as a lived contradiction. Nobody is immune. We all want some form of "both and," but Weinstein, following Kierkegaard, gives us the existential "either/or: you will regret *both*."

At the University of California, Berkeley, Edwards Field track stadium is ringed by a barbed-wire fence, looped with razor blades in each circle, fifteen feet to the top of the rectangular stockade. It is not clear whether the fence is there to keep people out, or in. Who can say with confidence whether one teaches in a prison or a school or a factory, or what difference it makes, for what purpose one disciplines and punishes? Marx wrote that if we could hear a human language we would experience it as a plea and a begging. Merleau-Ponty once remarked that the problem during the resistance in France was: how could you be a communist and how could you *not* be a communist? Weinstein asks: how can you *not* be a renegade? Sincerity is the criterion: who has the arrogance to call himself a person?

Hard sayings, no hope or faith here, no consolation. Only what Weinstein calls "ruthless compassion" in the struggle for personhood. An invitation to agony, to hear a human, personal language. How can you hear it? How can you *not* hear it?

J. WIKSE

*Shimer College*

**Terrorism and the Liberal State.** By Paul Wilkinson. (New York: John Wiley, 1977. Pp. xiv + 257. \$14.95.)

This book is not an abstract analysis or "value-free" study of the phenomenon of modern terrorism and its relationship to the liberal democratic polity. Paul Wilkinson has consciously pared down scholarly paraphernalia in favor of a pithier and more polemical approach. The scholarship is in the near background, but is instrumental rather than essential to Wilkinson's purposes. The chief of these, I think, is to alert liberals in the broad European sense, which includes American "conservatives," to the clear and present dangers of a wide variety of terroristic activities and movements. Within this broad notion of liberalism, Wilkinson wish-

es to convince tender-minded liberals that the time has come for tough-mindedness about escalating terrorism.

Sympathy with the "wretched of the earth" might dispose tender-minded liberals to excuse, if not to condone, certain terroristic actions. Even if their attitude is wholly condemnatory, the civil libertarianism of tender-minded liberals might cause them to blanch before effective counter-terrorist policies. Wilkinson's tough-minded liberalism hopes to convince the tender-minded that liberalism is a philosophy of state; it is not anarchism. The state accordingly is an organ of social self-defense to be used against those groups who have declared war on civility and the liberal polity. Whatever the ultimate goals of terrorist groups, their operational code that the end justifies the means, any means, involves a war of attrition against the values of liberal civilization. To Wilkinson it is clear that these values are worth fighting for and that liberalism's concern for individual rights is no barrier against serious counter-terrorist measures. There is a long distance between the current (though decreasingly) inadequate policies of liberal regimes and the threat of a police state.

The conception of terrorism that informs Wilkinson's book is this: "Political terrorism may be briefly defined as coercive intimidation. It is the systematic use of murder and destruction, and the threat of murder and destruction in order to terrorize individuals, groups, communities or governments into conceding to the terrorists' political demands. It is one of the oldest techniques of psychological warfare" (p. 49). Wilkinson is at pains to distinguish terrorism, thus construed, from violence, especially legitimate violence, which is inseparable from any state, liberal or otherwise. A liberal polity thus has not only a right but indeed a duty to employ force in the legitimate self-defense of itself and its citizens.

The first half of the book is devoted to establishing the above framework. It involves examining liberalism from a philosophical standpoint and terrorism from a social science perspective. The latter includes a useful summation of the types and roots of terrorism, where the author avoids any simplistic, say, psycho-analytical, account of this phenomenon. The second part of the book reflects a "policy science" commitment whereby Wilkinson goes into considerable detail about a wide array of counter-terrorist strategies, tactics, and devices. Here the concern is combating internal terrorism as with urban guerrillas and "incipient civil war" (e.g., Northern Ireland) and "international

terrorism" as with highjacking and diplomatic kidnapping. In either case the author ranges from brief case studies to evaluation and recommendation of proper police and military behavior and techniques. Thus we find him saying, for example, that the "new American 180 laser sub-machine gun should prove an invaluable anti-terrorist weapon" (p. 144).

In sum, Wilkinson's book is a somewhat unpleasant book about a most unpleasant subject. It has all the strengths and all the weaknesses of the "policy science" approach. In its specific concern the book seems measured and realistic. Its polemical zeal stems from the author's conviction that "terrorism is more than simply a manifestation of psychopathology, and more than a symptom of social discontent, oppression, and injustice—though it may be both of these things as well. It is also a moral crime, a crime against humanity, an attack not only on our security, our rule of law, and the safety of the state, but on civilized society itself" (p. 66).

MARK N. HAGOPIAN

*American International College*

**Adam Smith's Politics: An Essay in Historiographic Revision.** By Donald Winch. (Cambridge: Cambridge University Press, 1978. Pp. xi + 206. \$22.95, cloth; \$6.95, paper.)

Donald Winch maintains that the interpretation of Adam Smith's politics has been confused by treating his account of commercial society as an approximation to later conceptions of "liberal capitalism" and missing his central preoccupation with the political problems arising out of the relationships between commercial society and a regime of liberty and justice. For Smith, Winch shows, these are problems to be generally understood by "natural jurisprudence," to be specified by "experimental" and historical thinking, and to be managed by political wisdom. The nineteenth-century theorists with which he is mistakenly associated define the political concepts in terms of the socioeconomic and merge the accounts of primary processes in the two or three domains which Smith distinguishes. To ascribe such views to Smith deprives us of an understanding of his distinctive position and obscures the need to account for the subsequent shift to the later types of theories. Winch has reviewed the textual evidence in the context of political themes promine among Smith's contemporaries and he has uncovered a complex

political conception and a distinctive style of political analysis.

According to Winch, Smith's actual intellectual agenda is strongly influenced by the debates about the political implications of commerce begun in the preceding century and carried on in his time between thinkers who have been recently studied as continuators of a republican tradition of civic humanism and others characterized by Duncan Forbes as proponents of "sceptical Whiggism." Like Forbes, Winch generally takes Smith as representing the latter school of thought, which teaches that commercial society fosters habits and relationships which contribute to liberty and justice. At the same time, Winch shows, Smith must address issues raised by those who see in the triumph of commerce a dangerous source of corruption in the civic order, and he identifies some threatening effects they had not seen, chiefly concerning the degradation of the poor through the division of labor and the political machinations of the merchant class. Winch demonstrates that for Smith the political disadvantages resulting from commercial society can only be countered and the political advantages enjoyed by means of an adequate political conception embodied in wise leaders ruling within an appropriate political constitution. The theory of political economy does not itself provide that political conception and Smith never imagined that it could.

Winch expounds his interpretation in a sequence of studies taking up in turn Smith's relationship to the natural jurisprudence of his teacher, Francis Hutcheson, his appreciation of commercial society as a precondition for a political system of constitutional liberty, and his encounters with the great questions of the public debt, the militia, and policy toward America. The treatments, especially in the last three of these chapters, are full of rich and surprising insights. What emerges is the portrait of a political thinker with a far more generous vision of the ends of government than is credited by those who think that he intended it merely to perform necessary services for those in the marketplace. Winch stresses Smith's concern for the welfare and independence of the poor, his regard for the intrinsic moral worth of the governing function, and his

detestation for oppressive, arbitrary regimes. Moreover, Smith is shown to have an astute understanding of political structures and processes. His policy studies put the analysis of substantive problems in the context of care for the state of public opinion concerning the authority of government and accommodation to the complex of motives actuating individuals and factions in the governing class. There are no automatic solutions to be expected from the systems comprehended by either of the finished theoretical works. Civilization requires successful interventions by legislators and magistrates exercising the highest form of prudence and benevolence, even while it is constantly jeopardized by the ignorance and ambition of factious politicians.

The book tells all this very well and fulfills the major promise of the title. The uncertain places are those where questions about the philosophical character of Smith's political thought impinge on the political account. One difficulty concerns the vexing questions about the interrelationships between arguments derived from universal theories of human nature or of the natural economic system and arguments derived from the accounts of historical variety and change, and how both sorts of arguments bear on the justification of ethical judgments. Winch disclaims revisionist ambitions in this domain and declares himself satisfied with the view that Smith's historical interpretations guide the "experimental" specification and application of universal principles derived from the theory of human nature. While he is surely right to argue that Smith's major theories do not rest upon a "philosophy of history" in the nineteenth-century continental sense—let alone the economic determinist philosophy of history—Winch brushes past some materials that suggest that the case is more complicated and unresolved than he allows. But Winch seems willing to leave speculative difficulties to others. He provides a subtle and illuminating reading of Smith's political writings and a valuable corrective to anachronistic stereotypes which hamper the study of modern liberal thought.

DAVID KETTLER

*Trènt University*

## American Politics

**Constitutional Language: An Interpretation of Judicial Decision.** By John Brigham. (Westport, Conn.: Greenwood Press, 1978. Pp. viii + 182. \$15.95.)

This is a good book. It provides aid and comfort neither to behavioralists nor traditionalists. This is not the reason why it is a good book. It is good because the author's perceptive and insightful analysis of constitutional language points up a major shortcoming in the work of scholars in the vineyard of behavioralism as well as that of those who labor in traditional workways. The value of the work of both will be substantially enhanced if Brigham's counsel is heeded.

Behavioralists are faulted, not because of any failure to accomplish what they set out to do, but because they set their sights too low: "Research of this nature, as conventionally operationalized, permits no intermediary position between 'laws' on the one hand and 'men' on the other; . . . the dichotomy evident in this position has inhibited explanation of the legal process. It has driven a wedge between law, the nonfactual norms, and reality, the observable behavior of judges. The behavior of judges, although it tells 'what the law is,' is analytically distinguishable from the body of 'law'" (p. 39). A similar criticism is made of the traditionalists: "The impact of grammar on a decision is far more subtle and potentially more revealing than the rules that we traditionally look to for insight into the role of law in judicial interpretation" (p. 96).

Brigham's thesis is that the language of a judicial decision, rather than rules or attitudes, best portrays the work of the Supreme Court. The limitation that operates is "the authority of symbols rather than the symbols of authority" (p. 161). Brigham documents his thesis by semantic analysis of language and by reference to modern theories of language and linguistics. His treatment of the right to privacy in chapters 6 and 8 is especially telling. By treating the right to privacy—along with other provisions of the Constitution—as examples of the grammar of constitutional law Brigham establishes constitutional law not merely as an activity with a technical vocabulary, but as a grammar dependent on the existence of unique practices. As he points out, "If the process were merely one of looking up precedents, then of course, there would be no difference in the decision-making capabilities and propensities of a layman and a

judge trained in the law. The fact that all Justices of the Supreme Court have been trained in the law has meant that the decisions they render are different from those that would be rendered by lay magistrates" (p. 94).

It is unfortunate that the quality of the book's contents has been sullied by sloppy editing and gross overpricing. Not only do typographical errors and misspellings abound—e.g., "philosophers" (p. 146), "porposed" (p. 86), "Herman C. Pritchett" (p. 168), "*Village of Belle Terre v. Boraos*" (p. 158)—but also such nonsense as the following: "In the constitutional tradition, a practice is defined by its grammar rather than by the rules which apply to it. Although practice will be describable entirely in terms of their rules. Procedures sense of what these practices are" (p. 97). Garbled passages are sometimes excusable, but to overlook them in a book that emphasizes grammar as heavily as this one does asks too much.

HAROLD J. SPAETH

*Michigan State University*

**The Constitution Between Friends: Congress, the President, and the Law.** By Louis Fisher. (New York: St. Martin's Press, 1978. Pp. xii + 274. \$12.95, cloth; \$5.95, paper.)

Louis Fisher's new book performs a number of valuable services for students of the Congress and the presidency. Though in 1972 he published a book, *President and Congress: Power and Policy* that dealt with institutional theory and practice, his current book focuses more directly and consistently on such issues and also provides an up-dated examination of the significant events of the mid-1970s. In this book Fisher accordingly organizes his discussion around such topics as the appointment and removal powers of the president, presidential and congressional vetoes, congressional investigations and executive privilege, treaties and executive agreements, the appropriations power, administrative lawmaking, and the war power. In each case he treats the historical development of issues and practice as a prelude to delineating the current state of agreement and dispute. He relies heavily on judicial decisions in establishing points of reference in his analysis; but he is also sensitive to the impact of institutional practice and to the political significance of key historical outcomes and current



issues. Last but not least, he seeks in important areas of current controversy to define and defend policy standards of his own.

As might be expected in such a comprehensive and ambitious undertaking, the quality of the treatment varies from topic to topic. Though Fisher's knowledge is impressive, he nonetheless delineates issues, practice, and political implications far better in some areas than in others. Moreover, the ad hoc manner in which he defines policy standards tends to limit their depth and persuasiveness. Overall, however, Fisher provides an informative, intelligent, and often perceptive survey of the primary features of institutional terrain laid down in the Constitution.

Yet the significance of this book, and of Fisher's work generally, is broader than the substantive content of his analysis. His avowed aim is to reassert the centrality of formal powers and prerogatives as an aspect of congressional and presidential politics and to encourage political scientists to engage in institutional policy debates (p. vii). Fisher thus represents the continuation of a long tradition in American political science—the institutional, public law tradition. This tradition, to be sure, has not been dominant among students of the Congress and presidency since the advent of the behavioral revolution in the 1950s. Indeed, in the early years of the behavioral movement many of its advocates attacked the older tradition with fervor. They argued, not without reason in the context of much of the existing literature, that for political science to advance as a discipline, the focus of research should be on individual behavior and process, that formal powers and prerogatives were of secondary, if not superficial, importance, and that the province of the political scientist could not encompass evaluation. Ironically enough, however, the 1970s have witnessed a new receptivity to traditional institutionalist premises owing both to the character of institutional conflict and change and to increased awareness of the limitations of prevailing modes of behavioral research. Aside from contributing to our knowledge, Fisher's new book thus also has the virtue of incorporating traditional institutionalist assumptions regarding context, structure, and reason, and by so doing reminding us of the continuing need to take them into account. Put in more modern garb, these assumptions may be summarized as follows:

(1) In the case of complex organizations the wholes are always more than the sum of the parts and environmental relationships and interdependencies are of critical significance. Treat-

ment of the Congress and the presidency as wholes in rapidly changing and uncertain environments is thus critical to an understanding of these entities both in terms of internal structures and external relationships.

(2) Internal structures and external relationships involve normatively patterned interaction, some critical portions of which are formalized in law and rules. At any point in time these formalized patterns and the shared conceptions of norms underlying them constrain and affect the distribution of influence, the character of behavior, and the determination of substantive outcomes. Moreover, some significant aspects of such patterns and norms are continually in flux as a consequence of changing environmental pressures. Hence, a critical dimension of the politics of the Congress and the presidency relates to conflicts and outcomes in these regards.

(3) Rational persuasion on the basis of shared values as well as bargaining on the basis of interest plays a role in such conflicts. Ideas are indeed weapons, but they are not equal in weight nor open to unconstrained manipulation in the service of interest. Therefore, not only should reductionism in terms of reason or interest be avoided; in addition, students of politics can and should contribute to institutional policy debates. The arena should not be left to lawyers and historians who are less apt to take basic political factors or implications into account.

It is no criticism of Fisher to note that he neither attempts nor accomplishes any consistent or far-reaching integration of context, structure, and behavior. He well understands the need to treat the Congress and presidency as wholes, the political significance of formalized relationships, and the place of rational persuasion in institutional policy debates. But as in the case of the best of the pre-1950 literature, the theoretical apparatus employed lacks the concepts and measures needed to reduce the elements of vagueness and imprecision that persist in institutionalist analysis. As for more empirically oriented students, they have not as yet provided much assistance in this regard. Despite a growing realization of the need to deal explicitly with context and structure as critical variables, analysts in the behavioral tradition remain reluctant to approach the Congress and presidency as wholes—to focus on their roles or positions, interdependencies, and conflicts. The importance of context is far more widely recognized than formerly, but still largely with reference to individual behavior and either in terms of highly discrete

aspects of internal structure or broad single-factor theories which are then accorded all-encompassing explanatory significance. Here too, then, much remains to be done, but the prospects for synthesizing the best of the institutionalist and behavioral traditions are brighter than at any time in the past.

JOSEPH COOPER

*Rice University*

**Democracy and the Amendments to the Constitution.** By Alan P. Grimes. (Lexington, Mass.: Lexington Books, 1978. Pp. xi + 190. \$17.95.)

The "constitutional road to the decision of the people," admonished Madison in the 49th *Federalist*, should be "kept open for great and extraordinary occasions." Granted that not all constitutional amendments have met the Founders' stringent standards, we might nonetheless be disconcerted by the assertion that amendments to the Constitution of the United States are "public evidences of a transfer of power into the hands of a new majority, . . . settlements that the dominant party or coalition perceives to be advantageous to their continuance in power" (p. 162), and are a means by which "the power of an opposing elite" has been "undermined" (p. 163). Grimes could be right, of course. The motivations for and the processes of constitutional change may indeed be equated with garden-variety politicking, bearing little relationship to the serious concerns underlying Article 5. But we are entitled to some forthright discussion on the point.

This criticism aside, there is no doubt that Grimes has provided a fresh, ingenious, engaging and most useful perspective on the objectives, politics and results of constitutional revision. Carefully documenting his case, he argues that the overall "thrust" of "amendment politics" has been in the direction of "egalitarian democracy," defined as a system of "equal rights and majority rule" (p. 26), and that most amendments reflect sectional and/or demographic interests, ideologies and sentiments.

The first 12, which guarantee individual rights and limit the power of the federal government (1-9), provide for a degree of state autonomy (10 and 11), and (by assuring that the president and vice-president are of the same

political persuasion) bolster the principle of majority rule (12), he classifies as southern; and the abolitionist-inspired 13th, 14th, and 15th as northern amendments. Their Populist/Progressive origins designate 16-19 (income tax, direct election of senators, prohibition, women's suffrage) as western; and the 23rd through the proposed 27th (partial enfranchisement for residents of the District of Columbia, abolition of the poll tax, presidential succession and disability, enfranchisement for 18-year-olds, women's rights) are viewed as urban amendments. Amendments 20, 21, 22 and the unratified Child Labor Amendment ("lame duck," repeal of prohibition, presidential two term limitation) adopted during the period in which the politics of urban America supplanted the politics of regional and rural America, are tagged transitional amendments. Here Grimes is especially resourceful, arguing that the 20th, usually considered along with the 12th to be a technical or corrective amendment, has had, by shortening the period between election and assumption of office, the same effect of operationalizing the principle of majority rule. Some amendments, 11, 18, 21, 22, and 25, do not of course fit neatly into their assigned slots and have not furthered egalitarianism and/or majoritarianism. Grimes deals adequately with what are not awkward exceptions.

In addition to the classification system, the value of the book is enhanced by the way Grimes opens our eyes to the relationship between the adoption of amendments in clusters and the rise of new power blocs and constituencies. Amendments 1-12, ratified between 1791 and 1804, "signaled the triumph of the Jeffersonian[s] . . . over the Federalists" (p. 158). With the next three (1865-1870) the northern Republicans "sought to check the political power of the heirs of Jefferson and Jackson" (p. 158). The Western (1913-1920), Transitional (1933 and 1947, the sole exception to the clustering effect) and the Urban (1961-1970) Amendments in turn "rewrote the constitutional rules" (p. 159) in order to make significant inroads into the power of the Republican party and the southern establishment.

Whatever reservations may persist about the perhaps too facile "amendment politics" approach are compensated for by Grimes' illuminating analysis which forces us, like it or not, to reorient our thinking about constitutional history. And because his book is so succinct and clearly written, so well organized, informative and often lively, it is strongly recommended for undergraduates—not only for their edification,

but for their enjoyment as well.

MARY CORNELIA PORTER

Barat College

**The New Politics of Food.** Edited by Don F. Hadwiger and William P. Browne. (Lexington, Mass.: Lexington Books, 1978. Pp. ix + 267. \$19.95.)

The 1973 Agriculture Act was supposed to mark the end of the era in which the federal government intervened energetically to raise farm prices and incomes. Henceforth, farmers were to make their living from the market, with federal subsidies provided only during rare disasters. The future for farmers looked bright as massive wheat sales to the Soviet Union helped produce a sharp upturn in farm prices. Yet in 1977, Congress passed another Agriculture Act to raise farm prices through government intervention. Prompted partly by a new, militant group, the American Agriculture Movement, Congress was so willing to return to agricultural subsidies that vigorous efforts by the White House were necessary to limit the extent of the federal government's commitment to acceptable levels. Political scientists may be inclined to mutter "*plus ça change*," and pass on.

The contributors to this collection would argue that such a reaction is simplistic. While, as Browne notes in his introduction, "the authors seem to agree that the agricultural subsystem as a whole is alive and well" (p. 3), much too has changed. The grassroots rebellion which produced the American Agriculture Movement (which receives regrettably little attention in this collection) has had its effect not only on politicians, but also on existing farm organizations; apparently even the American Farm Bureau Federation was forced to modify its extreme laissez-faire views. But more significantly, new actors have forced their way onto the stage. Agricultural policy making is no longer just for farmers and their representatives. Environmentalists have raised subversive questions about current farming techniques and the direction in which research is taking the industry. Representatives of the consumer groups have succeeded in winning appointments under the Carter administration even in the Department of Agriculture itself. In a period when inflation and the level of government expendi-

ture have become popular political issues, it is less easy to assume that either the consumer or the government will pick up the tab unquestioningly for farm subsidy programs. The steady decline in the number of full-time farmers in the U.S. scarcely makes easier the task of creating a political coalition to defend farm subsidies.

The contributors to this study had a most valuable opportunity to describe and explain the changes in agricultural politics which these changes have produced. Yet the collection fails to exploit this opportunity and indeed notably lacks coherence. Many of the chapters do contain fresh information or ideas which political scientists will find useful. Thus Browne and Wiggins provide an interesting discussion of the general farm organizations. Berry contributes a useful account of developments in the food stamp program and two contributors make praiseworthy attempts to test aspects of the well-worn clientele theory (which their findings do not substantiate). But few common themes or questions link the chapters together: the attempt to monitor changes in the politics of food is soon forgotten. Major aspects of the current politics of food are neglected; thus no contributor discusses the possibly enhanced role of commodity pressure groups, and the chapter on the executive and agricultural politics is merely four and a half pages long. But above all, the contributors lack any adequate picture of the old agricultural politics with which current practices might be compared systematically. Insofar as the contributors implicitly share a picture of past agricultural politics, it is that of the "iron triangle" and clientelism, usually substantiated by references to general works on American government. Little account is taken of the implications of the analyses of writers such as Mayhew and Clausen on congressional support for farm programs. Indeed, it is regrettable that the chapters on the congressional politics of the 1977 Agriculture Act make so few comparisons with voting patterns on agriculture acts in the 1950s and 1960s. One would like to know whether the findings of older studies by Mayhew, Clausen et al. about the relative importance of constituency, ideology and party still stand. Much the same can be said, *mutatis mutandis*, of chapters on other aspects of the policy process. One wonders whether the explanation for such omissions is that contributors were given too little space to develop their ideas; a text of less than 200 pages is divided into 21 chapters. Perhaps fewer but longer contributions would have produced a collection

which explored more systematically the new politics of food.

GRAHAM K. WILSON

*University of Essex*

**Small and Large Together: Governing the Metropolis.** By Howard W. Hallman. (Beverly Hills, Calif.: Sage Library of Social Research, Vol. 56, 1977. Pp. 288. \$14.00, cloth; \$6.95, paper.)

This book will prove useful to scholars, students, reformers, and politicians. Scholars who have not yet read the 16 reports of the Advisory Commission on Intergovernmental Relations or studied the 276 Standard Metropolitan Statistical Areas (SMSAs) of the U.S. will appreciate the complexity of the metropolitan areas described in *Small and Large Together*. Students will find this a good introduction to the problems and prospects of government in the metropolitan areas where 73.2 percent of all Americans live. Would-be reformers and politicians will be introduced to competing political philosophies underlying many of the alternative proposals for governing these metropolitan areas.

The principal strength of the book is its treatment of the complexity and diversity of the 276 different SMSAs. While advocating local federalism as a general cure for dysfunctional aspects of current local governments in metropolitan areas, Hallman recognizes that this will take different forms depending upon the size of the population, the number of counties, and different existing political and governmental patterns in each area.

Despite his full treatment of complexity and diversity, Hallman advocates a specific point of view with which he concludes the book:

Metropolitan society is pluralistic, so its governmental structure should be diversified. But not so fragmented as to be incoherent and ineffective. As metropolitan citizens we need an areawide general-purpose government responsible to us through direct elections; as neighborhood citizens we need a governmental unit closer to home; and as citizens of intermediate areas—city or county—we need this focus too. All this is possible under local federalism, the system which brings small and large together into a sensible whole (p. 268).

The merit of Hallman's prescription is his attempt to join the movement for neighborhood government which seeks to bring government within the reach of each citizen, with the

movement which seeks to fashion a government for the emerging metropolitan community with sufficient scope and powers to govern these huge areas effectively. Hallman's local federalism allows the flexibility not only to join together both larger and smaller units of government but to do so without radically changing existing governments which have the power to block change. For many readers Hallman's local federalism will prove a touchstone by which to refashion current local government in metropolitan areas.

Hallman's book, with its mix of case studies, general statistics on all SMSAs, and competing political theories, overcomes the limitations of case studies of single metropolitan areas or single political philosophies. It suffers, however, from three limitations of its own. First, it does not "prove" that local federalism is inevitable or even the most likely governmental structure for the metropolitan area—it is a hope for reform rather than a compilation of provable hypotheses. Second, it does not erect a firm philosophical foundation for local federalism; rather, in the American reform tradition, it proposes a pragmatic solution which is grounded neither in assertions about the nature of man nor about the ideal society to be achieved. Finally, it does not provide a strategic analysis of the political basis for the proposed change, although trends and tactics are discussed. Nonetheless, Hallman provides a solidly researched book which captures the complexity of metropolitan areas and their governments, a useful book for scholars, students, and practitioners, and a hopeful book which advocates better government for the three-fourths of all Americans who live in urban areas.

DICK SIMPSON

*University of Illinois, Chicago Circle*

**The Emergence of Professional Social Science: The American Social Science Association and the Nineteenth-Century Crisis of Authority.** By Thomas L. Haskell. (Urbana: University of Illinois Press, 1977. Pp. xii + 276. \$12.00.)

The creation, development and death of the American Social Science Association were integral to the sweeping changes in the educational, scientific and professional life of the latter part of the nineteenth century and the beginning of the twentieth. New university curricula, new graduate programs, new scientific societies, new professional organizations

were the order of the day. At the same time, many of the leaders in these developments were greatly concerned about public policy and actively interested in public affairs. It was such people—both interested in science and in public policy—who created the American Social Science Association.

Concern about support for development of the natural sciences led to the creation of the American Association for the Advancement of Science (1847) and the National Academy of Sciences (1867). Concern about developing the social sciences and applying social scientific knowledge to the public problems of the day led to the creation of the American Social Science Association (1865) and the American Academy of Political and Social Science (1890). Concern about the need for specialization and professionalization of the individual disciplines led to the creation of the American Historical Association (1885), the American Economics Association (1885), the American Political Science Association (1903), and the American Sociological Association (1905). Concern about all these matters led to the new university curricula and the new graduate programs such as those initiated by John W. Burgess at Columbia (the School of Political Science was created in 1880), by President Andrew D. White at Cornell and President Daniel C. Gilman at Johns Hopkins.

Thomas L. Haskell's interesting and significant book brings us to one of the centers of such activity, the American Social Science Association, organized in 1865. The central focus of Haskell's book is the rise and fall of that organization. Most of the leaders in the intellectual and professional life of the times were involved: Daniel Gilman, Andrew White, William Lloyd Garrison, Thomas Hill, Theodore Douglas Woolsey, Frank Sanborn, William Barton Rogers, A. B. Palmer, Samuel Gridley Howe, Simeon Baldwin and Edward Jarvis, to name only a few. Haskell's story is well told and includes in the footnotes and bibliography useful sources for those who wish to explore the intellectual developments of the latter half of the nineteenth century and the beginning of the twentieth.

Haskell notes, and the history of the period confirms, that major changes took place in intellectual and educational life between the founding of the American Social Science Association in 1865 and its death in 1909. For example, when the American Social Science Association was founded in 1865, there were virtually "no specialized professional social scientists in this country or, strictly speaking, in

any other." But when the ASSA died in 1909, each of the specialized sciences were sufficiently developed to have created separate professional organizations, separate journals, and annual meetings—all based on newly developed specialized curricula at the university level. The ASSA played a significant role in these developments. Yet, at the same time, it was centrally concerned with political, social and economic reform, with solving the social problems of the day. There was continual tension between science and reform and this plus the lack of a solid basis in the academic disciplines in the universities accounted, in part, for the ASSA's failure to survive.

The internal conflict between reform and science is one of the most interesting parts of Haskell's story. In an era that was moving rapidly toward specialization and professionalization and that carried with it a strong element of positivism, the leading figure in the founding and development of the ASSA was Frank Sanborn, more reformer than scientist. He was secretary of the ASSA for 33 years and devoted more time and attention to the organization than any other individual. Sanborn believed that the term "social science" was "a convenient rubric for a kind of inquiry and reform activity in which scientists had no edge over novelists"; he characterized the *Journal of Social Science* as "my serial novel." He was, as Haskell reports, "free lance intellectual, teacher, poet, journalist, radical firebrand, polite reformer, government bureaucrat, philosopher, classicist, propagandist and philanthropist." In his support of John Brown, he made no pretense of being neutral. "He withheld facts and bent them when necessary to further a goal he felt higher than objectivity." It is easy to recognize in Sanborn a type as well known in the sixties and seventies of this century as in the same decades of the nineteenth.

Yet the ASSA and Sanborn were concerned with professionalization. Gilman of Hopkins, Eliot of Harvard, White of Cornell, Baldwin, founder of the American Bar Association and in 1911 president of the APSA, Benjamin Pierce, Louis Agassiz, Edmund Jones and the Lazaroni were all active ASSA participants. Side by side, sometimes in the same man, there was strong desire to create the community of the competent and an equally strong desire to contribute to remedying society's ills.

The ASSA was a combination of reformers and scientists: scientists who wished through specialization and professionalization to establish authority based on science, and reformers who wished to use the results of science to

prevent crime, treat the insane more humanely, improve sanitation, develop better education, provide relief and reduce unemployment. It was an uneasy combination—not unknown today—of those who primarily wished to understand society and those who primarily wished to improve it. There often was little distinction between scientific inquiry and reformist activism. Unhappily, there was too often an acceptance of the simplistic assumption, by both sides, that all of society's ills would disappear if only the right information could be gathered and rationally analyzed. Provide the facts, it was thought, and solutions will be obvious.

Unfortunately for the ASSA, it did not adjust to the changes taking place. While the ASSA aided in the organization of the new professional associations—such as AHA, AEA, APSA and ASA—it did not recognize that their development sounded the death knell of the ASSA. Daniel C. Gilman, who was invited to make the ASSA a part of Johns Hopkins, had a clearer vision of what was happening when he turned down the proposed merger.

Political scientists interested in the development of the social sciences will find Haskell's story a valuable companion to Somit and Tanenhaus, *The Development of American Political Science*. Haskell's book not only tells us a great deal about our past but also, like all good history, tells us a great deal about our present.

A few final comments: Haskell is interested not only in the history of the ASSA and the years it spanned, but also in an explanation of what happened. This leads him to devote a large part of the introduction to a discussion of the views of Morton White, Talcott Parsons, and H. Stuart Hughes. The discussion is interesting, often insightful, but fundamentally irrelevant to the story of the ASSA and what it tells us about "the crystallization of the social sciences in their present form as academic disciplines" or "the corresponding rise of the professional social scientist as paramount authority on the nature of man and society."

Further, Haskell's continuing effort to explain the development of separate academic disciplines, growing professionalization, and the concurrent rise of the professional social scientist on the basis of the growing "seamless web of interdependence" in society is interesting but unconvincing. In fact, one may justifiably take the reverse view; certainly one may ask which causes which and not come up with Haskell's answer.

Finally, Haskell does not deal effectively with the nineteenth-century "crisis of authori-

ty" that, given his title and his expressed concern, is intended to be a central issue of the book. Of course, he was not required to introduce or deal with this problem; his book has great merit without it.

EVRON M. KIRKPATRICK

*American Political Science Association*

**Jefferson and the Presidency: Leadership in the Young Republic.** By Robert M. Johnstone, Jr. (Ithaca, N.Y.: Cornell University Press, 1978. Pp. 332. \$15.00.)

Whether as philosopher or president, Thomas Jefferson is a figure of tantalizingly elusive brilliance who remains, despite a lengthy parade of scholarship, a towering presence, whose many nuances of thought and activity cannot be altogether explained. In this book of thorough scholarship and bold, but measured, interpretation, Robert M. Johnstone, Jr. addresses himself to Jefferson's presidency and the illumination it provides of leadership in a new nation. To structure his study, Johnstone utilizes the analytical design of Richard Neustadt's classic *Presidential Power*, with its emphasis on the president as a persuader and bargainer.

Jefferson fits readily into the Neustadt pattern. By nature, Jefferson was not given to issuing commands to perform the presidential function. Bargaining and the many subtleties of persuasion suited far better his preference to remove the "monocratic" features of the executive, and the reality that the principal arena for policy making was Congress where the presidency was weakest. Jefferson's extensive use of the Republican party, its caucus, and congressional and executive lieutenants was essentially an endeavor in bargaining and persuasion.

As Johnstone makes clear, Jefferson's conduct of the presidency was shaped by four basic assumptions about the purpose of leadership. Above all, in a republic, a leader must be attentive and responsive to the popular will. Although nowadays this is a cliché of textbook civics, it was an idea struggling for status in Jefferson's day. As leader, he did not seek to respond automatically to popular pressures, but to address the more important popular desires and aspirations.

According to a related assumption, the leader must be able to anticipate the popular will, to shape rather than be shaped by it, to educate the people to discern their own best interests. Jefferson also assumed that leadership

was instrumental, that its function was to formulate and effectuate programs to make the institutions of government more responsive to popular will. In this sense, leadership was substantive and pragmatic. Finally, Jefferson assumed that the leader must be capable of adapting to circumstance, of growing in office, adjusting or abandoning outmoded views, and of escaping enchainment by outworn dogmas. In essence, Jefferson meant to employ the resources of leadership to bring the nation's political institutions into resonance with its experiment in self-government.

The degree to which Jefferson turned the presidency away from the inherited monarchist habits of the Federalists, and committed it to advancing the popular movement which he headed to return to "first principles" of popular government and political liberty, was astutely recognized by John Quincy Adams: "The power of the [Jefferson] administration rests upon the support of a much stronger majority of the people throughout the Union than the former administrations ever possessed since the first establishment of the Constitution. Whatever the merits or demerits of the former administrations may have been, there never was a system of measures more completely and irrevocably abandoned and rejected by the popular vote" (p. 311).

There were other strings to Jefferson's presidential bow. Though deferential to popular will, he used power boldly in that most autonomous domain of the presidency, foreign affairs. The centerpiece of his success was the Louisiana Purchase. Jefferson's management of it demonstrated that he was not so wedded to constitutional purity that he would let pass an enormous national opportunity. To Madison he wrote, "I infer that the less we say about constitutional difficulties respecting Louisiana the better, and that what is necessary for surmounting them must be done *sub silentio*" (p. 72). The key to success was Jefferson's control over Congress, which he needed to exert only moderately because Congress and public opinion readily perceived the merit of the purchase.

As the Louisiana episode suggests, Jefferson perceived the presidency to be the depository of prerogative, the need of the responsible executive to exceed constitutional limits if the public good demanded it. Admittedly, this is one of the more perilous passages to be traversed by the presidency and the nation, a strain on the leader's integrity and his capacity to distinguish resolutely between acts supportive of and acts detrimental to the public

good, especially when the actor is also the judge of his own cause. Jefferson always seemed confident of his capacity to discern the public good, spurred by his commitment to popular rule and his supposition that his acts must pass scrutiny at the bar of public accountability.

Paradoxically, for all of Jefferson's regard for popular opinion, he had available only the most primitive tools for ascertaining it. He rejected one of the more established methods of the day, the "swing around the circle" as offensive to republican simplicity and tinged with monarchy. Jefferson chiefly relied on the party press and the exercise of extraordinary diligence in gathering political intelligence. He maintained an enormous personal correspondence with friends and political acquaintances throughout the country, most of it written in his own hand, and exhorting the recipients to write regularly about affairs of state.

Johnstone's volume will stand at the front rank of the huge corpus of writings on Jefferson. With admirable scholarship, a lucid style, and convincing judgment, it deftly challenges several prevailing interpretations of Jefferson and his presidency, and carves out new territory of its own.

LOUIS W. KOENIG

*New York University*

**The Making of Political Women: A Study of Socialization and Role Conflict.** By Rita Mae Kelly and Mary Boutilier. (Chicago: Nelson-Hall, 1978. Pp. x + 368. \$16.95.)

Students of women in politics appreciate that scholarly wisdom and popular opinion have often presented them with unrealistic perceptions and that analytic models have been sex-biased. Rita Mae Kelly and Mary Boutilier begin to remedy these difficulties in a simultaneously innovative and conventional study of selected political women, socialization factors and conflicted expectations. They provide the basis for a "great woman" theory of comparative politics, when many scholars in this field are moving to social psychological and sociological constructs.

Kelly and Boutilier rely on an eclectic "theoretical" framework constructed from choices made among competing views in a wide range of substantive areas. Their choices often seem rather arbitrary. For example, selecting a linear model of childhood socialization is not uncontroversial. The authors cite the impact of later situational factors (e.g., at pp. 224, 277,

279, 307), but downplay it for their own theoretical requirements (see p. 312). Yet the impact of situational factors as intervening variables probably is quite potent.

The study is conducted within a complex 18-celled typology; the baselines are: (1) sex role ideology, (2) extent and control of life space (one and two combined yield six sub-dimensions) and (3) nature of political salience (which is trichotomized). The proposed typology is applied to three categories of women: private (i.e., some political wives), public (i.e., issue-oriented political volunteers) and achieving (i.e., well-known political office holders or revolutionaries). The authors find these women types form a continuum for adult political behavior and for the stages of socialization they have traversed. They suggest, however, that "continued social science concentration on the 'typical' and the 'representative' (male or female) and the leaving of the atypical and unrepresentative . . . has meant that the social system always appears to outstrip our ability to understand it" (p. 5). Apparently they mean social science has focused on women as typically apolitical mothers and that they intend to typify more atypical political women, among them terrorists and revolutionaries.

The data, forthcoming from a series of brief biographies, both test and ground the theory. This procedure results in rather disconcerting shifts from deduction to induction. The authors are aware of this. They helpfully base some of their inferences on Guttman scaling, though Guttman scaling is not a powerful analytic technique. The authors must assume, for example, a relationship between independent mothers and achieving political women (see p. 241).

Still, a deductive/inductive approach may be promising in this area, given the premise that past work is suspect. If neither reliable theory nor sound basic research is available for guidance, then both must be generated. Such a dual approach requires interdisciplinary, i.e., historical, skills. Absent these, we may find ourselves saddled with meaningless results. Not that Kelly's and Boutilier's findings are meaningless. Far from it. Their results are, however, subject to rigorous reexamination, given several problems.

First, the biographies, even granting space limitations, are shallow and sometimes stereotypical: for example, the assertion that Elivera Doud, Mamie Eisenhower's mother, "never worked" (p. 215). The authors draw on limited sources: for example, many materials on First Ladies' public interests are ignored. Also, an argument could be made that the proposed

categories are not mutually exclusive (but see p. 313). Eleanor Roosevelt is a prime example. The literature is beginning to realize her extensive political participation and its ramifications for the Roosevelt presidency and afterward. There also is some thoughtful research, i.e., in speech communications, which establishes the depth of her self-doubt and ongoing willingness to serve as Franklin Roosevelt's conduit. Abraham Maslow found her self-actualizing; Truman eulogized her as First Lady of the World. This matter may be debated forever; however, it suggests slippage between the private, public and achieving women types.

Further, measurement problems increase with hindsight in this kind of study. The question remains: how to evaluate observations. The book notes Eleanor Roosevelt's rejection of her mother as a role model. What is not noted is Joseph Lash's assertion that Eleanor Roosevelt later understood and appreciated the pressures on, and responsibilities carried by, her mother. Or that Eleanor Roosevelt's maternal grandmother, four aunts and nurse presented models of strong, often unconventional women, before Mlle. Souvestre entered her life. Or that Eleanor Roosevelt's parents both were fatherless and were reared by mothers who had to cope with life directly.

A study positing the importance of early childhood years necessarily depends upon data from that period. It is difficult to attain, even for famous individuals. Kelly and Boutilier frequently confront this problem, for example, in the cases of Mary Wilson and Charlotte Corday. One also encounters "myths" in the case of famous people. For example, the authors note that Maria Tambussi, Ella Grasso's Italian, Roman Catholic mother, appeared a traditional private woman, but that a questionnaire completed by Grasso indicated something else. The authors did not have access to such questionnaires for all their subjects. The authors were not irresponsible in their coding procedures. This is a limitation intrinsic to their data.

Another problem is that Kelly and Boutilier sometimes discuss mostly mainstream literature with relatively few sentences devoted to their own findings, for example, regarding the impact of standard SES variables on female leadership behavior. The authors postulate that using such variables is not particularly fruitful. More than that, they *seem* to be able to support the claim empirically, but further discussion of their findings would have been necessary to do so. They also have a serious problem in their "focused sample" size ( $n = 36$ ), e.g., in



comparing mean educational attainment (pp. 288–89).

Despite these problems, Kelly and Boutilier usefully propose hypotheses and provide a comparative perspective. They do this with a sense of humor and within a nicely structured study. They state “that even with the limitations in the design our efforts are more likely to reveal the processes actually needed to socialize real political women than studies done on random, representative samples of . . . students or even of women in general which may well meet more adequately the rigorous scientific requirements usually established for such studies” (pp. 92–93). They may well be correct. Their study of socialization and role conflict is a landmark effort.

SARAH SLAVIN SCHRAMM

*Pittsburgh, Pennsylvania*

**Presidential Impeachment.** By John R. Labovitz. (New Haven, Conn.: Yale University Press, 1978. Pp. xiii + 268. \$15.00.)

To the Framers of the Constitution impeachment was an essential safeguard of civil government—a weapon of last resort in defense of liberty, a redress for power abused and trust betrayed, an alternative to insurrection and tyrannicide. What the veto was to the executive—protection from encroachments upon the separation of powers—impeachment was to the legislature. “No point is of more importance,” as George Mason put it, “than . . . the right of impeachment.”

In their view of impeachment, the Framers were inspired by British example, schooled by British mentors. The epic struggles between liberty and prerogative—struggles in which Parliament had used impeachment as a “bridle on the king”—were regarded by Americans then as part of their own constitutional experience. Books and pamphlets from the mother country—histories which dwelt on the “delinquencies” of Richard II and of the Stuarts, discourses on popular rights and liberties, works of constitutional exegesis, documentary collections and the like—almost all described or explained, invariably eulogized, impeachment. In the colonial era, Americans tried on several occasions to remove officials by impeachment—as late as 1774 in Massachusetts where the house of representatives accused the chief justice of “high crimes and misdemeanors”—but encountered opposition from the Crown. The renunciation of George III by the Continental

Congress, in which most of the Framers had served at one time or another, was in form and effect an impeachment. Prior to the Convention in Philadelphia, constitution-makers established the removal process in every new government created for the original states. De Toqueville, commenting on impeachment in both state and federal systems, was much impressed. “There is no question on which the American constitutions agree more fully.”

The Framers wrought the presidency in the image of British kingship but denounced the ancient maxim that “the king can do no wrong.” They provided expressly for the impeachment of their republican surrogate. This king-sized substitute for regal power *could* do wrong. He would not be shielded from personal responsibility by constitutional fictions or ministerial scapegoats. There would be no legalistic quibbling, much less bloodspilling—as for centuries there had been in Britain—over whether or not authority existed in the Constitution to depose the chief executive. The Framers intended impeachment as a check primarily on the one person entrusted by the Constitution with executive power.

Despite its importance to the American founders, impeachment has prompted few substantial works of scholarship, none by political scientists. Among the few, the productions of lawyers and historians, *Presidential Impeachment* by John R. Labovitz ranks with the best. Labovitz, incidentally, is critical of political scientists for slighting the subject. A lawyer, he served in 1974 on the impeachment inquiry staff of the House Judiciary Committee. Drawing upon the investigation of Richard Nixon’s conduct as chief executive, Labovitz presents an informed, cogent analysis of constitutional issues involved in presidential impeachment. His central concern, as unavoidably it must be for any analyst of American impeachment, is the nature of “high crimes and misdemeanors.”

Appropriately, the author begins with, and frequently recurs to, the Framers. His explication of their intent is the most satisfactory one now available. Still, in my judgment—I am currently engaged, with support from the National Endowment for the Humanities, in a study of the Framers’ intent—Labovitz could have documented his analysis more impressively had he investigated more thoroughly the Framers’ views, experiences, sources and authorities. The Framers knew, said and did more about impeachment than is divulged by the few sources Labovitz relies upon—chiefly Farrand, Elliot, *The Federalist*, the *Annals*. These sources do not impart, for example, views expressed

by a number of the Framers as they participated in the formulation of state impeachment provisions or were involved personally in state impeachment cases.

To elucidate "high crimes and misdemeanors," Labovitz analyzes the two major impeachments since 1787, those of Justice Samuel Chase and of President Andrew Johnson, which generated and sustained the protracted dispute—Labovitz calls it "one of the longest-running . . . sideshows in American constitutional law" (p. 126)—over the meaning of those words. Chase's defense brought to the fore the newfangled notion that "high crimes and misdemeanors" are indictable offenses, a notion which bedeviled American impeachment thereafter. Had the House of Representatives not been constrained by this notion, Labovitz suggests, the case against Johnson, whose impeachment the author believes may have been justified, might have succeeded. The case should have been built, he argues, principally on constitutional rather than statutory grounds. This contention reflects the author's dominant and pervasive theme.

"High crimes and misdemeanors," Labovitz maintains, are offenses against the Constitution. They are breaches of trust, derelictions of duty, abuses of power. They may or may not involve indictable crimes. Presidential impeachment is justified by persistent failure to "take Care that the Laws be faithfully executed," by violation of the oath "faithfully [to] execute the Office . . . and . . . [to] preserve, protect and defend the Constitution," by egregious misuse of constitutional authority confided to the president's discretion. Infidelity to the Constitution—not indictability—is the essence of "high crimes and misdemeanors." This construction, the view generally of previous commentators, is set forth more effectively by Labovitz than by others.

Again, however, Labovitz could have reinforced his position with fuller documentation, particularly from British writers and precedents familiar to the Framers. These sources make explicit, for example, that impeachment from the beginning was grounded upon standards and conventions distinct from the common law and statutes. This, in an oft-cited precedent, the House of Lords pronounced emphatically in the fourteenth-century reign of Richard II. During the seventeenth and eighteenth centuries, even in the nineteenth, Coke was quoted incessantly, more than any authority in the annals of impeachment, on the *lex et consuetudo parliamenti*. "As every court of justice hath laws and customs for its directions, some by the com-

mon law, some by the civil and canon law, some by peculiar laws and customs, &c. so the high court of parliament . . . has its own proper laws and customs." In all the voluminous writings and records on British impeachment available to the Framers, the indictable-crime notion is nowhere mentioned, much less discussed.

Had Labovitz drawn more deeply from these sources, his treatment would have been more decisive not only of "high crimes and misdemeanors" but of other issues too—executive privilege and congressional inquiry, presidential responsibility for subordinates, justiciability and other matters. Even so, his analysis throughout is astute and persuasive. What he has written considerably enriches the literature on impeachment.

MAURICE KLAIN

*Case Western Reserve University*

**The Quest for Justice: The Politics of School Finance Reform.** By Richard Lehne. (New York: Longman, 1978. Pp. vi + 246. \$11.95, cloth; \$4.95, paper.)

A *Time Magazine*-like description of events surrounding a series of New Jersey Supreme Court decisions in *Robinson v. Cahill* (62 NJ 473, 1973; 63 NJ 196, 1973; 67 NJ 35, 1975; 67 NJ 333, 1975; 69 NJ 133, 1976; and 70 NJ 155, 1976) almost distracts the reader from noticing the contribution to theory contained in this book. On one level *The Quest for Justice* may be read as an interesting case study of the judicial process, of educational policy, or of public finance. At another level, however, it tests Jonathan Casper's hypothesis, from his *Lawyers Before the Warren Court*, that litigation is a means of access to the policy formation process. Using data from an Eagleton Institute project (92 elite interviews during 1975 and 1976 with legislators, education officials, and interest group representatives) and a 1976 New Jersey poll, Lehne rejects the null hypothesis that the judiciary merely referees legal disputes. Instead, he concludes that an activist court can raise issues and set agenda that must be dealt with by the political branches. It is the treatment of this agenda-setting function of "managed litigation" that offers a modest contribution to a theory of policy formation.

Following the California Supreme Court's decision in *Serrano v. Priest* (5 Cal 3d 584, 1971) until the U.S. Supreme Court's ruling in *San Antonio Independent School District v.*

*Rodriguez* (411 U.S. 1, 1973), a number of state courts overturned their jurisdictions' school finance laws. *Serrano* held that, since there was a significant disparity in district tax bases, the law that relied heavily upon district revenues to fund public education denied children and parents equal protection of the laws. The *Rodriguez* case, however, upheld the Texas district funding law as a rational accommodation of various interests for raising school revenue. This definitive ruling, handed down after arguments had been completed in the *Robinson* case and while the New Jersey Supreme Court was considering its decision, prevented the court from relying upon the *Serrano* rule. Paul Tractenberg, Professor at Rutgers Law School and counsel for the NAACP and the ACLU (amici in the *Robinson* case) reports in *Law and Contemporary Problems* 38:312-32 that the plaintiffs-respondents had based much of their argument upon the *Serrano* principle: there was a large interdistrict disparity of wealth in New Jersey and considerably more than the national average of school funds (67 percent) came from district revenue. Fortunately for the plaintiffs, however, they had also argued enough on other grounds to permit the court to base its decision on a provision of the state constitution guaranteeing a "thorough and efficient" education to all New Jersey residents.

After six earlier decisions failed to encourage adequate funding for the school system, the New Jersey Supreme Court finally adopted the advice of its former chief justice, Joseph Weintraub, who said: "Any interim remedy imposed by the court in a situation where the legislature refuses to act should . . . be roundly disliked by everyone (p. 129)." On May 13, 1976 the court ruled: "On and after July 1, 1976, every public officer, state, county or municipal, is hereby enjoined from expending any funds for the support of any free public school (p. 155)." Closing the schools during the celebration of the nation's Bicentennial was not what most New Jersey officials desired. But it was this extreme measure that finally persuaded the state legislature, especially the lower house, to fund a school finance program, including an income tax, similar to what had been proposed several months earlier.

Lehne's interest in judicial policy formation extends to a concern over the effects of court activism. Does entrance into the political arena taint the judiciary and reduce its immediate or future effectiveness? In the *American Journal of Political Science* 22:896-904, Lehne and John Reynolds pursue this question with survey

data and report that

judicial activism did not directly and substantially affect public attitudes toward the court, but it leaves unexplored the question of whether court activism affects political opinion in more indirect ways (p. 903).

As far as the *Robinson* cases are concerned, Lehne concludes that long-standing favorable public opinion toward the New Jersey Supreme Court and its recognized professionalism, the low public esteem toward the legislature and its internal divisions, and the alliance of the Supreme Court and the executive prevented an assault upon the court by the public or other governmental institutions.

Almost as an aside, Lehne devotes a chapter to assessing the impacts of *Robinson*. Financially, he claims "almost all the money appropriated to support the school finance program would have been appropriated even if the litigation had never taken place" (p. 173). Educationally, the litigation attracted attention for the establishment of a "thorough and efficient" process, minimum standards, and accountability that might not have been "so well defined, carefully considered, or partially enacted" (p. 179) without it. Politically, *Robinson* increased the activity of education groups and accelerated the reform of the legislature. In summary, *Robinson* did set the agenda by making the issue "persistent, ambiguous, immediate, visible, legitimate, and intrusive" (p. 207).

JEDON A. EMENHISER

*Humboldt State University*

**Unsafe at Any Margin: Interpreting Congressional Elections.** By Thomas E. Mann. (Washington, D.C.: American Enterprise Institute, 1978. Pp. 116. \$3.25, paper.)

"All politics is local." So goes the favorite aphorism of House Speaker Thomas ("Tip") O'Neill. "Congressional elections are local, not national events." So goes the corroborating conclusion (p. 1) of this excellent, path-breaking study by Thomas Mann. "Congressmen," writes Mann, "are responsible for their own margins of victory or defeat; and the constraints they face are defined largely in their individual districts" (p. 3). And, he adds, this is increasingly so.

Mann is led to this kind of "candidate and local effects" conclusion by the questions he asks and the data he uses. His data come from

98 pre-election telephone surveys taken for the Democratic Study Group in 42 congressional districts (300–400 people per district) in 1974 and 1976. The surveys involve more people in more districts than we have ever had available for analysis. The question which interested the DSG was, in each case, how is our preferred candidate doing and what can he or she do to win? Or, as Mann puts it (p. 12), “What are the determinants of change in congressional district returns?”

Thus, the perspective from which he writes is *the candidate's perspective*; the focus of the analysis is *the district*; the unit of analysis is *the contest*—incumbent and challenger or, in open seats, two aspirants; the object of analysis is *voter choice*. This combination of ingredients imparts a new, candidate-centered, district-level twist to congressional elections research. And the book provides a persuasive argument for further research in its mode.

The subject of the book is voting behavior in congressional elections. Mann argues that our research to date underestimates the importance of candidate evaluation as a source of voter choice—and at a time when candidate evaluation (e.g., the advantage of incumbency) is becoming more important. Mann finds that large numbers of voters both recognize (rather than recall) and evaluate the candidates, positively and negatively, primarily with regard to personal characteristics and job ratings. He uses case studies to buttress his inference that “the political dialogue” within each local context does much to shape candidate image and candidate reputation. What candidates say matters. Campaigns matter, too. And finally, Mann argues—both by explaining party defection and by modeling candidate preference—that, at least, “public evaluations of the candidates are very important in congressional . . . elections” (p. 70) and that, at most, “votes for Congress are determined primarily by the reputation of the candidate” (p. 71).

Nothing in the book runs contrary to the widely researched view that incumbents have an advantage over challengers. Nearly every chart, together with the author's discussions of incumbency, confirms that view. But, more strikingly, Mann's evidence warns us on almost every page that “incumbency is a resource to be exploited more or less effectively, not an automatic advantage” (p. 4). Most of his results are displayed by providing both a mean and a range, which gives the highest and lowest value for all districts. Although the distributions between these end points are not revealed, the wide ranges point to a vast gap between

effective and ineffective incumbents—a gap which exists among challengers and among candidates for open seats as well.

Again, this emphasis on the variability with which incumbents exploit their incumbency stems from the purpose of the research. From the candidate's view, what matters is what happens in one's own district. In a chapter on election outcomes, Mann notes that candidates worry about the interelection swing in their own districts, more than about that national swing which has been the subject of our aggregate election studies. He demonstrates that interelection swings are anything but uniform across districts; and he argues that the candidates and other local factors must be studied if we wish to explain these variations. He repeats the argument after demonstrating the varying degrees of success candidates have in exploiting the basic distribution of partisans within their districts.

This is not a definitive book. Data limitations keep it from becoming such. For the methodologically critical, the concentration on marginal districts, the exclusive use of pre-election surveys, the small number of usable districts in some cases, the small number of cases in some cells will be cause for withholding final judgment. And rightfully so. But even with its imperfections, this unique data set has important virtues. It reminds us that useful electoral data can be unearthed in places other than in our great national collections, and that such data can be mined for the purposes of discovery, if not of confirmation. It reminds us, too, that such data, analyzed with care, can help chart future research directions for our great national enterprises—in this case toward comparative, district-level analyses of voter choice. People at the University of Michigan should read this stimulating book. So should everyone else interested in the subject of congressional elections.

RICHARD F. FENNO, JR.

*University of Rochester*

**Marcus Foster and the Oakland Public Schools: Leadership in an Urban Bureaucracy.** By Jesse J. McCorry. (Berkeley: University of California Press, 1978. Pp. xv + 163. \$10.95.)

Marcus Foster, the first black superintendent of a major urban school system, was assassinated in November 1973, just three years after arriving in Oakland. Because Jesse McCor-

ry, a personal acquaintance of Foster, was given close access to Foster's administrative staff, and because he treated that confidence with the seriousness and integrity it warranted, the superintendent's record has been given a permanence that his zeal and sense of purpose deserve. At the same time McCorry provides us with an unusual study which is once intimate in its appreciation of the "internal politics" of a big-city school administration and detached in its scholarly analysis.

The politics of the Foster recruitment demonstrates the bizarre set of circumstances that seemed necessary before Oakland (or any other big city?) could become the first to have a black school superintendent. Before awarding the position to Foster, the school board, responsible for schools with a 75 percent nonwhite enrollment, had offered it to three different white prospects. In the process it had hopelessly bungled the recruitment effort, exposing itself to widespread public ridicule. Only then did it search assiduously for a black person.

Once in office, Foster suffered the dual disadvantages of being both a black and an "outsider," the latter handicap perhaps being the more severe. He had three major objectives: increased community involvement, especially in the processes of principal selection; administrative decentralization; and the implementation of the then-fashionable planning, programming and budgeting system (PPBS).

Although McCorry gives the policy only passing attention, Foster had the greatest success in increasing ostensible community involvement. But this success was possible, curiously enough, only because community participation "did not affect the normal routines of the system's staff" (p. 145) and because funds to support the citizens' committee were provided by outside foundations. Decentralization of administrative practices to regional offices encountered more severe obstacles. Old-line staff members, with connections to board members, objected to the loss of power and status entailed in decentralization. Not only was Foster blocked from choosing one member of his team of regional administrators, but also regional offices were never given much staff support or assigned their anticipated set of responsibilities. PPBS, if anything, was more of a disaster. Apart from Foster's own limited understanding of budgets and PPBS' inherent limitations, there was, once again, the opposition of a budget office deeply convinced that standard routines were preferable.

After three years in office Foster could

hardly conclude that he had made the same impact in Oakland as superintendent that he had once made in Philadelphia as principal. Only six months before the end Foster himself publicly complained:

Blacks become superintendents only in cities like mine where the percentage of non-whites has reached 78 percent. Black educators don't get called into "cushy" jobs. . . . Then when you can't make it, they say, "I told you those niggers can't do it!" (p. 149)

McCorry's work is sober reading for any reform-minded school administrator who believes that urban bureaucracies simply require new leadership. Yet McCorry's analysis has its own limitations. It is not simply that quotations from well-known students of organizational theory and public administration too often substitute for the author's own analysis and interpretation. Nor is it primarily McCorry's inability to choose between characterizing Foster as a dynamic leader or as a frustrated superintendent. Biographers of John Kennedy are often left in similar confusion. What is most troublesome is the way in which the book's analytical standpoint wavers. At the close of the preface, McCorry makes the provocative, if improbable, claim that "bureaucratic resistance may not be the villain which so many advocates of change claim it to be" (p. xiii). But instead of developing this line of argument he provides a fine, first-hand account of administrative infighting. Certainly, "the problem" is always "a lack of knowledge" (p. xiii), but that is too general a formulation to help account for Foster's particular problems in Oakland. Uncertainty is the necessary but ubiquitous condition under which bureaucratic politics thrives.

PAUL E. PETERSON

*University of Chicago*

**The Rise of the Sunbelt Cities.** Edited by David C. Perry and Alfred J. Watkins. (Beverly Hills, Calif.: Sage, 1977. Pp. 309. \$18.50, cloth; \$7.95, paper.)

This is a provocative book. Its essays go substantially beyond popular treatments of southward shifts in population and economics. They put the rise of sunbelt cities into the context of American urban history, and clarify the events taking place in various urban strata. The book says as much about the old northeast as about the new south. It provides useful data analysis, fine case material, interesting predic-

tions, and challenging dispute between opposing prescriptions for urban policy.

Two themes crop up throughout the book: (1) Like American cities generally, those of the sunbelt thrive according to their success in material production and distribution. To do well, American cities must be good for business. There is little room in the United States for cities whose principal functions are as capitals of administration, religion, or culture. Sunbelt cities have been better for business than northeastern cities in recent decades; they have provided more freedom to entrepreneurs, and they have burdened their entrepreneurs with fewer demands on behalf of the unproductive poor.

(2) The economic progress of sunbelt cities has not been felt equally throughout their social strata. There remains substantial poverty from the past. Along with aggregate economic growth, there has been some widening of the spread between upper- and lower-income groups.

The final three chapters explicate some prescriptive implications of these themes. Gurney Brechenfeld shakes up a reader who might have been lulled by the left-of-center sameness in most earlier pieces. He warns the cities of the sunbelt to avoid the pitfalls of the old North. The resourceful leader should "cut expenses, curb crime, perk the place up with new action, and win battles with militant municipal unions. . . . The more aid cities provide for the poor, the more of them they invite to congregate within their borders. . . . One result is the triumph of misguided altruism over economic common sense." Murray Bookchin and then the editors urge cities to recognize other values than those of the marketplace. They evade the sharp thrust of Breckenfeld's beggar-thy-neighbor prescription, however, and thereby help to legitimize it. When Perry and Watkins write that the social unrest of a declining northeast cannot be hidden by the rise of the sunbelt, they are only partly correct. The pro-business, anti-welfare policies of the sunbelt do seem to work at the local level, supported by federal structures of government and wide respect for local autonomy that allow the cities of one region to gain at the expense of others.

One of the bright spots in the collection is a case study of Houston entrepreneurialism by William D. Angell, Jr. It is an exceptional piece that fits implicitly into the themes developed by other authors. Another gem is Robert Cohen's analysis of sunbelt dependence on national and multinational corporation giants financed and directed from outside the South.

He adds substance to the impression that sunbelt growth has been less an indigenous movement arranged by resourceful managers, and more a case of outsiders taking advantage of a ripe opportunity.

While there is much in this anthology to provoke discussion, it lacks systematic treatment of its own key themes. It suffers from the generic problems of uneven quality and anemic editing. The editors do supply cogent introductions to each section of the book, but they should also have pruned the contributions of several authors. At times the editors' comments are more satisfying than the articles they mean to introduce. There is considerable repetition of general themes from one chapter to the next, and too little progression in the analysis. Several chapters have cogent data analysis or interesting case materials, then lose their sharpness with a flaccid rehashing of themes that are treated elsewhere. The editors deserve thanks for departing from the northeastern bias that they perceive in the urban literature, but they might have done better with a wider base for their own efforts. Six out of 13 contributors come from the University of Texas. A more aggressive search for talent might have exposed the anthology to interesting variants like Miami, Jacksonville, Phoenix, and Los Angeles. All told, the book will carry its weight as a supplement to urban politics courses at the undergraduate level and several of its pieces may find themselves widely cited by specialists in the field.

IRA SHARKANSKY

*University of Wisconsin, Madison and  
Hebrew University of Jerusalem*

**What is Governing? Purpose and Policy in Washington.** By Richard Rose. (Englewood Cliffs, N.J.: Prentice-Hall, 1978. Pp. x + 173. \$5.95, paper.)

This modest-sized paperback is more effective in its parts than as a whole. Richard Rose's attempt to organize the study around the concept of governmental purposes and the elaboration of 18 "models of governing" in terms of purposes does not come off too successfully. For one thing, the notion of "purposes" is too thinly and imprecisely defined and, in application, it apparently embraces means as well as ends. For another, the study's use of the terminology of "models" has no persuasive rationale and no clear utility; students are more likely to be confused than

helped by it. Fortunately, the book's strengths—and they are considerable—emerge intact in spite of these limitations.

Considered apart from the problems related to its central organizing theme, Rose's study offers a brief but cogent review of a large variety of approaches and emphases in the study of American politics and policy. The 18 "models of governing" (some of which are sketched below) provide, in the aggregate, extensive coverage and touch on many subjects emphasized within introductory courses on American government. The volume should prove useful for providing context and perspective to beginning students of American politics.

The study's coverage includes examination of opposing views on whether government should seek to conserve or change society; whether government should aim to produce more wealth or more welfare for its citizens or more legitimacy or power for its public officials; whether policies should reflect individual preferences, group demands, an abstract public interest, or the interest of public officials or agencies. In addition, Rose treats varieties of decision making (satisficing, incrementalism, etc.) in each of the national political institutions as well as administrative implementation, planning, and policy evaluation.

On each of these subjects, Rose connects the "model" to the stage of the governing process, to the political institution, and to the category of public policies to which it is most relevant, and notes whether the "model" is or is not distinctive to American politics and what interests benefit the most from its application. Rose's necessarily abbreviated discussion of these matters should stimulate abler students to seek fuller and more systematic comparison.

Readers of this work should come away with a solid awareness of the complexity of American politics and policy and, consequently, of the complexity of studying those phenomena. At the same time, the study strengthens the students' ability to cope with that complexity by conveying, with brevity and good sense, diverse ways of undertaking such a study.

ALLAN P. SINDLER

*University of California, Berkeley*

**Goodbye to Good-Time Charlie: The American Governor Transformed, 1950–1975.** By Larry Sabato. (Lexington, Mass.: D.C. Heath, 1978. Pp. xvi + 283. \$20.00.)

Sabato has undertaken the task of testing

the "Goodbye to Good-Time Charlie" thesis: that governors of American states have increased their powers to govern; have exerted more influence both within their states and in national affairs; have moved from being a roadblock within the federal system to being key negotiators and coordinators for all levels; and, most significantly, have substantially higher qualifications in terms of vigor, training and performance than their predecessors. For active governor-watchers this is not news, but Sabato's examination of the thesis and its supporting data provides a solid basis for upholding the argument.

Sabato approaches the question by gleaning information obtained from other researchers and extensive archival probing, by conducting semi-structured interviews with governors, former governors and many who have worked with or watched governors over the past decade or so, and by a careful reading of the popular press. The result of these approaches provides a rather realistic analysis of just what has been happening in the states and the governors' chairs over the 25-year period.

Sabato has developed several useful lines of analysis in pursuing the thesis, some borrowed and some new. Taking a leaf from Joseph Schlesinger's *Ambition and Politics* (Chicago: Rand-McNally, 1966), he gives a very persuasive portrait of the change thesis by examining the governors' backgrounds and career patterns into and out of the governor's office. The closing chapter, "The Governorship as Pathway to the Presidency," caps this by comparing the governorship to the vice-presidency and the U.S. Senate as stepping stones to the White House.

Sabato's rating of the top governors during the period under study (37.5 percent or 117 of the 312 who served), while bringing forth some surprises, does fit with some of the "gut feelings" that observers have had about performances in the governorship. Important here are the characteristics associated with these "top" governors—they were younger, better educated, and had legislative experience to a greater degree than their lesser-ranked compatriots. Further, by definition they were not stand-patters, but governors "of conspicuous ability and competence whose term was characterized by personal hard work and firm dedication and who diligently attempted (even if unsuccessful in part) to meet the needs of the people of his or her state" (p. 51).

Comparisons of this top governors' ranking and Sabato's extensive analysis of the key issues leading to the defeat of governors at the hands of the electorate during the period are most

significant. They clearly show that being an outstanding governor by attempting to meet the needs of the people can lead to becoming a former governor—as meeting these needs too often translates into new taxes or tax increases and voter rejection. While the concept of “tax-loss governors” has been asserted, analyzed and questioned, Sabato’s state-by-state review seems to support its validity—and close observers know governors believe it to be true and make decisions with that as a premise.

Sabato makes careful and systematic analyses of the impact of some of the goals of reformers on the governorship: off-year elections and increasing gubernatorial tenure. He also explores the effect of ticket splitting, increasing campaign costs and party competition on the changes in governors and their roles. An excellent bibliographic essay and bibliography are contained in the appendix.

This study pertaining to a major office in our federal system is very well done and uses the most relevant data available to support its thesis. While the amount of factual data presented is sometimes overwhelming, I suspect the presentation has considerable utility to a wide audience including both practitioners and academics.

One fault of this presentation may be the basic assumption underlying Sabato’s argument. There is no question that the states have come a long way over the past decade to get their own houses in order, to move away from former Governor Terry Sanford’s overdrawn characterization of them as indecisive, antiquated, timid and ineffective, not willing to face problems, and not interested in their cities (*Storm Over the States*, New York: McGraw-Hill, 1967, p. 1). Constitutions have been revised, structures reorganized, legislatures strengthened, and gubernatorial power increased. State governments have also dropped much of their reticence toward becoming actively involved in policies and programs at the local and national level, and have increased their taxing powers significantly to meet the cost of a more active stance. Sabato carefully documents all these trends.

However, these are but trends. Looking across the states and their governors, one can see that considerable variations in their status still exist; despite the new breed of governors and better weapons at their disposal, much remains to be done. Actors and agencies at other levels must become aware of the governors’ capabilities and remove age-old blinders which restrict views of governors’ roles. Concomitantly, increasing evidence indicates that governors may now be even further restricted in

what they can do by various federal mandates, guidelines, standards and directives.

Sabato has incisively argued that states are growing stronger and their governors are of increasingly higher caliber and ability. It may be a case of having to run faster just to keep up with the neighbors, but this may be good news also.

THAD L. BEYLE

*University of North Carolina*

**The Giant Jigsaw Puzzle: Democratic Committee Assignments in the Modern House.** By Kenneth A. Shepsle. (Chicago: University of Chicago Press, 1978. Pp. viii + 333. \$30.00.)

The subject of committee assignments has attracted at least its fair share of attention. Interviews have been conducted with Committee on Committees members and with freshman members of Congress after their first assignment. Turnover has been analyzed along with patterns of transfer to and from committees. The outcomes of the process have been studied at length—for freshmen and nonfreshmen, marginal and safe members, southerners and non-southerners, liberals and conservatives, party supporters and mavericks. New data were added with the confidential “request lists” of members seeking assignments: the requests could then be compared against the assignments received. A 1973 *Review* article by Kenneth Shepsle and a co-author analyzed these requests for House Democrats in four congresses. *The Giant Jigsaw Puzzle* draws from and is an expansion of that earlier work.

The book pursues three separate endeavors. It offers, first, a wide-ranging review of the assignment process and selected congressional practices, including a chapter on congressional history, a summary of recent reforms, and considerable descriptive background material throughout. It offers, second, an empirical analysis using the request lists (expanded for House Democrats from 1958 through 1974) and supplementary interviews by the author and others. It offers, third, a mathematical specification and testing of some arguments about the assignment process. These arguments are called theories. Thus chapter 4 in a “theory of freshman requests” argues that freshmen will request committees which reflect their constituency interests or their personal policy interests. Chapter 5 discusses opportunity costs, “wealth effects” (the present committee assignment) and “the status of the queue” (the assignment sought) and finds that nonfreshmen already



holding good assignments will transfer only to the most prestigious committees whereas non-freshmen not holding good assignments are more likely to seek transfers to other committees. The "theory of requests," "theory of the negotiated committee structure," and "theory of assignments" (see p. 7) combine to tell us that members will seek assignments reflecting their own interests and that party leaders and the Committee on Committees will try to accommodate these requests wherever possible.

The descriptive chapters (essentially 2, 6, 7, 8, and epilogue) supply some highly interesting detail on the workings of the process, though much of this will be familiar to serious students of Congress. The mathematical sections (chapter 4, portions of 6 and 9 and the appendix to 7) are meticulously constructed, useful illustrations of how complex political questions can be translated into testable form. The empirical work (chapters 3, 5, and 9), partially previewed in the 1973 article, has been deepened and extended. Of particular interest is the finding that impersonal factors relating to the assignment (such as the amount of competition for the post and for the other posts requested, whether the regional zone of the requester already has "sufficient" representation on the committee, and whether the requester's predecessor in the district held the same assignment) are important to the success of the request, whereas factors relating to the requester (party support, region, electoral safety) are not found to be important. (See chapter 9.) This finding supports the thesis being developed throughout the book that the jigsaw is too large and formidable a problem to permit deviations into the personal or ideological from impersonal and institutional-level criteria. It also supports the established wisdom about Congress as an institution placing conflict management and accommodation above any particular policy concerns or political disputes.

The jigsaw thesis should be regarded as tentative. The measures selected—especially for the requester's characteristics—may not constitute an adequate testing of the propositions. For example, a general measure of party support for nonfreshman requesters is the only measure of ideological or policy influence tested and is found unimportant. Shepsle argues that "Committee on Committee members are, in the main, partisan advocates, so it was anticipated that, all other things being equal, they would be most disposed to those requesters who were strong party supporters" (p. 217).

The Committee on Committees, however, is

itself selected to represent the very diverse elements of the congressional party and so might as easily be expected to converge around the average as around the strongest party supporters. Strong party supporters in the Democratic Party are the liberals; but the Ways and Means Democrats, who comprised the Committee on Committees for the time span of the study, were rarely accused of a particularly noticeable liberal bias. More seriously, however, committee-specific ideological or policy positions were not investigated and these might be expected to have more influence than general party support scores on who gets what assignment.

The variety among committees also requires attention. Committees vary in subject matter, member goals, and political environment. Even the large number of middle-prestige committees ("semi-exclusive" committees) vary from one another and might well attract different requesters and different treatment in assignments. So while the semi-exclusive committees are broadly distinguished at points in the study from the non-exclusive committees (concerning which few requests are made) and from the exclusive committees (concerning which few requests are granted), the potentially more interesting variation within the middle-range category is not examined. If, for example, as past writers suggest, there are "pork-re-election" and "policy" committees in the large middle category, then many of the empirical findings and conclusions of the study may need to be qualified by this distinction.

We can hope that ways will be found to preserve anonymity and make the request data available to a wider research community. Necessary follow-up studies, with attention to measurement problems and committee variation, could then be conducted.

The book is not a major theoretical advance nor a major empirical one, though it offers an important hypothesis on the assignment process along with valuable data and rich detail. It is, however, a work of considerable effort and talent by an author skilled in mathematical and statistical design and at ease with diverse forms of congressional research. It is a very thorough, at times innovative, and always interesting account of the committee assignment process. While the price of the book will limit its use, it can be read with profit by students of Congress and of mathematical modeling applications.

BARBARA HINCKLEY

*University of Wisconsin, Madison*

## Comparative Politics

**In a Moment of Enthusiasm: Political Power and the Second Stratum in Egypt.** By Leonard Binder. (Chicago: University of Chicago Press, 1978. Pp. xxii + 437. \$22.50.)

This painstaking study of Egypt's "second stratum," the rural middle class, is a remarkable contribution to the literature on political development. While not breaking new theoretical ground, it transcends existing liberal and Marxist paradigms to offer an empirical explanation for the apparent stability of an authoritarian regime that neither school could dimly suggest. In the author's words,

If . . . we find particular phenomena, particular historical events, and particular statistics commonly referred to by the analysts of both persuasions, it may just be possible that some piece of social knowledge may be partly freed of its relativistic and polemical character by the deliberate method of collating middle range theory which deals with the phenomena in question regardless of the major paradigm from which it may be taken (pp. 10–11).

Binder is no blind empiricist—indeed, his work will be a classic in the development field precisely because the meticulous data manipulation usually has a theoretical rationale behind it—but the thrust of his argument is atheoretical. To put it crudely, the rural middle class stabilized Nasser's regime and Sadat's (at least up until time of publication, fortunately for the author) and enabled them to mitigate urban class conflict without granting significant participation to either the urban middle classes or the proletariat. To understand and interpret Egypt's model of political development, we must therefore analyze the social, occupational, historical, geographical, and political characteristics of this "second stratum" which Binder, following Gaetano Mosca via James Meisel, defines as "the instrument without which the rulers cannot rule" (pp. 12, 16, 406).

The analysis entailed fascinating detective work. From the Golden Register of the National Union, Binder studied the names of the 27,936 members of the 4,191 village and town committees elected in 1959 in Egypt's 16 agricultural provinces. He hypothesized that his second stratum would include most families two or more of whose members appeared on a given committee. His three coders could agree upon 5,498 individuals constituting 2,223 family sets, almost one-fifth of the total sample.

Unfortunately committee size varied—from 3 to as many as 35—obviously skewing the distribution of the putative second stratum (cf. Table 16, p. 95) in favor of the larger committees, where family sets may have been clans rather than families of notables. Binder was aware of the difficulty (p. 75) and tried to validate the family sets as a rough and ready indicator of second-stratum status. The family set member was more than twice as likely as the nonmember to have been elected to district office in 1959 and to have served as a member of the parliament elected in 1957 (p. 77). Moreover, these relationships held up independently of the size of the village committees. In general, too, family set members displayed higher occupational status than singles.

Binder then searched outside his sample of names, offices, and occupations for more concrete evidence of his second stratum. The obituary notices of *Al-Ahram* over a six-month period (1960–61) were mined for family networks of ancestors and survivors of the deceased. In 99 of 157 cases at least one person named in the obituary was found in the Golden Register, and as many as 60 were members of family sets. In 42 of these cases, at least two family members could be located in the register. "Hence, in 70% of the relevant cases it was possible to confirm that the family set is indeed a family set" (p. 154). But who were they? The historical dimension was the most exciting aspect of the quest.

The second stratum was supposed to be those 700,000–1,000,000 Egyptians in nuclear families owning 20 to 50 feddans—some 3 percent of the total population (pp. 156–57). Binder got one of his students, Sidney Chesnin, to analyze the names of izbah owners—proprietors of certain sorts of rural estates of at least 50 feddans—mapped in 1930. Chesnin found that 22.9 percent of the members of family sets had been izbah owners in 1930, compared to only 3 percent of the singles (pp. 106–08). Binder himself probed further back in time, locating and analyzing the names of rural notables appearing in Ali Mubarak's *Khitat*, a survey compiled in the mid-1870s of landed grandees and rural notables, among other things, by one of Egypt's most prominent civil engineers. Of the quarter of the family names definitely appearing in the Golden Register, some 32 to 34 could be traced to 40 family sets, while 25 others could be traced to 26

individuals (inferred from somewhat confusing data, pp. 116–18). Thus members of family sets were more likely than the singles to display a century of historical continuity.

Even more fascinating was the historical search for members of past parliaments whose descendants appeared in the Golden Register. With the help of another graduate student, John Anderson, Binder tracked down the names and occupations of the MPs serving from 1866 to 1882 and 1924 to 1950. Of the 2400 seats in the agricultural provinces, 37.5 percent were held by ancestors of members of the National Union committees, and 75 percent of the descendants were among the fifth of these members who belonged to family sets (pp. 130, 140). Some of these rural families, like those observed by Ali Mubarek, clearly retained their elite status over the generations, for the families of up to 60 percent of the nineteenth-century MPs persisted in the 1959 sample. Yet the historical search did more than merely confirm the importance of family set members in 1959; it also may have unmasked their principal political function. The percentage of “ancestor” MPs jumped from 17.4 to 58.7 in 1931, the year Ismail Sidqi elected a docile parliament to buttress his authoritarian regime (pp. 131, 136). Marshaling a variety of data concerning the landholdings and political affiliations of the MPs, Binder concludes quite plausibly that his second stratum “developed from the seed planted in 1931, that it was formed in the image of the electoral struggle of the 1930’s and 1940’s, and that it was decisively defined by the policy of excluding important former Wafdist politicians in the aftermath of the Naguib-Nasser dispute of 1954” (p. 142). Indeed the 1959 data throw retrospective light on the pre-Nasser period, especially upon the competition after 1931 between the Wafd and the palace to recruit the rural notables.

Ultimately, however, it is less clear what this study of the rural middle class tells us about contemporary Egypt. Part 2, “The Geographical Distribution of the Second Stratum,” gives us much information in the form of bivariate distributions, correlations, and maps about the National Union’s provincial elite—probably more information than the average reader would care to digest despite the suggestion on p. 281 that these may be only the beginnings of “an important subject for future research.” The relationships between second stratum strength and various indices of modernization could have been presented more parsimoniously using multivariate regression analysis (with provinces or provincial sets as dummy

variables), and factor analysis might have simplified structure of independent variables while minimizing collinearity. But the major difficulty I have with this book is not so much the methodology—though note, for instance, the strange inferences about occupational mobility derived from the inherently skewed data of obituary notices (pp. 160–67)—as the substantive interpretation of the second stratum’s political role.

The second stratum is variously defined as (1) the instrument without which rulers cannot rule; (2) the class that “can arouse, in itself and in the masses, a moment of enthusiasm in which it associates and mingles with society at large, identifies itself with it and is felt and recognized as the *general representative* of this society” (p. 19, citing Marx’s “Critique of Hegel’s Philosophy of Law”); (3) “the pool of qualified persons from among whom the important and not so important officials are chosen” (p. 28); and (4) “a process [of social change] rather than a structure” (p. 279). I suppose a rural middle class, as defined by landholdings, could turn out to be all these things, but the data, which represent village, district, and provincial elite families, can only tell us about its roles in the countryside and its adaptation to urban influences in the northern provinces, which tend to be more exposed than those of Upper Egypt. No evidence is presented in support of the hypothetical roles of the second stratum suggested by the first, second, and third definitions. The third definition is manifestly misleading: not all Egyptian important and not-so-important officials come from rural middle class families, as defined by 20- to 50-feddan landholdings. Perhaps this social category is over-represented in the national political elite (however it may be defined), but so what? So also is the urban “bourgeoisie and aristocracy” defined by Samir Amin. Perhaps at the very top, say among Nasser’s 50 most influential political hacks, most of whom would have been army officers, the rural middle class would be preponderant, but we are not given these data.

The Marxist “moment of enthusiasm” is far more problematical. Perhaps the Nasserist regime did attempt “to transfer the spirit of village Egypt to the seat of power in Cairo”—though Sadat appears to stage-manage the effort more self-consciously than Nasser ever did—but Binder also agrees that neither president could really mobilize the masses, much less emancipate society in the spirit of the early Marx. Indeed, neither the National Union nor its successor, the Arab Socialist, spearheaded

social change so much as prevented it by occupying any available political space. The second stratum may indeed be conceived as a mediating instrument between state and civil society, but then Binder should have turned for his theoretical inspiration not to Marx but to Hegel, who posits a leisured and public-spirited landowning stratum (not necessarily the Junkers!) as the spiritual glue binding a differentiated civil society with the state apparatus outlined in his *Philosophy of Right*. Binder's references to Marx seem forced, as if it is necessary to pamper other interpreters of Egyptian society who are Marxists. The comparisons between Nasser and the Louis Napoleon of Marx's Eighteenth Brumaire are particularly ambivalent. On pp. 22, 399, and 401 Nasserism gets compared to Bonapartism, though the parallel suggested on p. 22 would seem to be with the first Napoleon, not his imitator; on pp. 304 and 398, however, Binder corrects himself, for the regime "turned against the bourgeoisie." He quite rightly argues that the peasants were not Marxian sacks of potatoes, but the mystery remains: in what sense might Nasser have reflected (or conjured up) a rural middle class "moment of enthusiasm?" Perhaps what Binder really meant was that "Nasser's rural support was . . . more enthusiastic than materially interested"—a useful source of support because it was relatively undemanding (p. 304). Despite appearances, the rural middle class ("lucky Pierre" on p. 7) is not the principal beneficiary of the regime, but some of its urban kinsmen are supposedly favored over other, older professional, technical, and functionary groups (p. 377). Maybe so, and maybe the favors make the rural folk proud and happy back on their izbahs. But this book does not offer one shred of evidence in support of this interesting hypothesis.

In the last analysis, perhaps these folk really were the "instrument without which the rulers cannot rule," and Binder presents an ingenious theory of their relevance to Egyptian political development. They enabled Nasser and Sadat to minimize the political influence of any nascent liberal bourgeoisie or proletariat and to dampen any conflict between these urban forces. That is why liberals and Marxists find it so difficult to interpret Nasserism. "Egypt is the prototype of the situation in which there was a significant hiatus between the urban and rural political arenas" (p. 404), and this situation, one might add, enabled successive regimes to divide, rule, and paralyze the nascent civil society. But if the "social linkages between urban and rural segments become more important than the rela-

tively weak connections between the two political processes" (p. 404), an analysis of the urban process was needed to confirm them. As it is, Binder gives us only half of the story. The counterpoint to his interpretation of Egyptian politics is the philosopher's insight that the owl of Minerva takes flight only as dusk is falling, that is, that the second stratum is already losing its historical role. The understanding, backed by meticulous research over a 15-year period, was no mean achievement for an American political scientist. This book will be a minor classic and probably also a sort of twin testament to the epochs of Egyptian rusticity and American behavioralism.

CLEMENT HENRY MOORE

*University of Michigan*

**Power and Manoeuvrability: The International Implications of an Independent Scotland.**

Edited by Tony Carty and Alexander McCall Smith. (Edinburgh: Q Press, 1978. Pp. xv + 185. £6.95.)

The Scottish independence movement has generated a growing body of literature. This collection of six articles considers the extent to which an independent Scotland would be free to alter international political and economic arrangements to suit its interests. Detailed scenarios illustrate the constraints and likely options in politics, land control, oil, trade, and monetary policies. All are serious and scholarly analyses, though nationalist sympathy is not lacking. Americans are generally ignorant regarding this situation, and, whether one thinks that Scotland should or will be independent, understanding what might occur if it were, is of considerable practical and theoretical importance.

In his chapter, "Politics," Stephen Maxwell outlines strategic and tactical moves Scotland might make and analyzes the likely reactions of Westminster, the EEC, the U.S., the oil companies, OPEC, and the Nordic nations. He illustrates with experiences of other small states—Norway, Eire, Switzerland, Holland, Iceland, and Canada. The scope and detail of this chapter are impressive, the writing crackles with good argument and the coverage provides an excellent short course in the topic of Scottish Nationalism. Oil was the catalyst that set things in motion, but Maxwell argues that there is a connection between the oil situation and a long-standing Scottish grievance, the rape of Scotland's resources for the good of Westmin-

ster. Rapid extraction of oil to set right the U.K. economy is the most blatant example; fishing is another. "Scottish waters contribute 50% of the total UK fish stocks and nearly 30% of the EEC stocks" (p. 5). That Westminster should have bartered away the access rights of Scottish fishermen in return for entry into the EEC rankles. For Maxwell, these uses of Scottish resources to bail out a failing U.K. explain the apparent paradox in which there is a splintering of the U.K. along traditional ethnic lines at the very moment when modern economic integration of Europe is taking hold. To wit, as long as Scottish resources are being bartered, Scots should control the bartering. In an increasingly interdependent Europe, such leverage would be worth having.

Alexander McCall Smith directs attention to two points of international law that are relevant and problematic: the right to self-determination and state succession. The former may be a problem for Scotland if it can be argued, and some insist it can, that "it is in no sense prevented from full, equal and real participation in the multinational state. . ." (p. 45). The problem of state succession takes us directly to the crux: ownership of North Sea oil. As Carty puts it:

"Traditional state succession involved only the substitution of one sovereign, who was often a monarch, for another, and left the legal relationship between individuals intact." . . . [This] is now highly controversial precisely because the distinction between private rights and public institutions is widely regarded as unsound (p. 115).

The first goal of an independent Scotland would be to follow Norway's example and impose strict control over the extraction rate of oil. Carty argues this would be impossible without claiming ownership. The licenses granted by the U.K. are ambiguous on this, but appear to extend to the oil companies control over depletion rates. A possible model to follow is found in the "OPEC revolution." There, the old concessions explicitly granting proprietary rights were scrapped for contracts in which the companies extract, refine and distribute the oil at a rate and price determined by the states who retain control. To follow this path, Scotland will risk the wrath of the oil majors and their influential backers, the large American banks. Should the companies refuse to deal, or should they choose to make a stand by scuttling their platforms, Scotland would be hard pressed to develop an extraction capacity. Even short of that, as Carty rightly notes, renegotiation is made more difficult since Scotland can

hardly claim, as Ghana and the OPEC states could, that the concessions were unfairly wrested, through shrewd dealing, from a primitive and unsuspecting people.

Oil, with its guarantee of surplus balance of payments, is the heart of two articles concerning trade and money in the small state. Dermot McAleese and John Purvis assume that Scotland will be a rich state whose problems will be to contain domestic demand and to invest oil proceeds against the future while avoiding inflation. Interesting analyses, intelligible to the non-economist, argue that an aggressive policy of competition in free trade, limited membership in the EEC, and a monetary policy alert to that of England—then as now the main trading partner—would be prudent courses of action.

These are fascinating and informative pieces that have about them the kind of insight and argument that illuminated our *Federalist Papers*. Critics can note that the authors tend to take lightly the tactics available to Westminster. Also, a devolved Scottish Assembly might well over-extend the SNP and dampen enthusiasm among its voters. The Shetland Islanders' apparent willingness to throw in with England, taking with them two-thirds of the oil, deserves more attention. And finally, the process of gaining independence may take so long that the oil could be nearly gone by the time its control fell to Edinburgh. But these are minor objections to a very good book. Together with Donald MacKay's collection of scenarios concerning domestic policy options, *Scotland 1980* (same publisher), *Power and Manoeuvrability* is well worth reading.

RAYMOND E. OWEN

*University of Pittsburgh*

**Germany at the Polls: The Bundestag Election of 1976.** Edited by Karl H. Cerny. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1978. Pp. 251. \$4.75, paper.)

No West German election to date has received more scholarly attention than the election of 1976, even though, as most observers would agree, it was not one of the most interesting or consequential elections since 1949. 1976 lacked the suspense and "new frontier" spirit of 1969 or the frenzy and issue focus of 1972. In the end, the 1976 election maintained the balance of power, albeit with a reduced margin; the CDU/CSU had won, as Kaltefleiter put it, without achieving victory.

*Germany at the Polls* is mostly concerned with political parties and their role in the electoral game. In the opening chapter, Loewenberg does an admirable job sketching the party development in West Germany since 1945. Conradt focuses on the parties in the 1976 campaign, especially their campaign strategies, and offers an assessment, based on aggregate data, of the outcome. Like Loewenberg, Conradt finds kind words for the "maturity" of the West German party system. A "stable turnover system" has evolved in which party coalitions have alternated in power and in which such alternations remain likely without loss of system stability.

The contributions to the volume by Sontheimer, Kaack, Kaltefleiter, and Noack review in more detail the election campaigns waged in 1976 by the four parties, the SPD, F.D.P., CDU, and CSU. Each of these chapters covers the intraparty ideological struggles, such as between the *Jusos* (Young Socialists) and the party establishment within the SPD, the conflicts over the drafting of the party platforms—with due attention to the controversial "freedom versus socialism" slogan of the CDU/CSU—and the presumable effects of such slogans on the voters' choice at the polls. Yet, except for the Kaltefleiter chapter, none of these contributions exceeds the level of quality journalism; Kaltefleiter draws on electoral surveys to highlight voter evaluations of the key campaign actors and the issues of concern to the mass public.

Two contributions of a very different sort round out the collection. The chapter by Schoenbach and Wildenmann is devoted to the mass media, particularly the coverage of the election campaign by the prestige press in the Federal Republic. The political editorials of these newspapers are examined by systematic content analysis. The authors draw some intriguing comparisons between the "profile" of that coverage and the profile of party platforms, speeches by party leaders as well as party evaluations on the part of the mass public. The final chapter, by Kaase, takes up some of the issues—as well as promises—raised by the spread of public opinion polling in the Federal Republic. Kaase notes the extensive collection of electoral surveys available to the scholarly community—through the service of the *Zentralarchiv* in Cologne, and through the ICPSR in Ann Arbor—but deplores the inadequate infrastructure for conducting university-based electoral research in West Germany.

In a volume like *Germany at the Polls* some overlap between chapters is unavoidable, but

the degree of redundancy in this volume is somewhat annoying. Two nearly identical tables appear in the Conradt and the Kaase chapters (see pp. 41, 46, and 221). The four single-party chapters cover a good deal of the same ground. Moreover, while each of them addresses the question of the outcome of the 1976 election, none is capable of answering it because the focus on a single party's campaign simply precludes that possibility. It is a critical flaw of this volume that a chapter "German Voters at the Polls," i.e., an analysis of voter choice in 1976, using survey data, is missing. To be sure, Conradt approaches the subject and Kaase devotes a good portion of his chapter to it, but this topic deserves far more attention than is allocated.

Not only will those curious about electoral behavior in West Germany be disappointed by this volume, but also those interested in new perspectives on German political parties. The various chapters in this volume add little to our conceptual understanding of parties or party systems, and the reader waits in vain for a new or innovative twist in the study of parties and elections, aside from the media chapter by Schoenbach and Wildenmann. These objections notwithstanding, this volume provides the general audience with a useful introduction to contemporary party politics in West Germany.

HELMUT NORPOTH

*State University of New York, Stony Brook*

**Commandos and Politicians: Elite Military Units in Modern Democracies.** By Eliot A. Cohen. (Cambridge, Mass.: Harvard Center for International Affairs, 1978. Pp. 134. \$8.95, cloth; \$3.95, paper.)

Arthur Schlesinger, the historian, was recently quoted as saying: "Some day, I'm going to conquer the art of writing short books." What Schlesinger has not yet mastered Cohen has—and very well.

Using primarily America's Special Forces (1952–1978), Britain's Commandos (1940–1945) and Special Air Service Unit (1941–1978), the French and Israeli paratroopers (1942–1962 and 1953–1977), the pre-State Jewish Palestinian *Palmach* (1940–1949), and Unit 101 of the Israel Army (1953), Cohen does essentially two things. He discusses the military and political costs of elite

units and then offers suggestions for managing them better within the democratic context of civilian control of the military.

What makes this book different from other studies of civil-military relations is that Cohen "looks at discrete units rather than the officer corps of an entire army," and more importantly, he devotes a great deal of time to examining how "politicians interject themselves into the soldiers' roles rather than the reverse" (pp. 16-17). Indeed, this is really the main point of the book.

The politicians not only interject but they protect: John F. Kennedy and the Special Forces, Sir Winston Churchill and the Commandos, Moshe Dayan and the *tsamkhanim*, or paratroopers. In different countries and circumstances and at different times each of them sheltered their favorites among the military units at their command. They did so because of varying combinations of elitism, activism, romanticism, and crisis.

But a price was paid—not only in Israel, Britain, and America, but in France, French Algeria, and other places as well. The price included a certain amount of military glorification, the blurring of the meaning of and relationship between peace and war, the demoralization of regular units, the misuse of elite units, the weakening if not outright disregard of the chain of command, a decrease in discipline, and an increase in atrocities.

As in any book, there are some contradictions here. For example, Cohen believes that elite units "can [and by implication do] damage various aspects of a country's security" (pp. 27-28). At the same time, he believes that as specialists they exemplify "a productive military division of labor," are useful military laboratories for new tactics, trends, and tasks, and may be "nurseries" for new leadership (pp. 30-35).

As for the specific management suggestions I mentioned earlier, Cohen suggests (1) restricting the size and role of elite units, (2) cutting their institutional autonomy to the barest minimum, (3) discouraging the publicity that has usually surrounded them, and (4) above all, restraining somehow the penchant for some politicians to play at being soldiers. "Democratic politicians," he concludes, "should resist the temptation offered by elite units. Without such restraint the defense of a modern democracy can only suffer" (p. 102).

From "internal evidence," as historians put it, it is obvious that Cohen is a relatively new member (B.A., 1977) of the Fraternity of Scholars. But he thinks clearly, writes concisely,

and makes the reader ponder. What more can we ask from a scholarly work?

EDWARD BERNARD GLICK

Temple University

**Metropolitan Latin America: The Challenge and the Response** (Latin American Urban Research, Vol. 6). Edited by Wayne A. Cornelius and Robert V. Kemper. (Beverly Hills, Calif.: Sage, 1978. Pp. 346. \$18.50, cloth; \$7.95, paper.)

If it can be said that the decade of the 1960s was one in which Latin American urban research was in its infancy, then it is fair to claim that by the close of the 1970s there have been strong signs of maturation. In no small measure the emergence of sophisticated work in this area may be attributed to Francine F. Rabino-vitz and Felicity M. Trueblood, who initiated the Sage Latin American Urban Research series. The present volume, the sixth and last in the series, reflects just how far we have come, and also shows how far we still need to go.

Wayne A. Cornelius, co-editor of two previous volumes in the series, along with Robert V. Kemper, focus their attention in this volume on nine Latin American cities, the problems they face, and the successes and failures each has had in coping with those problems. The articles follow a similar format, introducing the reader to a brief historical overview followed by succinct descriptions of the challenges which the cities face and the responses offered by the government. The editors are to be congratulated for their obviously successful efforts at maintaining the uniformity of presentation, and for their ability to include so much factual material in so little space. The bibliography of recent urban studies on Latin America, 1974-1976, updates the two earlier compilations included in volumes 2 and 4 of the series. It is to be hoped that even though the series is coming to an end, the very helpful bibliography could be continued, perhaps included in the publisher's *Urban Affairs Quarterly*.

The volume includes papers on most of Latin America's major cities: Bogotá (Allan Gilbert), Caracas (David J. Myers), Guadalajara (John Walton), Guayaquil (Richard J. Moore), Lima (Henry A. Dietz), Medellín (David W. Dent), Mexico City (Gustavo Garza and Martha Schteingart), Port-au-Prince (Simon M. Fass), and Rio de Janeiro (David M. Vetter and Ana Maria Brasileiro). Unfortunately, for reasons of

space, the editors did not include an article on Buenos Aires, and a contribution on Havana did not materialize. Nevertheless, the coverage is certainly broad enough to satisfy the requirements as a solid text for a graduate level course on Latin American urbanization. Undergraduate students might find the wealth of data a bit hard to assimilate, and might better profit from reading two or three selections from the volume coupled with other essays in earlier volumes.

My disappointment with this volume arises from my unfulfilled expectation that, as the final volume in the series, it would make an attempt at theorizing and theory testing. It would appear that enough research has now emerged that such an attempt can be expected. Indeed, some recent monographs on urban Latin America have attempted to do precisely that. Unfortunately, this volume largely restricts itself to descriptive case studies. Cornelius, in his introductory essay, states that the studies in the earlier volumes "provided a fragmented and incomplete view of the metropolitan phenomenon, making systematic comparative analysis difficult" (p. 7). While each of the essays does provide a comprehensive view of the city treated, the comparative analysis is left to the reader. Indeed, Cornelius states that "the contributors were urged to resist the temptation to make detailed comparisons between the metropolitan areas examined in their case studies and other Latin American cities" (p. 9). This limitation is unfortunate, one which restricts the book's utility. John Walton's essay on Guadalajara is perhaps the one most sensitive to the question of theory, but as he emphasizes in the conclusion of his article (p. 48), a case study is the wrong vehicle for assessing theory.

Finally, the reader can heartily agree with Cornelius' conclusion that "The thrust of the evidence presented in this volume is that the degree of concentration in many Latin American countries today is clearly excessive. . ." (p. 9), but isn't that a conclusion which has been well known for some time? Despite these shortcomings, this is a book which adds incrementally to our knowledge and is to be recommended to all urbanologists.

MITCHELL A. SELIGSON

University of Arizona

(New York: Holmes and Meier, 1978. Pp. xxiv + 349; xxi + 334. \$33.75, each.)

Major studies of industrial conflict have become a rarity in the American social science literature. The topic itself still attracts attention, but collections of essays on the vast scale of *Industrial Conflict* (1954), edited by Arthur Kornhauser, Robert Dubin, and Arthur Ross, or broadly conceived monographic studies, such as *Changing Patterns of Industrial Conflict* (1960) by Arthur Ross and Paul Hartman, seem to be a thing of the past. Most of the recent studies have eschewed ambitious generalizations, however tentative, in favor of more limited horizons, often explored with formidable quantitative research tools.

This work, an inter-European collaboration, follows the older model. Its massive two volumes contain 18 chapters prepared by 28 authors, mostly sociologists attached to research institutes and universities in Western Europe. Their inspiration was a shared interest in the increased workplace militancy which from the late 1960s on became, together with decentralizing trends in bargaining structures, characteristic of labor-management relations in most European countries. Subsequently the group's focus widened to encompass the study of industrial conflict at all levels of the industrial relations system, including the role of the state, the impact of the business cycle, and in some countries such as France and Italy the influence of changing ideologies and political alignments.

The first volume contains six country studies (Belgium, France, Italy, the Netherlands, Britain, and West Germany), together with an introductory essay on strike statistics in which Michael Shalev appropriately emphasizes the limitations of most such data. By and large the country studies follow a common scheme of organization in which the major subcategories are (1) economic, political, and social contexts; (2) new developments in the configuration of industrial conflict; (3) new demands put forward by workers; and (4) the changing nature of industrial relations systems. For comparative purposes, of course, a common format is highly desirable, but this particular sequence imposes a heavy burden on the untutored reader. Anyone not familiar with a particular national industrial relations system and its institutions (the organizations of unions and employers, the agencies of the state, the structure of collective bargaining, the role of law, etc.) will probably have considerable difficulty following the sometimes very intricate discussion of new forms of

**The Resurgence of Class Conflict in Western Europe Since 1968, Vol. I: National Studies; and Vol. II: Comparative Analysis.** Edited by Colin Crouch and Alessandro Pizzorno.



conflict and new kinds of demands without an introduction to the functioning of the industrial relations system.

The author of the Belgian chapter, Michel Molitor, disregarded the model outline and reviewed Belgium's industrial relations system before analyzing recent trends in industrial conflict. As a result his contribution is among the more lucid. Yet, like several other contributors he labors hard to demonstrate the existence of a resurgent class conflict and, I think, ultimately fails. Colin Crouch's review of British strike experience is by a wide margin the most polished, and despite problems of internal organization is probably most accessible to American readers. The chapter on France by Pierre Dubois, Claude Durand, and Sabine Erbès-Seguin is repetitive, acknowledges the existence of only the two largest labor federations (CGT and CFDT), and suffers from obscure formulations, some of which may be the result of poor translation. The Italian section by Ida Regalia, Marino Regini, and Emilio Reyneri is not only surprisingly readable but also contains a fascinating account of new strike techniques—spot strikes, rolling strikes, confetti strikes, registration strikes—invented or adapted by Italian workers to try to shift the cost of partial work stoppages to employers.

There must have been a substantial time lag between submission of chapters and actual publication in 1978, as indicated by the failure of the Dutch authors, Tinie Akkermans and Peter Grotius, to mention the January 1, 1976 merger of the Dutch socialist and Catholic trade union federations, one of the most important developments of the past decade in the Netherlands. To demonstrate the resurgence of class conflict in the Netherlands was a task on which the authors wisely did not waste much effort, although they properly call attention to a general hardening of labor-management relations. All the more determined is the attempt by Walther Müller Jentsch and Hans-Joachim Sperling to show that an intensification of class conflict (in the general sense) has occurred in West Germany in spite of most union leaders' readiness to negotiate collective agreements. According to the authors, the militancy of the German work force, however "undeveloped in comparison with that reached by the class struggles in Great Britain, France and Italy" (Vol. 1, p. 297), constitutes a repudiation of the theory "spread by the bourgeois social sciences" that the German working class is socially contented and has been integrated into bourgeois society. That interpretation is, to put it mildly, not well supported. The authors'

disdain for the alleged tameness of the official union leadership is shared by some of the other contributors.

The second volume contains 11 contributions that seek to explore on a more or less comparative basis new themes in industrial conflict. Several deserve special mention. Pierre Dubois summarizes recent trends in industrial disputes and finds—not too surprisingly—more radicalization, localization, and institutionalization. Richard Hyman reviews ongoing changes in occupational structures and their impact on working-class militancy. Roderick Martin compares the internal politics and administrative arrangements of German and British unions. His analysis is insightful, but its link to the theme of industrial conflict is rather distant. A similar comparison by Rainer Zoll examines German and Italian unions, though Zoll's emphasis, in contrast to Martin's, is on workplace organization and shop-level collective bargaining. Colin Crouch pulls together the findings of several country studies on the role of the state in restructuring industrial relations systems in an age increasingly characterized by corporatist elements. Frank Soskice suggests an economic interpretation of the 1968–1972 strike wave, taking special care to explain the largely unofficial character of the conflicts. Finally, in one of the more stimulating contributions, Alessandro Pizzorno proposes a theoretical idea to explain the political aspects of labor markets and their relationship to collective bargaining. He suggests that beyond the established concepts of individual and collective exchange relationships in labor markets that are designed to ensure performance and continuity of production, there also occurs a political exchange where the chief actors are the unions and the state and where the key object of exchange is the achievement or maintenance of a basic political consensus.

As in other works of this genre, there are wide gradations in quality which even the most meticulous editing can never quite overcome. Nevertheless one wonders whether the editors might not have been well advised to omit the weakest links, for example, the chapter on "Women and Immigrants: Marginal Workers?"—all the more so when its relevance to the work as a whole is tenuous. One should also ask whether more competent translations could not have been commissioned for some of the 11 chapters that were originally prepared in a language other than English. Though most are at least adequately rendered, and a few have benefited from very skillful attention, too many passages remain that after several readings

still refuse to yield up their meaning. For example:

Calling into question as they do a system of negotiation between capital and labour that is based on the buying and selling of skill-level and in which they can have no part, unqualified workers reveal by their actions the basis of the prevailing system of production on *lack* of skill. The worker is plainly reduced to a role as a mere force of labour over and above any ideological considerations the aim of which is to value work as a means of allowing the worker to have some control over his own exploitation (Vol. 2, p. 71, *italics in original*).

Admittedly it is not always clear whether such obscurantism is chiefly the responsibility of the authors or the translators.

One should try to end on a positive note. For a few years some of the chapters in the first volume may be useful for reference purposes. The second volume contains several essays that may stimulate further research and discussion. Social science libraries ought to acquire the work, provided they can afford it. Apparently a Hong Kong printing job is no guarantee of reasonable pricing.

JOHN P. WINDMULLER

*Cornell University*

**The Army and Politics in Indonesia.** By Harold Crouch. (Ithaca, N.Y.: Cornell University Press, 1978. Pp. 377. \$18.50.)

In 1965 the Republic of Indonesia was almost torn asunder by an attempted coup. In the reaction to that event over 100,000 people were killed and President Sukarno was discredited along with the institutions and policies of "Guided Democracy." Faced with massive political violence and severe economic and social dislocation throughout the country, the Indonesian army under General Suharto became the paramount force in the affairs of the archipelago. In his book, *The Army and Politics in Indonesia*, Harold Crouch, of the National University of Malaysia, effectively chronicles the military's rise to power and ably evaluates the results of the institutionalization of army rule over the past decades. His research, based on extensive field work, will not only provide the country specialist with new material but will also be invaluable as a case study for comparative scholars interested in the role of the military in the states of the transitional area.

Crouch notes that as a result of its involvement in the struggle for independence there was an "absence of an apolitical military tradition" among the officer corps (p. 25). Yet, the political roots of the military were tempered by the "Middle Way" concept of General Abdul Haris Nasution "according to which the army would neither seek to take over the government or remain politically inactive" (p. 1). This reluctance to assume power directly was further conditioned by the divisions within the military "based on personal, regional, divisional and political loyalties" (p. 28), the preeminent position of President Sukarno and the fragmentation and diversity within an ineffectual political process. Therefore, even during the uneasy cooperation that characterized the united front during the heyday of "Guided Democracy," the army was reluctant to directly challenge the growing power of the Communist party of Indonesia. It was only when the attempted coup took place on October 1, 1965 that the military carefully filled a vacuum that would be created by the destruction of the P.K.I. and the loss of President Sukarno's base of support.

In his chapter on the coup attempt, Crouch clearly presents the different interpretations of that watershed in contemporary Indonesian history. He ably gives a balanced presentation and tightly employs the extensive documentation that is now available for scholars of the crucial period. Nevertheless, while one can appreciate Crouch's understandable caution in assessing what transpired, it would have been interesting if he had presented more directly his own learned judgment on what took place. His excellent research provides background material sufficient to extend beyond primarily recounting the conflicting versions of the coup attempt.

The author masterfully describes the army's protracted and successful attempt to strip away President Sukarno's sources of support and legitimacy. The ability of General Suharto to neutralize opposition within his own ranks on one hand, and to render "Bung Karno" powerless on the other, is a classic illustration of the indirection that can be found in much of Indonesian society and politics. Crouch also is very successful at showing how the politically astute Suharto skillfully dominated the surviving political parties by offering them "a role in the system" while at the same time taking steps "to ensure that they were headed by amenable leaders prepared to cooperate closely with the government" (p. 234).

The institutionalization of army rule is further explained by the author's excellent

analysis of the army's system of "unconventional financing," where "selected officers at all levels were placed in positions where they could raise funds on behalf of the army" (p. 275). Of course, as Crouch notes, such funding was also used for private purposes and consequently led to incidents of massive corruption involving "the interests of the military elite and the civilian bureaucrats and the business groups—both domestic and foreign—closely linked with it" (p. 303). Crouch is to be praised for practicing a form of investigative reporting that is too rarely found among political scientists in skillfully unweaving the economic machinations of a section of the contemporary elite. He aptly shows what can happen when an army which justifies its rule on the basis of eliminating corruption is itself corrupted by taking on a variety of nonmilitary functions.

In the concluding section of the book, Crouch cogently raises the question of the long-term ability of the Indonesian military to continue to rule. While the army is to be credited for bringing a needed degree of political order and economic stability to a troubled country, Crouch's following statement illustrates the uncertainty of the past, the present and the future. "Although civilian frustration was acute, there seemed to be no prospect of removing the military from control of the government" (p. 351). Yet, in the long run, if the army can not remove the sources of that "frustration," there is always the possibility of new conflict in the super-heated climate of Indonesian politics.

STEPHEN SLOAN

*University of Oklahoma*

**Making Budgets: Public Resource Allocation.** By James N. Danziger. (Beverly Hills, Calif.: Sage Publications, Sage Library of Social Research, Vol. 63, 1978. Pp. 225. \$14.00, cloth; \$6.95, paper.)

James Danziger's well-organized volume on budgeting in British county boroughs should help us all see the variety of ways in which budgets can be used in political analysis. Neither a methodological tour de force, nor a theoretical maze, his book tells how budgets may be analyzed in straightforward terms, points out the limitations and general arguments for each approach, and neatly uses his careful empirical studies to illuminate and bolster his admirable eclectic conclusions. His book will be of great interest to students and

scholars who wish to have a compact and informed introduction to the politics of budgeting.

Citing the relevant theoretical literature and assessing its political significance in concise terms, Danziger examines the variations of the boroughs' budgets using the four main frameworks for budgetary analysis: demographic, incrementalist, organizational and process explanations. Those who like definitive conclusions might be disappointed that he concludes budget analysis needs "alternative explanations" (p. 219); however, in doing so, they will also miss the point. Budgets are an extremely rich source of information about local decision making and local democracy. They also provide diverse ways of disaggregating and testing macro-theories which the organization of the book clearly illustrates.

There has been considerable budgetary analysis of British towns and counties, the most ambitious previous attempt being Noel Boaden's *Urban Policy-Making* (Cambridge: Cambridge University Press, 1971). Contrary to the impression of more enthusiastic American budgeteers, studying local budgets in Britain has a history pre-dating our discovery of this data pool: for example, Claus Moser's and W. Scott's *British Towns* (London: Oliver and Boyd, 1961) which was written when local budget studies were only done in the United States by public finance experts. Local budgets also happened to be of immense interest to the early Victorian Benthamite reformer, Sir Edwin Chadwick, and to one of Britain's most distinguished chancellors of the exchequer, Sir George Goschen. All this means that sober reflection on what political science has added is welcome and useful. With less of an axe to grind than many who study budgets, Danziger helps us understand how much we have done and how much we have left to do.

Why budgets vary depends as much on the questions asked as on methodology or even abstract theory. Danziger's critique of the demographic explanations (pp. 82–83) will be welcomed by many for he shows how it represents a rather uncritical superimposition of sociological thinking on political analysis. The chapter on variations in budget allocations shows mastery of statistical skills and points to the need for further disaggregation of budget decisions. Whether this route will yield surprising or very illuminating results remains in question, even with Danziger's analysis. One wonders whether finding more statistical variation as one breaks down major services into their components (p. 63) or deals with smaller

budget items (p. 75) might be simply an artifact of the method. The more surprising finding would be to have *less* variation as we subdivide budgets into smaller amounts. Whatever may be the results, Danziger's use of simple measures like the coefficient of variation seems promising if one wants to capture internal variations of decision making rather than the tried, tested and over-applied correlational techniques which blur internal variation.

Perhaps the most valuable chapter deals with organizational explanations which have on the whole been neglected by political science. Here Danziger displays his own sensitivity to the internal workings of British local government and his mastery of more complex methods. Many can do these things separately, but bringing them together is ingenious and rare. He does this by simply describing the variety of local budget processes in the four towns he has studied in more detail. Alert to the limitations of more complex mathematical models, he makes a careful evaluation of the Cyert-March simulation of bargaining under conditions of uncertainty. He points out (p. 157) that 'Crescine's model of municipal spending permits five decisions per department which are only enough to last most municipal department heads until lunchtime. Of course, simulation is only the tip of the iceberg in organizational theory so one hopes that Danziger's exploration will not be the limit of this avenue of inquiry.

A refreshing and lively book on a subject that has been sifted through many sieves is not easy to write. Danziger's firm grasp of the major theoretical distinctions surrounding budget analysis combined with a resourceful and flexible use of findings make his book valuable.

DOUGLAS E. ASHFORD

Cornell University

**Revolution and the Transformation of Societies: A Comparative Study of Civilizations.**  
By S. N. Eisenstadt. (New York: Free Press, 1978. Pp. xvi + 348. \$15.95.)

This book, like much of Eisenstadt's earlier work, is very difficult and time-consuming. I cannot pretend to have mastered its scope and detail, but its general thrust seems clear enough, and I examined a few of the major arguments with some care.

The principal thesis of the study appears to be that revolution should be seen as a "special process through which social change and trans-

formation takes place" (pp. 9, 329), rather than as a special kind of change. The "pure," "true," or "classical" revolutions, also called "modern" revolutions, were the Revolt of the Netherlands, the Great Rebellion and the Glorious Revolution in England, the American Revolution, and the French Revolution (p. 173). (It is not clear whether the Russian, Chinese, and other communist revolutions were "pure," although they appear to have been "modern.") These revolutions, however, are not treated consistently as processes: the author repeatedly refers to them as "far-reaching" changes or transformations, with particular scopes and goals (e.g., pp. 68, 173, 215, 334). Sweden, Denmark, and Switzerland, moreover, were transformed as were those societies that experienced "pure" revolution without, however, going through the *process* of revolution (p. 197). Perhaps the issue is only terminological: a distinction between "revolution" (a process) and "revolutionary transformation" (a result) is made early (p. xv), but it is not pursued. We also are told (p. 9) that there are "different types" of revolution, but subsequently nothing is said about those that are not "pure."

Eisenstadt's concept of revolution, in short, is at best ambiguous. This in itself is not too serious. Although revolution might not be identical with a special process, something still could be said about the processes associated with the "far-reaching" changes normally considered revolutionary. The author says that the "structure of elites" *explains* "the dynamics of change of different societies" (p. 105). Yet he assumes that the concept of elite is precise and meaningful and, as far as I could determine, does not go beyond saying that, given the proper conditions, such as technological development, the more members of the elite there are and the more influence they have, the more likely that a revolutionary process will occur. There are, however, many apparently independent variables connected with revolutionary processes—characteristics of elite persons and conditions under which they operate, for example—so many that the study's conclusions are presented as catalogues (e.g., Ch. 7). The general theoretical relations among the items of these catalogues were most unclear.

The kind of "grand theorizing" Eisenstadt has presented is impressive, but I learned little about civilizations from it. There were too many undefined concepts, too much borrowing from tired theories, such as the "class" analysis of revolution, and too many subtle distinctions. Reflecting that the fault may have been mine rather than the author's, I looked more careful-

ly at a specific problem in which I have some interest and background: the nature of "traditional" society—that is, the kind of society that preceded the modern and in which the revolutionary process (or change) occurred.

Whether revolution is a process or a result, it was "modern"—that is, an innovation—in the "pre-modern" society in which it occurred. The classic problem, of course, has been to account for the "pure" revolutions that occurred only in Western Europe and (marginally) in the United States, to discover something in the "pre-modern" societies of these areas that did not exist in "pre-modern" societies elsewhere. But, logically prior to this, there is the problem of specifying the nature of the "pre-modern," or as it is often called the "traditional." Eisenstadt's treatment of traditionalism deals at length with what he calls patrimonial, tribal, city-state, imperial, and imperial-feudal regimes, with some attention to exceptional city-states and tribal federations. The imperial and imperial-feudal systems include the Russian, Byzantine, and Chinese empires and Islamic and Western European civilizations.

Three chapters covering some 120 pages are devoted to a close examination, in the author's complicated manner, of "patterns of change" in these traditional societies. At the end of this section (pp. 159–61), Eisenstadt concludes that low levels of technology and organization, a limited conception of the possibilities of human innovation, and the political policy of divide and rule limited the "transformative capacities" of traditional society. I encountered this rather conventional conclusion stated in ordinary language with relief, for now I knew what I had been reading for the preceding 117 pages. When I then returned to analytic topics, such as the "structure of centers" and "center-periphery relations," and to the case-studies of specific regimes, I was more comfortable, but still I found only variations on the principal themes of traditionalism mentioned above.

It seems to me that Eisenstadt's model of traditionalism leads to the conclusion that traditionalism's basic characteristics not only limit "far-reaching" transformations but also prevent them (pp. 160–61). In other words, his analysis has precisely the weakness he criticizes in the "structural-functional approach" (pp. 26–27): stressing the interrelatedness of the components of a society—its ideology, stratification, elite members, social systems, technology, what-have-you—results in a logical construct that cannot even acknowledge "fundamental change," much less account for it. This, in my opinion, is the lesson to be learned from the

present book: the comparative method, applied to "governments" or to "civilizations," is static and backward-looking. No matter how broad the scope and how refined the distinctions, revolution (as either process or result) cannot be understood in the way the author here has tried to understand it.

As a postscript, I do not believe it captious to take issue with the dust jacket: I cannot see how the book could provide "an entirely new dimension" to and an "important breakthrough" in the study of revolution, nor create "an eminently useful vantage point" from which to understand present and future civilizations.

C. W. CASSINELLI

*University of Washington*

**Political Development and Bureaucracy in Libya.** Omar I. El-Fathaly, Monte Palmer and Richard Chackerian. (Lexington, Mass.: D.C. Heath, 1977. Pp. ix + 122. \$12.50.)

Libya today is a unique specimen in the Middle East political aquarium. Like the states of Arabia, it remains a largely tribal-patriarchal society, endowed with great oil wealth, and just beginning to mobilize socially. But unlike the case in the Arabian states, a "modernizing" middle-class military leadership, on the Nasirite model, has made a breakthrough at the center, displacing the tribal monarchy by coup d'état, poising itself atop the bureaucracy and repressing the rudiments of party and ideological politics in the name of national unity. Thus, like its Nasirite predecessors, Libya's special problem is to bridge the gap between state and society and mobilize the population for development. But in Libya the response has taken a peculiar twist which, seeming to mix puritanical desert Islam with radical mass-organizational practices, leaves the observer baffled. This book is valuable because it gives some fascinating glimpses of these developments, and because, owing to a laudable collaboration between Libyan and Western social scientists, it brings to the analysis empirical techniques and evidence.

One survey, showing that the bulk of Libya's rural population hold strong particularistic, fatalistic and non-participant attitudes and support their traditional tribal leaders, and another survey, showing that these leaders are themselves indisposed to change and hostile to the regime's efforts to penetrate their local bastions, suggest the challenge faced by the new leadership. Its first response, essentially bureau-

cratic, was to replace traditionals with modernizing officials at the levers of local government, but attitude surveys show that, owing to the low priority the new officials gave to popular participation, they failed to win local political leadership. A subsequent effort to create a single party on the Egyptian model likewise foundered on excessive bureaucratization and the tenacity of traditional leadership. It was a perception of these mobilizational failures, according to the authors, which led Qaddafi to embark on his famous "cultural revolution," culminating in the erection of a new political structure called "People's Power." This is a Soviet-style pyramid of locally elected popular assemblies and committees which elect provincial and ultimately national bodies, vested in principle with powers of legislation and control of the executive. The evidence suggests that the new system has succeeded at least to the extent of recruiting a new third type of leadership, distinguishable from the traditional chiefs by their modest origins, and from the officials by anti-bureaucratic attitudes. Unfortunately, the authors do not give us attitude data on their orientation toward modernization and tradition, nor do they explain why a population evidently committed to its tribal chieftains did not elect them as their representatives.

The book could be challenged for a too-uncritical acceptance of the regime's own definition of the problem, namely that the obstacles to participation lay mainly in the traditional character of the population and the influence of tribal leaders. Owing to this approach, the authors take no cognizance of the serious doubts raised by Ruth First (*Libya: The Elusive Revolution*, Harmondsworth, England, 1974), as to the willingness or competence of the new leaders to mobilize participation. According to her, the regime has refused to allow the development of a true revolutionary party, virtually excluded politically active progressive intellectuals from a share of power, and, while calling for popular participation, "suffocates any political action or thought not initiated by the state." The authors could also have considered whether "People's Power" might be less a modern mobilization instrument than a new variant of old Islamic practices—some mixture of prophecy and patriarchal chieftainship which consults with the people, but reserves all decisions to itself and brooks no opposition. Methodologically, one can quarrel with the operationalization of several of the attitude dimensions. A more serious defect is the abominable organization and editing of the book. The chapters are badly integrated, repetitious,

overlapping parallel presentations rather than a continuous and coherent argument. Chapter 3, a badly written mishmash of others, has no place in the book, and the last chapter, a rather esoteric discussion of public administration theory, seems barely related to the main thrust of the study. There are also numerous typographical and stylistic errors. Unfortunately, the format and presentation does credit neither to the authors' research nor the judgment of their editor.

RAYMOND A. HINNEBUSCH

*American University in Cairo*

**In Search of Identity: An Autobiography.** By Anwar el-Sadat. (New York: Harper and Row, 1977. Pp. viii + 360. \$12.95.)

The significance of autobiographies to political research has been widely debated by scholars. As self-portraits, autobiographies are too often designed to accentuate the positive aspects of a given leader's personal attributes and behavior. Well-written autobiographies of leaders in office could well constitute an effective means of projecting a favorable image to maximize public support; to the leaders of yesteryear these self-portraits or memoirs are a means "to set the record straight" by justifying their actions taken while in office. In either case it is safe to assume that virtually all such volumes are self-serving in varying degrees; nevertheless, they should not be dismissed as exercises in propaganda. When analyzed properly, autobiographies can be a most valuable source of data for historians and political scientists alike.

To writers of conventional history, autobiographies constitute "received" or "official" sources of fact and interpretation which are scrutinized along with other source materials in their efforts to reconstruct past environments of human activity. To students of political leadership, on the other hand, autobiographies are often indispensable as a source of data to determine the basis of elite behavior. President Sadat's *In Search of Identity* is no exception. It tells us perhaps as much about the man and his milieu as did Charles de Gaulle's memoirs and Richard Nixon's most revealing self-portrait, *Six Crises*.

Indeed, in contrast to the conventional historian, the primary concern of the student of leadership is not the validity of facts as such, but the autobiographer's personal view of specific events, personalities and develop-

ments which have affected his life since his formative years. In this sense the Sadat memoirs contain substantial information if one is concerned with identifying the components of his basic character and personality—how he orients himself toward the world environment, toward the Arab-Israeli conflict, the great powers and the sweep of history itself. In Sadat's case one can identify and trace the modalities of interaction between his character and his evolving environment and point to certain patterns of behavior which are likely to repeat themselves in his dealings with both friends and adversaries.

It follows that *In Search of Identity* is a veritable treasure chest for psycho-historians and elite theorists. However, two words of caution need be said before a serious psychological study is contemplated. First, memoirs constitute a good starting point and the need for additional data is a certainty. Second, the scholar must resist the temptation of blindly applying purely Western psychological constructs and paradigms to the study of a non-Western personage. The use of Freudian methodology, for example, would almost certainly produce scientifically untenable results. Even Erik Erikson's seminal study of Gandhi is plagued to an extent by the problem of cultural relativism. What is needed is an eclectic theoretical framework which is not psychoculturally blind to peculiarly Egyptian traits of personality and environment.

Meanwhile, what Sadat has chosen to tell us about himself confirms the impressions of outside observers. True to his deep roots in his native Munufiyyaha province, he is a most patient and careful planner and master of timing. Lacking the political "capital" of his charismatic predecessor Gamal Abd al-Nasser, Sadat possesses an unusual shrewdness, flexibility and sensitivity to the world milieu. Perhaps the most salient pattern that emerges is his tendency to take sudden dramatic initiatives calculated to surprise and shock foe and friend alike. Buffeted by problems that seem to defy quick solutions, Sadat uses dramatic exploits as a source of domestic legitimation and as a means of placing his Arab, Israeli and American counterparts on the defensive. In this sense the parallel between Sadat's dramatic moves—the Soviet exodus of 1972, the Oct. 1973 War, the Jerusalem visit—and Nasser's performance are more than striking. However, Sadat is less rhetorical and far more cautious with respect to Israel and other potential adversaries. In planning his quantum initiatives Sadat is a loner; his decision making is a very personal and private

matter pursued in cool detachment through a mixture of rational analysis and intuition. His political behavior springs from a strong sense of self-confidence forged in prison and years of revolutionary activity, and from the deeply held convictions of a proud Egyptian nationalist.

Ignorance of Sadat's basic character traits has led to underestimation of his potential for successful leadership by political leaders, scholars and seers alike. The Egyptian president is painfully aware that his and Egypt's capabilities have been repeatedly underestimated by Israel, the Soviet Union, the United States, and others. This he has turned to his advantage by unleashing "shock effect" initiatives when least expected.

Readers of differing perspectives will find a great deal in these memoirs which might be considered objectionable. Indeed, there are a number of gaps in Sadat's account which betray the Egyptian leader's blind spots and shortcomings. A case in point is Sadat's general unconcern with the economic situation at home which reflects his order of priorities the first of which is Arab-Israeli peacemaking. Meanwhile, the home front is bound to remain a vulnerable area for the Egyptian president in the foreseeable future. Another possible danger is the tendency to achieve a world-wide impact as peacemaker too zealously, although Sadat is too much of a pragmatic politician to engage in messianic acrobatics.

RICHARD H. DEKMEJIAN

*State University of New York, Binghamton*

**Black Power in South Africa: The Evolution of an Ideology.** By Gail M. Gerhart. (Berkeley: University of California Press, 1978. Pp. xi + 364. \$14.50.)

Over the years black political aspirations in South Africa have constantly been blocked by the white system and Africans have only marginally participated in the realm of white politics. But this has never meant that Africans have not engaged in politics. Within the nationalist movements, political recruitment, political socialization, and the aggregation and articulation of interests have all occurred with increasing intensity. Despite harsh and overwhelming difficulties and the hostility of the white system, Africans have financed and organized nationalist movements, held meetings, disseminated publications, communicated with branch organizations, and traveled across the

country often at grave personal cost. It is possible to trace a linear development in African politics from the African National Congress (ANC) in 1912, whose leaders sought acceptance by the white system for an educated elite, to the violence and militancy of the Soweto generation of the 1970s. Indeed, the increasing violence, both physical and psychological, done to Africans by the white system has been reflected in a parallel frustration and escalation of violence by some blacks.

Many questions have arisen concerning the politicization of Africans in South Africa. How deep have the differences been between the ANC and the more militant Pan-Africanist Congress (PAC)? What factors have led some Africans to commit themselves to a violent rather than constitutional struggle? Why have some factions accepted the involvement of white liberals and others rejected it? What would be the place of whites in a future black state? What means would be acceptable for the achievement of black political goals? What relationship has existed between the black urban elite and the masses? And finally, to what extent are factors such as age, personality, class, and personal experience relevant to the differing strands of African political thought?

Gail Gerhart, in an important study, *Black Power in South Africa: The Evolution of an Ideology*, addresses many of these fundamental questions. She looks at the divisions of black leadership and the evolution of black consciousness in the 1940s and 1950s by leaders such as Anton Lambede, Jordan Ngubane and Asby Peter Mda. These were political leaders and theorists who were concerned with the psychological damage done to Africans in a white-dominated society and who sought to inspire the masses with a sense of their own dignity which they believed in turn might lead to political action.

Gerhart correctly traces and analyzes the evolution of this increasingly significant strain of African political thought and examines the role of the African urban intelligentsia in the process. The distinction she draws between orthodox or "Black Power" nationalism and what might be called the *liberal or non-racial nationalism of the ANC* is vital to our understanding of African politics in the period since the Second World War. Orthodox African nationalism (also sometimes called "exclusivist nationalism") defines South Africa as a country belonging to Africans by right of first possession, and on the grounds that they are the great majority of the population. Furthermore, for the orthodox nationalist, whites are guests in

the African house permitted to remain in Africa only on terms set down by their indigenous hosts. Non-racial nationalism has aimed at a pluralistic sharing of power, while orthodox nationalism has tended towards a "winner-take-all" view, with Africans winning power to control the state and, ultimately, the economy through the state. Orthodox nationalism for the most part leaves the future fate of whites unclear, apart from suggesting that they should somehow merge themselves into the predominant African population.

The overriding question is whether these strands of thought and action constitute a fully fledged ideology or even an emerging ideology. More accurately, at this point, they are perhaps expressions of militancy. Ideology implies an all-encompassing integrated system of social and political action and thought, and as such differs from the more restricted notion of militancy. Violence and revolutionary strategies do not in themselves constitute an ideology. While there are clearly ideological elements in the thinking of more militant African leaders, it is doubtful whether an ideology has emerged. Most Africans continue to see the struggle as one against *apartheid*, but large numbers still differ with the more militant approach to resolving the problem. An ideology should receive validation and acceptance by at least a significant segment of the masses or else it remains a theoretical concept.

Gerhart's analysis is provocative and interesting. She is aware that she has deliberately overdrawn the ideological positions in order to make distinctions clear, and her device succeeds. Her polarized position emphasizes the roots of militancy in South Africa and points to the increasing possibility of violent upheaval in that country.

Without doubt this is one of the most important books to emerge in Southern African studies in the past several years. It is an interesting fusion of theory, analysis and historical information. The book not only helps us to understand important figures of the past, but also the relevance of contemporary leaders such as the late Steve Biko and Robert Sobukwe. Perhaps the greatest importance of *Black Power in South Africa* is that it provides us with insights into a future in which black power and white intransigence might confront each other.

PATRICK O'MEARA

*Indiana University*



**Health Insurance Bargaining: Foreign Lessons for Americans.** By William A. Glaser. (New York: Gardner Press, 1978. Pp. xiii + 265. \$18.00.)

That this book can't be given unqualified praise is a reflection more on the state of the discipline than on the quality of this work. Glaser defines national health insurance quite conventionally as a method for organizing a country's spending for doctors, hospitals and other providers of health services for all citizens. Reasoning that the United States is the only industrial democracy without a broad-gauged public health insurance plan and that we may soon adopt a comprehensive program, Glaser concludes that we should learn from the experience of other nations.

Approximately 80 percent of the book is given to a country-by-country description of the national health insurance schemes in Canada, France, Belgium, the Netherlands, the Federal Republic of Germany, Switzerland, Sweden and Great Britain. In each case, Glaser focuses on describing the structure of the health insurance system with particular stress on the machinery used to determine physicians' fees. Except for Great Britain, where National Health Service doctors are direct public employees, sick funds similar to Blue Cross/Blue Shield receive and disburse funds for health services, using a fee schedule developed by negotiation between the largest medical association and a semi-autonomous public agency. The character and structure of these negotiations is described in some detail and the strengths and weaknesses of each national health insurance system are listed by the author. Lamentably, the book contains no comparative economic statistics. A comparative analysis of the median income of doctors and the percent of GNP allocated to health services in each country surveyed might be revealing and certainly would have increased the utility of this book.

European experience may not be a particularly good predictor of the pace or direction of the American move toward national health insurance. When national health insurance was phased-in abroad, occupational groups were covered successively. Among the national health bills pending in the Congress, only the Kennedy bill proposes a full-blown, European-style national health insurance plan. Competing bills, according to Glaser, "do not constitute national health insurance at all and are methods of . . . expanding existing nonprofit and for-profit insurance" (p. 210). The competing bills maintain the "usual and reasonable charge"

method for determining the doctor's fee and expand medicare health insurance to include children, the unemployed, and victims of catastrophic illness.

Implementation of national health insurance in the countries surveyed commonly produced certain policy problems which might be anticipated in the United States should the Congress pass the Kennedy health bill. In every case, national health insurance was based on a negotiated fee schedule rather than the "uniform and reasonable fee" system used by medicare. In most cases, introduction of national health insurance led to the development of militant unionism among hospital doctors, interns, and residents and consequent large and unanticipated increases in the cost of hospital care. To complicate fiscal problems, national health insurance costs not covered by employer-employee contribution must be covered from general revenues. Thus, national health insurance is inherently entitlement legislation and government expenditures for national health services are uncontrollable in precisely the same sense that social security costs are uncontrollable.

The European experience, as explained by Glaser, suggests that the adoption of national health insurance in the United States will occasion a radical restructuring of relationships within the medical fraternity and between the medical fraternity and the government. Adopting national health insurance will also signal the fiscal and organizational expansion of the federal government into a new and important area of activity. *Health Insurance Bargaining* has made an important contribution to the developing literature on this timely topic.

GAYLE AVANT

*Baylor University*

**Fascism in the Contemporary World: Ideology, Evolution, Resurgence.** By Anthony James Joes. (Boulder, Colo.: Westview Press, 1978. Pp. xvi + 238. \$17.50, cloth; \$8.50, paper.)

Central to this book is the argument that fascist regimes similar in ideology and structure to Mussolini's in Italy constitute a logical and quite rational response to the problems of modernization in late-developing nations. Anthony James Joes views fascism not as an aberration that can be exorcised or wished away and certainly not as a phenomenon of a given time and place—i.e., Europe between the

wars. Rather, he sees fascism as arising and continuing to arise as a definite reaction to the need for discipline, order, and authority in modernizing nations torn by factionalism and the possibilities of disintegration and seeking to achieve integral national development.

In this study Joes explores the similarities between past and contemporary fascisms and seeks to explain the circumstances in which fascism develops, to whom it is attractive, and what its recent reemergence signifies. In the first part of the book he examines the development of fascism in Italy and uses the Mussolini regime as his prototype. He arrives at a definition of fascism that stresses the following factors: nationalism, repudiation of parliamentary liberalism, statism, productionism, corporatism, authoritarianism, and elitism. Armed with this model, Joes proceeds in part 2 to an examination of fascism in the developing world. He finds characteristics of and tendencies toward his fascist model in regimes as varied as Meiji Japan, Perón's Argentina, Nkrumah's Ghana, Nasser's Egypt, and the present Brazilian and Peruvian regimes.

Joes writes in the provocative manner of A. James Gregor, whose writings on fascism he quotes often and who has added a foreword to the book, and his analysis contributes many insights. He argues forcefully for understanding the complex multicausality of Italian fascism rather than relying on any single, monocausal explanation. He develops some fascinating parallels between the Italian situation prior to Mussolini's coming to power (delayed and dependent economic growth and industrialization, incomplete national integration, unsuccessful experimentation with Western liberalism) and the frustrations felt and the search for new, alternative models experienced by many developing nations today. He concludes provocatively that for Third World leaders fascism represents a more widespread and attractive response to the wrenching problems of modernization than does either liberalism or socialism.

Although many of Joes' arguments and analyses are interesting and persuasive, the book also gives rise to a number of problems. Probably the foremost is the use of the term "fascism." First, many students of fascism in Italy would define it quite differently from Joes, perhaps stressing its totalitarian nature, its secret police, its racial persecution, its "behemoth" qualities, as distinct from the rather mild authoritarianism described in this book. Second, Joes has a problem in his effort to apply the fascist model to the present-day developing nations. On the one hand, there is so

much emotive content to the term "fascism" that its use as a precise analytical tool has been almost entirely lost. On the other, it strains credulity to use the term in reference to such varied persons and regimes as Nasser, Perón, Vargas, Nkrumah, Senghor, Nyerere, and the Brazilian and Peruvian militaries, a number of whom consider themselves syndicalists, socialists, or perhaps corporativists, but certainly not fascists.

In using the fascist label to describe such diverse regimes, Joes has both cast his explanatory net too wide and got himself in a mesh of troubles. If he had focused, as other scholars are presently doing, on the nationalist, statist, corporatist, authoritarian, developmentalist, and elitist nature of these systems, both the Italian prototype and the Third World parallels, he would have been on much more analytically surer—and safer—ground. It is the use of the term fascism and its broad sweep—too broad—that gets Joes in difficulties, as it has Gregor. One senses that Joes recognized this, albeit too late, since he has a fascinating appendix called "A Closer Look at Corporatism" which is far less emotionally charged than is his fascism analysis. Had Joes proceeded to examine the Mussolini system and those Third World nations he discusses in terms of the more neutral set of characteristics he himself had identified, he would have produced a sounder analysis. As it is, one must pick through the analysis for its useful insights since the main theme is not argued convincingly.

HOWARD J. WIARDA

*University of Massachusetts*

**Divided Loyalties: British Regional Assertion and European Integration.** Edited by Martin Kolinsky. (Atlantic Highlands, N.J.: Humanities Press, 1978. Pp. viii + 216. \$19.25.)

How can the United Kingdom accommodate two apparently opposing forces of radical political change, European integration and the devolution of power to regional assemblies? The question could as well be asked of Belgium, France, Italy or Spain, each of which is experiencing the simultaneous stresses imposed by integration and sub-state nationalism. These came to a head in Britain after the general election of October 1974 which saw the Scottish Nationalist party emerge as the second largest in Scotland and *Plaid Cymru* make important gains in Wales. Because of internal

party divisions the Labour government had already committed itself to reviewing British membership in the EEC and this was put to a referendum and approved in June 1975. In the fall of 1976 proposals to establish regional assemblies in Scotland and Wales were introduced in Parliament.

This book, a collaborative effort by 12 British scholars, does not take us far beyond the question with which we started—how can the two policies be accommodated?—but it does make a useful contribution by setting the scene and clarifying some of the problems. It describes the attitudes of the political parties and other groups and identifies three sets of relationships which have to be considered—the U.K. and the EEC; the U.K. and the regions, Scotland, Wales and Northern Ireland; and the EEC and the regions, a particularly interesting new topic. Edward Moxon-Browne, for example, finds that direct relations between Northern Ireland and the EEC are bringing a new dimension to the Irish problem. Martin Kolinsky discusses changes in parliamentary activity and, in the best essay in the collection, Helen Wallace deals with the constitutional and administrative problems posed by integration and devolution. She is particularly effective in placing them in a context which includes civil service and local government reforms between 1964 and 1975. The result, she argues, has been an unsettling climate of continuous administrative reorganization.

The great failure of British government in this period of great activity is summed up by Wallace when she writes, "No clear conception of a wider strategy of constitutional change has at any point been evident" (p. 91). This is also the great weakness of the book. More than a decade of feverish activity has left us with no clear sense of constitutional coherence in the United Kingdom. Kilbrandon's *Royal Commission on the Constitution*, which recommended devolution, refused to review the constitution in any fundamental sense. But for the referendum defeat last March, the U.K. might soon be governed in six different ways, Scotland by an assembly with executive and legislative powers, Wales by an assembly with only executive powers, Northern Ireland by some form of power-sharing and England by the U.K. Parliament. That Parliament would also have certain responsibilities for the U.K. as a whole and the EEC would have authority over the country as a member of the community. This would amount to six different, if overlapping, levels of authority. Furthermore, the relationships between these levels would be ambiguous, largely

because of the uncertainties inherent in an unwritten constitution and the rule of parliamentary supremacy. The authors are aware of this constitutional chaos but do not address it directly or explore remedies.

This constitutional disarray has a number of explanations. First, as Wallace suggests, coherent policy making is inhibited by the internal divisions within the major parties. Second, the government has reacted to the fait accompli of the EEC and the threat of disintegrative sub-nationalism with policies which are primarily expedients rather than reasoned constitutional innovations. Third, since the days of Irish home rule, the United Kingdom has responded to challenges to the regime by trying to legislate radical changes in political institutions without radically changing the principle of parliamentary supremacy. This process continues today and is a tribute not to a flexible constitution but to an inflexible one. Fourth, the constitution and the Westminster model both operate to centralize power in the hands of the executive and the bureaucracy. Those who have power are pathologically unwilling to surrender it and one of the ironies of devolution and integration is that, as this book demonstrates, the central government has been strengthened as it acts to coordinate policies between the U.K., Europe and the regions.

These essays, and most British academic writing, share with the government and the major parties an apparent unwillingness to come to terms with the real condition of the constitution.

ALAN J. WARD

*College of William and Mary*

**The Working Class in Welfare Capitalism: Work, Unions and Politics in Sweden.** By Walter Korpi. (London and Boston: Routledge & Kegan Paul, 1978. Pp. xxi + 448. \$25.00.)

Since Marquis Childs' *Sweden: The Middle Way* (New Haven: Yale University Press, 1936), Sweden has held quite a fascination for Americans. Here was a powerful labor movement which successfully engineered an alternative to the fascist and communist polarization of the 1930s with its pursuit of welfare state Keynesianism. The result was, or so it seemed, class harmony. As most advanced nations followed step in the postwar era, the class harmony thesis was generalized to all affluent societies in the form of "end of ideology" or "logic of industrialism" theories. Reality followed by

social research has generally invalidated these optimistic assumptions but, unfortunately, research has not yet given us much information on what, alternatively, the real nature of modern class politics is. Walter Korpi, one of the most interesting Swedish sociologists, gives this old debate a remarkable twist and offers a new perspective on working-class politics which demands serious attention.

In his central thesis Korpi delivers a simultaneous attack on pluralism and simplistic Marxism. He holds that the Swedish social democratic welfare state (it is unclear whether we can generalize beyond Sweden) has not made workers embrace bourgeois society, nor has it arrested the formation of a socialist consciousness.

Korpi understands class conflict in terms of a political process model which stipulates that the nature of conflict varies with the distribution of power resources in civil and political society. The militancy of French workers, for example, may reflect their lack of power resources just as the tranquility of Swedish industrial relations may testify to the vast power of working-class organizations. The accumulation of class power is not merely the immediate result of inherent tendencies in capitalism's maturation, but is also conditioned by the political and economic achievements of the labor movement itself. Power resources flow from the level of working-class solidarity which, in turn, depends on the types of reforms and class organization which have evolved. Moreover, the growth of power accelerates the drive towards democratic socialism because additional power resources will be brought to bear on lingering social inequalities.

The bulk of Korpi's monograph subjects this general theory to the empirical test. Following a brief profile of the history of the Swedish labor movement, Korpi marches us through a detailed analysis of working-class attitudes based on a sample of Swedish metal workers. He covers an amazing variety of issues such as extrinsic-intrinsic job satisfaction, shop floor conditions, union leadership and structure, worker influence, and so on. The treatment is methodologically excellent, but substantively a little tedious and at times the variables appear rather peripheral to the book's central argument. The sample is large and often the coefficients reported are not especially strong. The cumulative evidence, however, manages to destroy conventional myths of a prevailing economism and narrow instrumentalist working-class consciousness. The workers do appear to view themselves as part of a collectivity with

a strong sense of class solidarity; such is true of Swedish metal workers, at any rate.

About half of the book is devoted to the metal worker sample, and that seems extravagant. Korpi introduces one new variable after another and they cover an extraordinarily broad range. In effect, the reader is overloaded with attitudinal dimensions and their precise theoretical relevance is not always clearly stated. A few carefully chosen relationships would, I believe, have convinced us equally effectively.

But the chief problem with metal worker attitudes is that they speak only indirectly to the critical question of class politics and power resources. The utility of Korpi's findings is somewhat limited when, in the final chapters, Korpi returns to these issues. Here the power position of the labor movement, the Social Democratic party in particular, is examined in light of its class support, the emergent alliance between manual and white-collar workers, and the programmatic shift towards economic democracy. The Social Democrats' traditional dependence on private economic growth is seen as conditioned by their lack of sufficient power. The adoption of the Meidner plan for economic democracy, however, attests to the radical shifts occurring in the Social Democrats' underlying power base. The hypothesis offered is that these shifts are of sufficient magnitude realistically to raise the demand for socialism on the political agenda.

The possession of a decisive degree of power rests on a number of key variables, one being the consciousness and solidarity present in the working class; secondly, the role of the manual workers' union federation—the LO; thirdly, the white-collar federation—the TCO; and finally, the Social Democratic party. The massive power of the LO, which represents about 90 percent of manual workers, is clear. It has made massive inroads into managerial prerogatives. Since the potential for economic democracy and democratic socialism is contingent on the TCO, we need to know the extent to which the white-collar workers will align themselves with LO workers and the Social Democratic party. Korpi argues that the TCO's position is converging with that of the LO and that its politics are coming closer to the Social Democratic party: this formidable coalition is already in the making. With regard to the Social Democratic party, we need to know its capacity for mobilizing an electoral majority.

The latter two factors are not convincingly argued. The party's election performance has been slipping since 1968, and it is presently in

opposition. Korpi's data indicate a substantial drift of working-class votes towards the Center party on the right, and they even suggest that the Social Democrats are losing their hold over younger-generation workers. Gains among white-collar workers are insufficient to offset losses among manual workers, and with a weakening grip on the young, the party's future appears problematic. The absence of a more extensive treatment of the TCO also hampers Korpi's argument. For instance, it would be helpful to know what rank and file white-collar workers think of economic democracy.

This is one of those books which disappoints at first reading, but later begins to creep up on you and demands that you seriously rethink old, comfortable ideas. It has flaws and loose ends which certainly have nothing to do with the author's scholarly qualities, but which attest to the inevitable gap which must occur when a challenging and ambitious theory confronts insufficient data. The long attitude survey may be tedious reading, but the study as a whole is intellectually exciting and provocative. There are not many books on this theme of equal stature. Also, it is easily the best treatment of Swedish class politics available in English or, for that matter, in Swedish. It demonstrates just how far Sweden—and the study of politics—has developed since Marquis Childs. Korpi may not exactly prove that the working class remains the "gravedigger of capitalism," but he does make a strong case for why we should study this issue in today's welfare state.

GÖSTA ESPING-ANDERSEN

Harvard University

**Eurocommunism: A New Kind of Communism?** By Annie Kriegel. (Stanford, Calif.: Hoover Institution, 1978. Pp. xiii + 131. \$12.00.)

Annie Kriegel wrote this short essay nearly two years ago in response to a valid question posed by many at that time: is Eurocommunism a new kind of communism? Does it represent a potential challenge to Soviet leadership of the communist world similar to the challenges of the Yugoslav and Chinese communists? To answer these questions, Kriegel first looks at three leading definitions of the Eurocommunist movement. To those who see it as *a new variety of communism that combines socialism and freedom*, Kriegel responds that for all the references to civil rights, bourgeois

freedoms, pluralism, renunciation of the dictatorship of the proletariat, etc., the Eurocommunist parties continue to practice a democratic centralism which badly undermines their new-found commitment to tolerance of contending ideas and the democratic process. The second possible definition of Eurocommunism, according to Kriegel, is that it is *a regional strategy for the conquest of state power*. Given its limitation to France, Spain and Italy, we probably ought then to call it SemiSouthern-Eurocommunism.

But more importantly, Kriegel points out that on major questions, these three parties are by no means unanimous. Thus she contrasts the timidity of PCF and PCI criticism of the Soviet Union with Carillo and the PCE's more aggressive stance. On foreign policy questions, such as NATO and European construction, she contrasts the views of the PCI and PCE versus those of the PCF. On domestic programs she groups the PCF and PCE versus the PCI. These shifting coalitions lead Kriegel to stress the third definition of Eurocommunism—*national communism*. Thus Eurocommunism represents a "temptation" on the part of independent Western European Communist parties to "escape from the ruins of Stalinism" (p. xi) in their own individual ways. Nevertheless, Kriegel finds the temptation hardly overwhelming and remains unimpressed with the Eurocommunists' efforts to distance themselves from their Stalinist pasts or from the Soviet Union.

Kriegel goes on to note that each of these definitions contains some truth, though always qualified. What then, she asks, are the obstacles to the full development of the "as yet mostly undifferentiated embryo of Eurocommunism?" (p. 54) First, Soviet hostility; second, the fact that the Eurocommunist label has not appreciably increased the voting strength of those parties; third, the lack of room for maneuver. That is, the Eurocommunists are not alone on the left—close by are the democratic socialists, the *gauchistes*, the Neo-Marxists, etc. So the Eurocommunists are in limbo, because "the alloy of verbal radicalism, practical reformism, and strategic nationalism, besides being likely to break down because of the inherent incompatibility of its components, is not capable of providing its own legitimacy" (p. 58).

There are three scenarios open to Eurocommunist development, according to Kriegel. Return to traditional orthodoxy, rupture with the Soviet Union, continuation of the present—confused—course. Kriegel concludes that this last scenario is the most likely one. After all, she argues, when compared to the communist

world's attention to Asia, Africa, and the Middle East, Eurocommunism is a peripheral, not a major, communist development.

Kriegel's book was one of the first to raise interesting questions and outline options with regard to Eurocommunism—yet that is precisely the problem. Written originally in French in the spring of 1977, published in English in the summer of 1978, the book is now outdated, superceded. Of what use, for example, is the final third of a book which speculates on what would happen once the united left in France won the March 1978 legislative elections? In fact, the PCF decision to sabotage that united left victory has shed new light on Eurocommunist development. If the Eurocommunists are really committed to the (democratic) emancipation of the European working class (their logical aim, according to Kriegel), why do they favor governments of national union with conservative or centrist elements, rather than union of the left governments? The French Communist party courts the Gaullists while vehemently criticizing the Socialists; the Italian Communist party seeks power with Christian Democrats rather than with Socialists; the Spanish Communist party speaks favorably of the center-right government in Spain. Is it possible the real communality of interests among Eurocommunists is now the forestalling, the undermining of the democratic socialists' growing "Europeanization" (cooperation at the EEC level, especially after the direct elections in June 1979) and "internationalization" (in a reinigorated Socialist International)?

NANCY LIEBER

*Paris*

**Women in Soviet Society: Equality, Development and Social Change.** By Gail Warshofsky Lapidus. (Berkeley: University of California Press, 1978. Pp. x + 381. \$17.50.)

Gail Lapidus' work on women in the USSR is a masterful, comprehensive, well-researched book which will be frequently cited by scholars and students of Soviet and women's studies for the foreseeable future. She demonstrates a thorough grasp of relevant Soviet and American literature, skillfully applying the conclusions and insights of other writers to her presentation of data and to her own analysis. Her knowledge of Russian is effectively reflected in the diversity of both well-known and little-known Soviet sources used. The book is enhanced by the

extensive list of sources in the bibliography and footnotes. The overall approach and coverage of the work also testify to her credentials in sociology as well as political science.

Among the major topics covered in this lengthy work are: women in pre-revolutionary Russia; a history of Soviet policies affecting the status and roles of women; overviews of women in the work force, in politics and in the family; an examination of sex roles; and finally, general conclusions. Most of the topics are treated in considerable detail, even though study of some issues is handicapped by the paucity of available Soviet data. The book purports to study the question of women more or less as a case study in the politics of equality, especially focusing on the impact which Soviet-style modernization has had on equality. The dual theme is somewhat lost in the development of the various topics, although the author returns to it in the concluding chapter. In so many areas of life Soviet women have clearly not achieved equality, but a sharper, more concerted pursuit of this dual theme in each major subdivision would have been possible.

In launching her study, Lapidus examines first the nineteenth-century women's movement in the West generally as well as in Russia. Perhaps more space could have been devoted to the movement in Russia and less to general nineteenth-century developments. Her treatment of Marx and the women's issue raises as many questions as it answers. While one can agree with her that the women's question was never a central one to Marx, her analysis of Marx's positions is sometimes unclear. Her treatment of the reforms adopted in the USSR in the 1920s and of the changing policies of the 1930s is especially noteworthy. The chapter, "Enabling Conditions: Affirmative Action, Soviet Style," presents an excellent overview of existing laws and of areas of strength and weakness in Soviet policy.

The work definitely belongs in every college and university library and in the personal collection of scholars interested in Soviet domestic policy. It makes a major contribution both to policy studies of women and of the USSR. The sophisticated treatment of the material and the author's presumption that the reader is equipped with considerable knowledge of the Soviet Union suggest that the work can best be used by the professional scholar or advanced student. This work is not intended for the casual lay reader.

Despite a preponderance of positive features which deserve recognition and commendation, the work has some weaknesses. Although Lapi-

dus writes very well, at times her points are ponderously made or lost in a maze of complex, slightly meandering arguments. In several places she suggests there may be important implications arising from a particular trend or policy, but fails to examine them. The lack of follow-through on some problems studied may be a by-product of such a far-reaching study, but does occasionally leave one frustrated. For example, on p. 143 Lapidus states that the fact that girls outnumber boys in secondary education may have significant implications for subsequent occupational roles but does not attempt to explore further. In the chapter on work, she points out that, from the 1940s to the 1960s, many women entered the field of engineering, a prestigious occupation in the USSR from which many promotions flow (pp. 182-83). Yet few women engineers are in top administrative or managerial positions. Is the "time lag," or late entry of women into this field, the single most important explanation, as suggested in the text? Again in the chapter on politics, she cites the "time lag" as a factor in the failure of women to reach the higher echelons of power in significant numbers. The Soviet system is now over 60 years old, and the commitment to women's rights was part of the revolutionary thrust. Yet, after six decades few women make it to the top in politics or industry. In at least two sections of the book, she alludes to the increasing "privatization" of life in the USSR but fails to give a thorough analysis of its possible impact on the status and roles of women.

In the penultimate chapter, Lapidus focuses on major problems affecting women which exist in the USSR today. Perhaps too much attention is devoted to population policy while relatively little is given to some other cogent issues. In the concluding chapter, the author attempts to put the question of sexual equality in comparative perspective. Coverage of this topic could be expanded considerably both here and elsewhere in the work.

On balance, the work is excellent and can be highly recommended to political scientists and other social scientists as a landmark study in the field.

NORMA C. NOONAN

Augsburg College

**The Korean Workers' Party: A Short History.**  
By Chong-Sik Lee. (Stanford, Calif.: Hoover Institution Press, 1978. Pp. xiv + 167. \$5.95, paper.)

This monograph is the first in the Hoover Institution Press series on the histories of the 16 ruling communist parties. Chong-Sik Lee is the author of the widely read *The Politics of Korean Nationalism* and the award-winning *Communism in Korea*, the latter written in collaboration with Robert A. Scalapino.

Casual students of Korean affairs and history would tend to equate the communist movement in Korea with the indefatigable Kim Il-sŏng and his Korean Workers' party, which is the official name for the North Korean communist party. Kim's successful elimination of his rival factions in the late 1950s and his highly propagandized doctrine of "Chuch'e," or "self-reliance" (not "the foundation of action," as this author would have it), have had the effect of encouraging that dubious equation. The establishment in more recent years of a veritable civil religion devoted to the adoration of the supreme leader and his supreme family would seem to suggest that this misconception has been carefully woven into the fabric of the North Korean belief system.

Lee's immensely knowledgeable account of the evolution of the Korean communist movement comes as a salutary corrective. It reminds the reader that there was a communist movement in Korea long before the victorious Soviet Army installed Kim as their man in 1945 and that it was, above all else, an extension of the nationalist movement whose principal objective was to liberate the country from Japanese colonial rule.

It is not surprising to learn that the early leadership of that movement was relatively uninterested in the ideological disputations which imbued the sister communist movements in the Soviet Union and China. Indeed, Lee lays major blame for the tribulations of the prewar Korean communist movement on the Comintern's wrong-headedness. Having affirmed, as it did in its 1928 "December Theses," the primacy of the anti-colonial political struggle over the class struggle aimed at an agrarian revolution, the Soviet leaders got carried away in the early thirties, so it would seem, with a mindless radicalism which demanded an all-out assault on every reformist group in Korea whose ideology and objective did not jibe with the Comintern orthodoxy, branding it as "reactionary." This posture stifled the anti-colonial struggle in Korea; but, further, it hopelessly undermined the cause of class struggle itself, which had yet to find a foothold in a society dominated by stubborn conservative traditions.

The author makes a catalogue of unfavorable circumstances under which the Korean commu-

nists had to operate prior to the liberation of 1945, and in so doing he seeks to explain their "repeated failures" (p. 29) at carrying out their twin objectives of an agrarian revolution and the overthrow of Japanese rule. He cites, in particular, the uncanny efficiency and ruthlessness of the Japanese police. But it is not clear what sort of developments the author would have required as evidence of the communists' "success." For, surely, he does not believe that the Japanese would have stood by while the Koreans carried out their revolution, much less pulled out under the pressure of a Korean "united front," however massive or determined such an effort might have been.

This reviewer's major reservation about the book, however, has to do with its treatment of the post-liberation period. Lee's analysis of the radical politics under the U.S. occupation forces and the outbreak of the war in 1950 betrays a curious lack of critical insight, at times making him appear downright naive. To be sure, it may be some time before we can synthesize our fragmentary knowledge of these subjects with any finality. Nonetheless, enough has been told in recent years to compel a serious reappraisal of much of the conventional wisdom which has been bequeathed to us as a cold war legacy. When it comes to these crucial topics, our author seems in no great hurry to revise his old views.

This book still remains a welcome addition to the literature, certain to please especially those who have an appetite for the subject matter but not the patience required for plowing through a full-blown study. Lee has given us both kinds of work: after one is finished with this volume one can always turn to Lee's larger works to sample his better efforts.

SUGWON KANG

*Hartwick College*

**The Politics of the Chinese Cultural Revolution: A Case Study.** By Hong Yung Lee. (Berkeley: University of California Press, 1978. Pp. xiii + 369. \$16.00.)

Lee is not the first writer to pursue an interest-group approach to China's Cultural Revolution of the 1960s, but his effort is the most thorough and well-researched one. His main source of data is Red Guard newspapers, as catalogued and indexed in *A Research Guide to Red Guard Materials in the United States* (Occasional Paper, University of Michigan Center for Chinese Studies, forthcoming). Such

documentary findings are soon to be augmented by the work of Stanley Rosen and others who have undertaken extensive interviewing of Cultural Revolution participants.

The interest-group approach has characteristic strengths and weaknesses. The strong side relates to the interior of the revolutionary process, that is, to the dynamics of the movement once it was under way. To quote Lee's own concise summary of his complex argument:

The basic component of Mao's strategy of mass mobilization was to exploit the existing social contradictions by shifting his support from one group to another, according to the groups' positions on a particular issue and according to the practical needs of the moment. This strategy enabled him to supplement his diffuse power with genuine support from particular political groups, and at the same time allowed him maneuverability in his actions. He relied on the masses to criticize the elite and to break the coalition of reds and experts within the elite. But, at the same time, he continued to recognize the need for leadership from the top and actually used the organizational capability of the PLA to maintain control from the top. He mobilized the radicals to attack the power holders and to compel the privileged social groups to renounce their privileges. On the other hand, he depended on the conservative mass organizations to put pressure on the bourgeois experts and to check those with bad family backgrounds from directly challenging the Marxist class line (p. 330).

The weak side of the interest-group approach is its tendency to slight both the origins and the ultimate consequences of revolution. It is a way to make some sense of people's motivations and strategies only in the short run.

The most well-documented parts of Lee's study are hypotheses about group participation in the Cultural Revolution, such as: "The radical mass organizations were largely composed of underprivileged social groups, whereas the conservatives were heavily drawn from the better-off social groups" (p. 5). The seven "actors" to whom he gives the most explicit attention are Chairman Mao, the Cultural Revolution Small Group, the People's Liberation Army, the party organization, the government, radical mass organizations, and conservative mass organizations. These are admittedly very large aggregates, but Lee is able to draw several meaningful conclusions about them nonetheless. Hypotheses about elite-mass relations and about authority fill out his interpretation of this period. Generally, however, these are less carefully developed. Lee's points about groups



are his original and important empirical contribution.

GORDON BENNETT

*University of Texas, Austin*

**Iran Under the Pahlavis.** Edited by George Lenczowski. (Stanford, Calif.: Hoover Institution Press, 1978. Pp. xxii + 550. \$19.95.)

Along with many other close observers of the Iranian scene, the 12 authors of these collected essays completely failed to foresee the turmoil which engulfed Iran in 1978 and led to the downfall of the Shah. On that score alone, the authors are blameless. But all too often these essays are apologia which fail to offer even the slightest analysis of the forces which so violently erupted. With few exceptions, these pieces constitute a celebration of the Pahlavis.

Only Charles Issawi in his "The Iranian Economy 1925-1975: Fifty Years of Economic Development" warns that "In the mid-1970's Iran's growth was definitely excessive, and was straining the social fabric and administrative capacity" (p. 163). He faults the Iranian economy for failing to provide sufficient employment opportunities, failing to check inflation, and warning that Iran's growing inequality of income was "a deplorable and socially dangerous trend" (p. 165).

But with almost no other exception in over 500 pages, one would be hard pressed to recognize the same Iran whose monarch left for an "extended vacation," whose economy is in a shambles, whose population reveres a long-exiled religious leader, whose army collapsed under the immense strain of months of martial law and brief fighting, and whose formerly feared secret police remains in total disarray.

What the book demonstrates is a form of what might be labeled "Pahlavi historiography"—a focus on a very select set of aggregate statistics about Iran as an entire system or about Iran's international relations. Missing is the politics—whether it be of literature or land reform; of religion or education, of social change or planning. Who did what to whom with what effect can rarely be discovered in these pages.

In the emergence of Iran from under the Pahlavis, it seems appropriate to ask how Western scholarship became bewitched by the Pahlavis as did Western journalists and most Western governments. This is the appropriate moment for scholars to apply their skills at critical inquiry upon themselves.

Also ripe for critical reassessment is the 1951-1953 period during which Dr. Mohammad Mossadegh served as Prime Minister. Virtually no research on that period has been possible until now. Given that certain of the key figures are still living, a rare period of immense scholarly potential is at hand. This new assessment seems especially appropriate because the essays here reflect a not uncommon view, often propounded by the Shah, that Mossadegh pursued essentially negative policies. Once he had nationalized Iran's oil, he had spent his only card. Rather than facilitating progressive social change, he impeded it. As Roger M. Savory puts the case in his "Social Development in Iran During the Pahlavi Era," Mossadegh "possessed the power to effect a social revolution but seemed devoid of ideas. . ." (p. 102).

Mossadegh's significance as a symbol of Iranian nationalism is ignored in this volume, but is clearly elsewhere appreciated. We can hope that a critical analysis of his government will be forthcoming which will not match the excesses of "Pahlavi historiography."

Yet another problem with these essays is their failure to use Persian language sources. Peter Chelkowski's essay on "The Literary Genres in Modern Iran" is a careful survey of much modern Persian literature. But with that exception there is scant reference to indigenous scholarship, much less Persian language materials. The scholarship of the post-Pahlavi era will clearly need to rely far more heavily on materials in the Persian language.

While these essays do not address the most salient contemporary issues, certain of them contain fascinating insights or oversights. Pio Filippani-Ronconi in "The Tradition of Sacred Kingship in Iran" argues that in Zoroastrian doctrines, "Kingship and Priesthood are considered to be the two fundamental poles of human society" (p. 56). The thesis-antithesis relation of the Shah and Ayatollah Khomeini during this year of struggle seems a contemporary manifestation of ancient doctrine.

One might wonder how in all these pages, including a specific essay by D. R. Denman, "Land Reforms of Shah and People," the name of Hassan Arsanjani can be missing. The former Minister of Agriculture was the key actor in the initial stages of the land reform in the early 1960s, when land reform was at its most decisive. Yet he is not remembered here.

Three sets of problems which confront any new government in Iran—certainly not peculiar to the Pahlavis—emerge in these pages. It is clear from these essays that once the land

reform began, the government felt it had the right continually to alter patterns of rural ownership. The result has been disarray in rural society and agriculture and increased migration to the cities. Despite the turmoil—and the disaster of Iran's attempts to create massive agro-businesses which is ignored in this volume—agricultural production has been annually increasing at roughly 3.2 percent.

But with the burst in oil revenues, urban demand for foodstuffs increased to the point where in 1974/1975, Iran was spending \$1.5 billion to import food—more than its entire oil revenues had been in 1970/1971. Iran's agricultural and food systems cry out for attention.

A second long-range problem is noted by Savory who observes that "social opposition to the concept of limiting family size has been tremendous" (p. 117) while Issawi projects a total population for Iran of 60 million by the end of this century. But Issawi repeats the misleading notion that "even with 60 million inhabitants, Iran would have a lower density of population than most countries" (p. 139). This is undoubtedly true if the calculation is made on the basis of Iran's total land area, but given the small percentage of arable land, such a large population would be a great economic burden.

A final issue which any new government in Iran must confront is the role of foreigners. In his excellent essay on "Iran's Foreign Policy," William E. Griffith specifies the "intimate" relation between Iran's domestic and foreign policies. The Qajars fell and Reza Shah came to power, he suggests, at least partly, "because of the humiliating penetration of the Iranian polity by foreign powers" (p. 369). Other essays make clear Reza Shah's refusal to accept foreign loans or allow sizeable numbers of experts from major foreign powers to participate in Iran's development.

His son took a different route. The extent to which the Shah's downfall was brought about by his close association with the United States, with 50,000 American experts in Iran, and with the "Americanization" of the Iranian military, is yet to be analyzed.

MARVIN ZONIS

*University of Chicago*

Our understanding of the evolution of modern party systems has improved considerably in the last two decades with the appearance of a number of insightful works: Lipset and Rokkan, *Party Systems and Voter Alignments*; La Palombara and Weiner, *Political Parties and Political Development*; and Duverger's *Party Politics and Pressure Groups*, among others.

Now comes *The Emergence of the Australian Party System*, and it is, regrettably, as though the earlier works had never appeared. Loveday and his colleagues have produced a lengthy, well-researched study of the development of Australian state and federal parties between the 1880s and 1910s, but the study adds little to our understanding of the evolution of parties, either in Australia or more generally.

The major problem with the book is that the authors reject any interpretation in terms of general themes. The avoidance of interpretation is so consistent across the book's chapters that one almost has a sense of conspiratorial determination among the authors. Thus, in the chapter on Queensland, Joyce comments, "No single cause seems adequate to explain the changes in the Queensland political system between 1890 and 1910" (p. 168); and in the discussion of party evolution in New South Wales, Loveday, Martin, and Weller suggest that "it would be a mistake to search for overarching explanations of a socio-economic kind" (p. 244); and, once more, Jaensch argues that "No single factor provides an adequate explanation for the development of the South Australian party system. . ." (p. 296). (The rejection of interpretation is probably facilitated by this recurrent identification of interpretation with single-cause explanations.)

This problem applies even to the best of the book's chapters, a concluding chapter by Loveday on "Emergence: Realignment and Consolidation." Loveday admits to being "tempted" toward general explanations by the many similarities between party system evolution in Australia and elsewhere, but he concludes that Australia is unique because "the problem in the Australian context is to explain the transformation of non-party to party politics, not the conversion of middle class to mass parties" (p. 460). This argument can be faulted on two counts. First, part of the problem is to explain the evolution of mass parties for, as is illustrated in almost all of the book's chapters, one aspect of Australian party evolution was the development of mass, class-based labor parties. Second, Australia is hardly unique in having experienced a transformation from non-party

#### **The Emergence of the Australian Party System.**

Edited by P. Loveday, A. W. Martin, and R. S. Parker. (Melbourne: Hale and Iremonger, 1977. Pp. xviii + 358. \$19.95, cloth; \$9.95, paper.)

to party politics in this period. Many countries, including such a seemingly non-comparable country as France, experienced similar transformations during much the same period, often concomitantly with the development of mass parties.

Nonetheless, this concluding chapter remains interesting, and in large part because Loveday briefly forgets his antagonism toward explanation and offers an interesting analysis of party evolution in Australia (pp. 480-87). The analysis draws on several theories, and makes this the one chapter worth reading by students of party development.

The interpretive failings of the rest of the book are especially unfortunate because the book reflects extensive and painstaking research. That research has produced impressive detail on the chronologies of party and legislative behavior during the early years of Australian parties. Other students of Australian parties may find this detail useful in developing the explanations which these authors have for the most part disdained.

JOHN CLAYTON THOMAS

*Texas Christian University*

. . . and the Desert Shall Rejoice: Conflict, Growth and Justice in Arid Environments. By Arthur Maass and Raymond L. Anderson. (Cambridge, Mass.: MIT Press, 1978. Pp. 447. \$19.95.)

Irrigation institutions, like most political institutions, exist for the purpose of resource allocation, conflict resolution, and to defend that ephemeral quality—justice. If they are to be successful at all, institutions must be adapted to the technological conditions within which they must function. These conditions—principally the flow nature of the resource and its unpredictability—have dictated distinctive norms of behavior and attitudes. It is to the distinctive character of these institutions that this book is addressed, using for its raw material case studies of irrigation practices in six communities: three in Spain and three in the United States: northern Colorado, central Utah, and the Kings River Service area in California.

As the authors put it, this is a “parochial communities” view of institutions as distinguished from the view usually expressed by students of political development and modernization. The latter tend to focus on central institutions and their capacity to encompass traditional or parochial communities within

their control. This “bottom-up” view reveals that rather than being brought within the control of centralizers of the modern nation-state, irrigation communities have been remarkably resistant and independent. The authors’ central conclusion is that Wittfogel was simply wrong: irrigation agriculture does not tend toward centralization of political power, in part because no government is so despotic as to shut off the tap once the supply and its delivery system have been provided, and in part because the irrigators themselves have contrived institutions that reflect their consensus about water allocations and the procedures by which those allocations should be made.

The major portion of the book—355 of its 400 pages—are devoted to a very detailed accounting of the development of irrigation practices in these six locations. Nearly one-third of the book alone is dedicated to the Kings River service area. In each case there is a discussion of the physical conditions, historical development, the governing institutions, the procedures and rules for allocating water both in times of abundance and shortage, and finally a discussion of community objectives in terms of the norms of equality, equity, efficiency, conflict resolution and popular participation (although this systematic discussion of community objectives is strangely lacking from the Kings River case).

There are significant differences in the practices of these irrigation communities in terms of standards for allocating water, the methods of organizing and decision making, and procedures for adjudicating disputes when they arise. But all reflect a community consensus about what is fair when shortages occur. Moreover, all reflect an awareness of the need for consensus and effective operating procedures in order to retain the capacity to deal effectively with central institutions. As the need for water expanded, and it was realized that such needs could be fulfilled only by the higher levels of government, the local institutions enhanced their bargaining power by the strength that they displayed.

The central thesis of the authors is especially relevant to U.S. students of water policy as the Carter administration endeavors to impose new standards on water development projects and to adopt water conservation as a major program to ensure adequacy of water supplies. A reasonable lesson derived from this book is that irrigation farmers may be induced to adopt water conservation measures if the federals will simply pay for it; if the federals try to impose constraints on irrigationist behavior, as EPA did

in the application of the NPDES system, they simply will not work because of the massive resistance of irrigation interests.

This point leads to an observation that curiously escapes the authors of the book: non-irrigation interests in the United States are seldom hostile to irrigation agriculture. Despite the intellectual assaults of economists and the competition for scarce funds from urban interests, irrigation agriculture, at least in the United States, has done very well politically. Perhaps this reflects our agrarian heritage, perhaps the magic associated with mastering nature, especially through making the desert blossom. In the western states themselves, where competition for water is keenest, few want water to be taken from irrigation agriculture. Far from being hostile and centralizing, federal institutions as typified by the Bureau of Reclamation, are decentralizing, accommodating and submissive to the local water interests.

On the other hand, federal interests *are* bureaucratic and determined to protect themselves. It is one thing to demonstrate, as the authors do, that central institutions cannot compel behavior of irrigationists once the projects are built, but it is another to demonstrate that these bureaucratic agencies may not wield substantial influence when the local interests want something federals are loathe to give. Having adopted operational criteria, they are likely to defend them despite reasonable argument or potential injury. And in crisis situations, as in the western drought, the bureau was quite capable of reducing agricultural deliveries in California by 75 percent, while reducing municipal and industrial deliveries by 50 percent.

In the concluding chapter, the authors consider the meaning of irrigation agricultural practices for economic growth, equity and equality. Having utilized simulations of the procedures used in each community to estimate the benefits of each approach, they then compare the various procedures in terms of their economic efficiency. They conclude that a demand plan procedure and a market structure for buying and selling water rights are not only most efficient but also provide greatest equality as measured by several standards such as the percentage loss in income per acre. Such procedures and structures are more often than not rejected, however, because of fear of instability and loss of local control. The authors also observe that land speculation, whatever the distribution of profits and losses in the development process, actually was more likely to contribute to stable communities of smaller

farms than was cooperative development.

Despite these observations, this is a curiously non-economic book. If one thinks of the charges of corporate interests gaining dominance in the Central Valley of California and the existence of districts in which a few large interests predominate, one finds little discussion of such allegations and/or facts. Perhaps if the California example had been the Westlands Water District, the picture might have been somewhat different. The authors do observe, however, that irrigation farmers are prepared to accept rules that guarantee higher levels of inequality because the rules promote stability and local control and because farmers may even subscribe to the ethic of hard work as the basis for economic success.

In sum, this is an important book. The research effort was prodigious, the exposition lucid (albeit so detailed that the reader has difficulty in digesting it all), and the results significant. The authors themselves raise the question as to whether other local institutions are equal in their capacity to resist external influence and to maintain their integrity. Certainly that question should be addressed by others in other contexts.

DEAN E. MANN

*University of California, Santa Barbara*

**Power and Class in Africa: An Introduction to Change and Conflict in African Politics.** By Irving Leonard Markovitz. (Englewood Cliffs, N.J.: Prentice-Hall, 1977. Pp. xii + 398. \$12.95, cloth; \$8.95, paper.)

Irving Leonard Markovitz's *Power and Class in Africa* is a work of impressive scholarship. Encyclopedic in the wealth of material it presents, this book will be consulted as a useful source of reference by both specialists and non-specialists. Put simply, Markovitz has attempted to synthesize and review all of the significant literature on the political economies of independent African states. That he has succeeded is alone worth the price of this book.

*Power and Class in Africa*, however, is fundamentally a textbook, and it is on this ground that it must ultimately be judged. But by what standard? Any attempt to arrive at a single set of criteria by which to evaluate and choose a text is as much a reflection of what questions a given instructor regards as important for students to understand as it is an evaluation of the author's analytical skills. At the risk of being somewhat presumptuous, I list

my criteria as basically four: (1) The text should explore the *reciprocal* relationships between the basic political and economic factors that constitute the structure and dynamics of contemporary African societies. (2) The text should present a historical (though not necessarily a dialectical) analysis of the emergence and evolution of these factors from the pre-colonial period, through colonialism, and to the present. (3) The text should consider the alternative ideological and policy options available to political leaders of independent African states, their impact, and their attendant costs. (4) The book should be exciting for undergraduates to read.

Against these criteria, particularly the first and the second, *Power and Class in Africa* is more rewarding than virtually all of the few alternative texts now on the market. Markovitz is on particularly solid ground when discussing the process of class formation, and its significance for the distribution of political power. This is the central theme of his book, and it is not surprising that he develops it well. His discussion of the historical developments which led to the emergence of the class structures of contemporary African states, particularly in the late nineteenth and early twentieth centuries, is especially detailed and rich in basic information.

*Power and Class in Africa*, however, does not fare as well in respect to the third and fourth criteria. In my opinion, a delineation and explanation of the distribution of power is only half the story. One must also ask the question, "power for what?" Although Markovitz states on several occasions that he is concerned with the basic questions of who gets what, when and how, and although he devotes the final third of his book to a discussion of how those with power have consolidated their positions, the reader does not come away with an appreciation of the conflicting political forces which shape the policies of contemporary African states. It is one thing, for example, to discuss the creation and continuity of an "organizational bourgeoisie," as Markovitz has done superbly in chapter 10. It is quite another to discuss the internal conflicts within this class, and especially its relationships, both harmonious and competitive vis-à-vis other centers of power, i.e., the nascent national bourgeoisie, the rural power elite, the peasantry, ethnic blocs, multinational corporations and foreign powers. Although Markovitz considers the development of several of these on a one-by-one basis in exhaustive detail, his discussion of the relationships between them, especially the sub-

stance of the competing policies they demand, is limited, with the result that he presents an incomplete description of the stuff of which African politics is made.

It is not enough, for example, to lament, as Markovitz periodically does, about the extent of inequality on the continent. He must also consider what the likely impact, tradeoffs, and reaction will be to policies designed to ameliorate this basic problem. The leadership of a country, and I will take Tanzania as a case in point, may enact a progressive income tax and other restrictions to reduce the amount of wealth accruing to the "organizational bourgeoisie" relative to the rest of society, with the unexpected result that many members of this class become corrupt and neglect their official duties. Or, a government might pursue a policy of paying low prices for agricultural produce, as the Tanzanian government also did prior to 1975, to reduce the cost of food to urban workers and the amount of income flowing to rich farmers. In this case production fell, and the smuggling of produce out of the country became rampant.

Markovitz's insufficient consideration of the policy alternatives available to African states mars an otherwise sound presentation. This omission, however, when combined with the book's exhaustive detail, results in a volume that is unlikely to excite the average undergraduate. The greatest strength of *Power and Class in Africa*, its richness, is also its primary weakness as a text. Most undergraduates will be unable to digest the huge quantity of data Markovitz expects them to consume. Superb scholarship and a flair for presenting basic material in a crisp yet comprehensive manner do not always go hand in hand. Such a balance will have to await an effort by another, perhaps less ambitious scholar than Markovitz. Until then, *Power and Class in Africa* will partially fill a serious gap in the literature.

JOEL D. BARKAN

C.E.R.I., Fondation Nationale des Sciences  
Politiques and University of Iowa

Classe, religion et comportement politique. By  
Guy Michelat and Michel Simon. (Paris:  
Presses de la Fondation nationale des sciences  
politiques, 1977. Pp. xii + 498.  
FF110, paper.)

This is a very fine intellectual construction, complex in design, subtle in detail, and of such methodological scrupulousness that the reader

is often required to consider the authors' own question—is our procedure at this stage defensible? What the design calls for is nothing less than a structure resting on two pillars, qualitative and quantitative. The former is built up from 77 interviews originally conceived as part of a research program, mounted in 1966, into perceptions of the political field, especially the structuring of political attitudes. The interviews were non-directive in the hope that the inquiry, not the inquirers, would give definition to the field. Guy Michelat and Michel Simon then set out to verify what the interviews revealed. This, the quantitative part, takes up most of the book: it is so rigorous that it gives the general impression that Descartes might have written it had he done graduate work under Lazarsfeld. That is not to say that it is unreadable, only that it is demanding. Fortunately, the authors proceed step by step, with frequent recapitulations, so that anyone can follow who has patience and a sense of adventure (as well of course as a reading knowledge of French).

The qualitative research revealed two different structurings of the political field and more broadly two visions of the world characteristic of two groups of individuals typed by the authors as avowed Catholics and irreligious communists. To these two groups belonged different patterns of attitudes, norms and values, one centered on the Catholic religion, the family, and so forth, the other on feelings of class identification. That sets the scene for a quantitative assessment of the reciprocal relationships between class and religion as tending, although only in a probabilistic sense, to govern political behavior. Other variables get some attention (sex, type of habitat, level of education, etc.), but are not deemed central. Political behavior always means voting behavior, which is always verbally manifested (i.e., intentions, elicited by questionnaire), but the authors believe that the deeds match the words (p. 150).

Class is resolved into its objective and subjective components, the former given what may be called a quasi-occupational meaning ("quasi" because *socio-professionnelle* is not identical with social class, only—it would seem—the least inadequate indicator of it, p. 139, n. 7). Objective class is found to influence the vote via subjective class, but continues to exert its own independent influence, the final result being cumulative. Thus the probability of voting left, especially communist, was at its maximum .60 (percent) among workers who were the children of workers and who perceived themselves as working-class. (However, 26 per-

cent would vote gaullist or centrist, prompting the authors to remind us that the interaction takes place within a sociohistorical context in which people are exposed to signals from many sources, p. 257.)

And so to religion—meaning Catholicism, since the number of "other confessions" caught in the research net was too small to keep and make use of. The authors first reexamine the venerable ecological relationship between the high proportion of conservative votes in regions of regular religious practice, the Left being strong where such practice is weak. From a reanalysis of public opinion polls covering the first round of voting in the presidential elections of 1965 and 1974, and the legislative elections of 1966 and 1973, Michelat and Simon conclude that the relationship still holds. However, some interesting tendencies were observed, e.g., in the legislative elections, among those without religion, support for the Communist party fell but support for the non-communist Left and the Center rose. In the cantonal elections of 1976, the socialist Left may have gained ground even among the most faithful churchgoers (p. 376, n. 52).

The authors' own findings are congruent with the traditional view, but they proceed to acknowledge that the ecological inverse does not hold: there are departments of weak religious practice that exhibit conservative tendencies. Turning a gun on themselves, they even speculate whether the apparently verified proposition is not susceptible of another interpretation—that it is not Catholicism as such but *a* specific Catholicism that happens, for historical reasons, to be associated with political conservatism, so that what is at work is a contextual variable operating in certain regions (p. 407). Their final judgment, however, is that the expected relationship does hold (pp. 413 and 446).

Finally, religion and class, objective and subjective, having been put asunder, are conjoined the better to explain political behavior. One finding among many is that the effects are cumulative, e.g., it is among workers objectively and subjectively who have "lapsed" or are without religion that the left vote (communist and non-communist) is at its maximum (73 percent), and the right vote (gaullist and centrist) at its lowest (16 percent). Here again, it is claimed, the findings are congruent with the expectations derived from the qualitative part of the research. Thus it is the action and interaction of the three variables that accounts, probabilistically, for voting behavior and possibly other forms of political behavior. Can one

say which variable contributes most? For what it is worth, a tree analysis points to religion, followed by subjective social class, with objective social class contributing little.

On that (for me) rather dubious note, the book ends. All in all, a very fine piece of work, perhaps more to be admired than enjoyed, but certainly to be read by specialists in French politics but also by political scientists in a broader sense and by the sociologists of politics.

GRAHAM WOOTTON

Tufts University

**West Bank/East Bank: The Palestinians in Jordan, 1949–1967.** By Shaul Mishal. (New Haven, Conn.: Yale University Press, 1978. Pp. xiii + 129. \$9.50.)

Shaul Mishal, of Tel-Aviv University, uses Hebrew-language publications as well as material from Jordanian archives captured by the Israelis in 1967. Unfortunately, *West Bank/East Bank* promises more than it delivers and is essentially a long essay with a definite and limited objective. It is not, as the subtitle claims, about Palestinians in Jordan. It is not entirely about Palestinians on the West Bank since it ignores those Palestinians in refugee camps, for instance. Neither does it cover fully or adequately the political (let alone the social, economic or cultural) dynamics of Palestinian life on the West Bank. According to Mishal, "Since 1967 the Palestinian issue has been a major and complex element pervading world politics." Mishal's objective, therefore, is "to achieve a sense of balance" in order for us not to be overwhelmed by this resurgence of Palestinian identity. In other words, instead of a Palestinian entity or state on the West Bank (and Gaza), he wants to explore the idea of continuing foreign rule over the West Bank the way Jordan ruled from 1949–1967.

With this objective firmly in mind, Mishal proceeds to explore the relationship between the Hashemite regime and the Palestinian political and ideological leadership. As a result, while much that is presented is factual, the end result is rather inaccurate and misleading. In part, this is because the author is selective in his sources. Information from the Jordanian Security Services archives adds little of value and often merely corroborates material from other available sources. Also, the Hebrew-language sources are relatively few in number and, in any case, are mostly secondary sources, chiefly ones

providing Israeli interpretations of attitudes and behavior of Palestinians on the West Bank. In fact, almost one-fourth to one-third of all such citations come from one source whose English title is "The Political Parties in the West Bank Under the Hashemite Regime," edited by Amnon Cohen and mimeographed in 1972.

In Mishal's account the Palestinians never emerge as a living, thriving community. One cannot help feeling that this is intentional. Hence the use of "Palestinian primordialism" to refer to Palestinian nationalist aspirations, for instance. Also, Mishal claims that the Palestinians on the West Bank, having been prevented from realizing their desires, i.e., Palestinian nationalism and statehood, adopted what the author terms "floating identity." Thus they allegedly had Arab, Muslim, Jordanian and Palestinian identities simultaneously and tended to display at any one time the identity that caused them least harm or brought them most good. Somehow this is presented as if it were different from contextual or situational identification. In any case, to "prove" his point, the author presents the views of political parties on the West Bank such as Muslim Brotherhood and the Ba'th and how they worked for ultimate political salvation through a Muslim or an Arab state. What this has to do with "floating identity" is not clear, since it surely was not the case that Palestinian followers of the Muslim Brotherhood were also and at the same time Arab Nationalists, Jordanian nationalists and Palestinian primordialists (i.e., nationalists).

Two obvious mistakes should have been caught by the readers of the manuscript. On p. 105, last paragraph, two references to the "party system" should read "election system." More disturbing is the statement: "The Baghdad Pact, one of the first manifestations of an integrative concept of Arab unity, came into being in late February 1955, when Iraq signed a military agreement with Turkey and Pakistan" (pp. 53–54). Needless to say, Iraq was the only Arab country to join the pact which was generally viewed as a measure to combat Arab nationalism and unity.

In sum, the book suffers from the fact that Mishal begins with a thesis which he sets out to "prove" and, in the process, chooses his sources selectively, arranges the data in a manner suitable for his purpose, and resorts to expressions such as "Palestinian primordialism" and "floating identity." How inadequate this procedure is may be better illustrated through a hypothetical parallel history of Zionism written in this vein. Such a history would discuss "Jewish primordialism" and refer to a "floating

identity" as illustrated by the simultaneous Jewish attachment to Russia (or Poland, France, the U.S., etc.), pan-Jewish movements, secular Zionist organizations, and communist or socialist programs. Of course, such a history would also teach us (or should have taught us) how to avoid establishing a Jewish state!

MICHAEL W. SULEIMAN

*Kansas State University*

**The Politics of Industrial Relations: The Origins, Life and Death of the 1971 Industrial Relations Act.** By Michael Moran. (Atlantic Highlands: Humanities Press, 1978. Pp. 195. \$16.00.)

Michael Moran's excellent study addresses a key problem plaguing the British polity: how to reconcile the new system of functional representation with the traditional parliamentary system of government. His work addresses this theoretical issue by exploring the development and ultimate failure of the notorious Industrial Relations Act of 1971.

The Industrial Relations Act was the Conservative party's strategic response to its governments' successive battles with trade unionism, as well as to that moment's chaos in industrial relations. Conservatives sought to bring the union movement within a framework of laws for the first time. Their specific purpose was to restore discipline in Britain's strike-ravaged industrial relations and thereby reduce the disruptive influence of unionism on economic policy. Such intransigence in several forms had paralyzed the policies of both Labour and Conservative governments since the end of the Second World War.

In this book, Moran explores the sorry postwar relationship between the unions, the Conservatives, and their several governments. He describes the evolution of Conservative thinking on the reform of industrial relations and examines the struggle between the Trades Union Congresses, as spokesgroup for trade unionism, and the Heath Government over the passage and then the implementation of the Industrial Relations Act.

Moran concludes, rightly, in my opinion, that the troubles suffered by the Industrial Relations Act express the wider problem in the system of government itself. That problem is the challenge of the functional system of representation to the existing parliamentary system. The defeat of the Industrial Relations Act demonstrated this threat. The act failed

because the functional representatives—both the unions and the employers—refused to accept it.

Moran goes on to insist that some means of accommodation between the two systems must be found. The Wilson-Callaghan Labour Government explicitly recognized this in its conduct of the social contract which brought the union movement into the closest consultation at the very center of decision making. While that arrangement has been useful, it is hardly a permanent answer nor one which would be as available to the Conservatives. Moran explores several other schemes including the often-suggested reform of making the House of Lords into a functional body—which he likes best—but in the end admits that he has found no perfect solution.

Michael Moran's book is first-rate. He has produced a thoroughly researched and thoughtful piece of work. His writing style is interesting and the book is well organized. Most importantly, I find his analysis of the problem at the root of the struggle over the Industrial Relations Act quite convincing. Too bad he fails to provide a more imaginative "way out" for Britain, but no one else has produced that bit of magic either.

GERALD A. DORFMAN

*Iowa State University*

**Les Guerres Grises: Résistance et Révoltes en Angola (1845–1941).** By René Pélissier. (Orgeval, France: Pélissier, 1977. Pp. 630.)

**La Colonie du Minotaure: Nationalismes et Révoltes en Angola (1926–1961).** By René Pélissier. (Orgeval, France: Pélissier, 1978. Pp. 727. FF 280 for both volumes.)

These companion volumes represent the culmination of a decade of research on Angola by a French scholar. In original form the material herein constituted a doctoral dissertation; in the present volumes it represents a political history of Angola, richly documented and illustrated with maps, charts, and tables. Although Pélissier was denied access to archives in Lisbon prior to 1974, he has relied on a synthesis of secondary sources. He focuses on the unceasing African resistance to Portuguese rule in Angola. Whereas Portuguese officials and other observers have described aspects of this resistance, in no single work has a broad sweep of this history yet appeared in such detail. Thus these volumes guide the reader toward the task



of reconstructing an African history of Angola during the Portuguese colonial period.

In the first of these volumes Pélissier sets forth the proposition that the struggle for Angola essentially dates from the middle of the nineteenth century; evidence that the Portuguese never fully controlled the colony is drawn from the more than 180 military campaigns they launched between 1848 and 1926. Pélissier describes the different phases of this period of conquest. Nineteen chapters look closely at events and developments in each of the principal regions of Angola. African uprisings, Portuguese military operations, and warfare are reviewed in detail. Pélissier shows that between 1879 and 1926 the Portuguese were at war nearly half the time and were forced to commit more than 30,000 troops to their cause. In addition to tracing these battles, Pélissier delineates the causes of the resistance and revolts by examining the impact of colonial administration, the commercial penetration, as well as taxation and slavery. The exploitation of Portuguese civilians and military is exposed.

The second of these volumes covers the period from the end of the conquest period in 1926 to the outbreak of African warfare in 1961. A first part describes the social and political conditions as of 1961, covering population data, the administrative system, nationalism, religious forces, and white separatists forces. Ethnic groups are related to the emerging national liberation movements of the time. A second part of the book deals with three revolts in 1961, including the urban uprising in Luanda during February, the revolt in Baixa de Cassange during February and March, and the insurgency in northwestern Angola in March. Considerable attention is devoted to the last of these events, in particular to describing the rise and decline of the Bakongo resistance and war. The successes and failures of that momentous year marked the beginnings of an exhaustive war of liberation culminating in independence many years later.

These volumes are invaluable for scholars who wish to delve into the African struggle for Angola. They serve as a framework for future study and research and should prove especially useful to political scientists interested in southern Africa.

RONALD H. CHILCOTE

*University of California, Riverside*

#### **Ireland at the Polls: The Dail Elections of 1977.**

Edited by Howard R. Penniman. (Washington, D.C.: American Enterprise Institute, 1978. Pp. 199. \$4.75, paper.)

This collection of essays by several of Ireland's most noted political scientists serves a number of useful purposes. For the general reader it provides a highly readable introduction to electoral politics in Ireland, and the first authoritative account of the background, conduct, and results of an Irish general election. For students of Irish politics there is an additional bonus: the record of the highly unusual, if not "critical" election of 1977.

Near the top of the list of things noteworthy about this election were its many surprises: "an unexpected change of Government, the highest vote for a single party and biggest parliamentary majority in the history of the state, the precipitate resignation of two national party leaders, the defeat at the polls of three cabinet ministers, and the embarrassment of virtually the entire corps of observers and commentators, academic and journalistic, all of whom had failed to predict the outcome" (p. 133).

Lurking behind this intriguing tale of the unexpected seems to be another message: the coming of age of Irish elections by way of more modern content and technique, and the almost wilful failure of the "inside dopesters" to comprehend the meaning of these changes. Elections in Ireland have traditionally been characterized by their combination of ideological rhetoric and highly personalized and parochial concerns. The days and heroes of the Irish "Troubles" enjoyed an afterlife celebrated seasonally at campaign times, a celebration which added to, but never disguised the deeply idiosyncratic flavor of local constituency contests.

These "traditional" features of Irish elections have been slowly receding, a transition spotlighted by the events of 1977. The Ulster issue was pushed to the sidelines. The dominant items on the voters' minds—as measured by the polls—were not parochial concerns but economic issues such as unemployment and inflation. Techniques were changing as well. Tried-and-true methods of Irish electioneering, such as the gerrymander (in this election renamed the Tullymanner after the minister whose artwork shaped the new constituency boundaries) failed those in power. By contrast, the opposition party (Fianna Fail) sported several technical departures; "the early planning and sophisticated professionalism of the promotion campaign; the identification of crucial issues

and segments of the electorate; and the use of market research to monitor and correct the campaign" (p. 119). Perhaps one of the best symbols of the changing times was the shift from the wall poster to the tee-shirt as a campaign gimmick.

To suggest that Fianna Fail's victory was that of a more modern campaign over a traditional one would be, perhaps, an overstatement. But the vast proportions of their victory, which produced the biggest turnover of deputies at a single election since 1927, suggests that this was much more than the perennial desire of electorates to "turn the rascals out." How much more, and why all this was happening in 1977 does not really emerge from these essays.

The Irish, frequently for good reason, have been skeptical of "scientific" studies of their own highly insular politics. But this valuable collection of essays, several of which are in the "scientific" mode, tells a tale which is likely to erode such skepticism in the future.

PAUL M. SACKS

*University of California, Los Angeles*

**Civil War in China: The Political Struggle 1945-1949.** By Suzanne Pepper. (Berkeley: University of California Press, 1978. Pp. xxi + 472. \$18.50.)

One of the more enduring pastimes and occupations of Westerners during the momentous latter half of the twentieth century has been to observe and to interpret events in the non-Western world. The record of accuracy in observation or in interpretation is not an enviable one. No part of this assorted group of observers has been more devoted or assiduous, or had a more fascinating subject than those known as the China Watchers; and their record of failure compares favorably with that of those who have watched in other directions.

There are many reasons for this frustrating experience, but certainly one of the most important is that Westerners, and perhaps especially Americans who make up a large bulk of the profession, look at China through their own eyes and experience, and tend to make judgments in terms of their own values. Seldom do they attempt to evaluate how things look from the other side of the river. Understandable as this may be, the results at best are conducive to misunderstanding and sometimes a great deal of mischief.

Now that at long last civility is being restored to Sino-American relations, *Civil War in China: The Political Struggle 1945-1949* by Suzanne Pepper is a splendid contribution to our understanding of that part of the Chinese Revolution between the defeat of Japan and the proclamation of the Peoples' Republic of China which has affected history for all time. Writing largely from Chinese sources, many of them previously unused, Pepper gives a dramatic account of how Chiang Kai-shek and his Nationalist Government, which in the fall of 1945 seemed to have the Mandate of Heaven if anyone ever did, could in less than four years lose every shred of it to the Communists. It was not primarily a military story. It was a political one in which the corruption and incompetence of the Kuomintang in the end turned every section of the population against it.

Business people and industrialists had no illusions as to their future under the Communists; and the intellectuals had good reason to suspect that untrammelled freedom of expression and the continuation of their traditionally preferred position in Chinese society would not be acceptable. Nonetheless, both came to feel that anything would be preferable to the Kuomintang. Even these essentially urban defections would have been insufficient had the Communists been unable to mobilize peasant support. Here Pepper makes a major contribution. The landlord-tenant problem, ownership of land, was not a major problem in north China, and the Communists muffled it prior to 1949. What was needed was a whole new definition of political and social relationships, namely, of power, who exercises it, and to what ends. It was precisely here that the Communists demonstrated their finest understanding of the Chinese people. Having acquired this support, the Communists had become invincible, as those of us who lived there at the time did understand.

Apologists for the Kuomintang have tried to make a case that although it lost, the Communists did not win; they simply flowed into a power vacuum. The thesis never did make much sense, but Pepper shows that it simply was not true. Even before General Marshall gave up in disgust, no one in China, the general included, any longer believed anything the Kuomintang said, and everyone knew the Communists meant exactly what they said and said exactly what they meant. One might not like it, but the security this promised was preferred to the chaos and misery that had wracked China for 50 years. And if the military part of the story was not primary, still, the impeccable behavior

of Communist troops was unprecedented in Chinese history, whereas that of Kuomintang troops was conventional in a society in which the soldier was traditionally dragged from its dregs and behaved accordingly.

It is fashionable in some circles to attribute the victory of the Communists to the opportunity offered by the Japanese War to identify themselves with Chinese nationalism. Pepper does not ignore this argument but she does, I think, place it in perspective. The Communists did exploit it to the fullest, but they also used it to learn and correct mistakes made in Kiangsi. The Kuomintang had an even better opportunity and missed it on every count. If one wishes to speculate as to why Japan invaded when it did, a major reason must have been an estimate that Chinese nationalism had reached a point which would soon make it inviolable. It was Mao Tse-tung who also understood this, not Chiang Kai-shek. Chiang's obsession with the Communists and with Confucian feudal politics, which he understood only too well, had a kind of fatal self-fulfillment about it.

The Chinese being a very private people about their own internal affairs, we are not likely to have a better account of the politics of a major turning point in history than this one.

JOHN F. MELBY

University of Guelph

**Social Democracy in Capitalist Society: Working-Class Politics in Britain and Sweden.** By Richard Scase. (Totowa, N.J.: Rowman and Littlefield, 1977. Pp. 184. \$15.00.)

Questions about ideology bedeviled political scientists before Marx, but his writing set the tone for research and theory on ideology in industrial societies. Without elaborating on the development of class consciousness, Marx makes it clear that ideology arises from objective conditions. Attempts to investigate whether differences in objective conditions do indeed give rise to differences in ideology and political activity have confronted two barriers. First, even if the hypothesis is correct, the result may not occur because the variation in conditions across advanced societies represents only a truncated portion of the range Marx envisioned. Second, given the infrequency with which ordinary citizens link issues into ideologies, it is virtually impossible to hold that meaningful class consciousness springs automatically from conditions in the real world.

Political sociologists have lately taken a more Weberian tack, arguing that causation runs the other way. That is, it is not so much the fact of inequality as the changing views which leaders of working-class movements and parties take toward it, which determine whether and how such issues come into the political arena. This makes for a more complex picture. Where politics was once epiphenomenal and parties little more than creatures of their class constituencies, politics and conflict now move toward center stage. Party leaders, though dependent on their class constituencies, play an important role in shaping the demands to which the government responds. Frank Parkin has articulated this approach most forcefully, proposing (in *Class Inequality and Political Order*) a classification of party ideologies in terms of their potential for fostering redistribution. A working-class party might adopt either a *subordinate* value system, which accommodates and legitimates existing inequalities, or a *radical* one, opposing extant distributions. Although the radical ideology may seem a "mere reflection" of class interests, its more important function is to translate latent objective interests into electoral demands and a political program. The party contributes to class consciousness by acting as exemplar of the class's political interests. In *Social Democracy in Capitalist Society*, Richard Scase, a British political sociologist, attempts to test Parkin's thesis in a comparative context, focusing on the Labor Party in Britain and the Social Democrats in Sweden.

Although an hypothesis about the effects of party leadership on the political consciousness of citizens could be investigated solely with attitudinal data, Scase begins with the prior question of policy outcomes. Stipulating (largely on the basis of campaign announcements) that the Swedish party is the more radical, Scase expects that the Social Democrats will have achieved a greater equalization of income and wealth than the British Labour party. Relying on a wide range of published research and government statistics, he finds precious little evidence for the belief that redistributive policies have equalized economic rewards or enhanced the opportunities for upward mobility by members of the working class in Sweden. This finding—that in objective terms some four decades of Social Democratic rule have left the Swedish working class in virtually the same position as Labour's episodic participation in government has left the British working class—suggests that party ideology may not matter in the final analysis, i.e., in the ability of the party

to equalize the ownership of social wealth. It seems to imply that, if the parties have different effects on their constituents' political beliefs, then these beliefs can be nothing other than false consciousness. These findings may be explained by a closer look at the policy-making process. It is unfortunate, then, that Scase does not address current literature on policy making in the two countries, particularly Heclo's argument (in *Modern Social Politics*) that party competition has not mattered greatly to the development of the welfare state in either nation. While this growing body of literature would not necessarily have embarrassed Scase's theory, confronting it might have honed a sharper set of testable propositions.

As for the effect of party ideology on citizen beliefs, Scase's choice of these two countries makes the demonstration of his hypothesis difficult, since their leftist parties are so similar. The measured effects might have been different had one of these parties been contrasted with the American Democrats or Italian Communists. Data come from a small-scale survey of workers in two factories. They reflect the laudable subtlety of his instruments, and their story is complex and sometimes contradictory. Scase reads the findings as supporting his expectation that Swedish workers will manifest a clearer appreciation of social inequalities and be more resentful of their continuation than the British. I find this interpretation tendentious. The image that emerges shows British workers convinced that status is transmitted by the family as a locus of advantage within an essentially unequal society, and Swedish workers perceiving a meritocratic system in which equality of opportunity operates through education (cf. pp. 102, 108). Scase concludes that Swedish workers are more "radical" because of their greater belief in equality, but this interpretation conflates a belief in equality of *opportunity* with the traditional socialist conception of equality, with its emphasis on *outcome*.

There are other annoying theoretical quirks. For instance, in a book focused on the question of change in working-class conceptions of social structure and political power, the notion of a political generation is absent—despite its centrality to the prevailing (Butler and Stokes) explanation of political change in Britain. Absent also is any consideration of the growing American literature on belief systems and on the interplay of political leadership and issue-conceptualization by the mass public. It is incredible that Scase is ignorant of this body of work—convergent with that of Parkin and

others—yet neither his conceptualization of the problem nor his citation of the literature suggests that he attempted to integrate this research.

Despite its flaws, however, this ambitious work makes an important empirical contribution to the neo-Weberian theories of class structure currently emerging through the efforts of Parkin, Anthony Giddens and others. If it provides a model for further, better work, it will have fulfilled an important niche in the literature.

M. STEPHEN WEATHERFORD

*University of California, Santa Barbara*

**Soviet Perceptions of the United States.** By Morton Schwartz. (Berkeley: University of California Press, 1978. Pp. viii + 216. \$12.50.)

The Soviet perceptions considered in this volume are those of the members of Moscow's Institute for the Study of the U.S.A. and Canada, founded in 1967. The book's primary data base is the institute's monthly journal *USA: Economics, Politics, Ideology*, and, secondarily, a number of volumes either sponsored by the institute or written by persons affiliated with it.

Initially, there is a brief consideration of attitudes toward American politics, society and the economy. The author reports that the institute's analysts have considerable respect for the strength and resiliency of the latter two because, despite the social and economic problems they have examined so closely, they never expected a social or economic collapse. As for politics, they have made considerable progress in moving beyond narrowly orthodox, simplistic approaches toward a more sophisticated understanding of the political system's complexities.

The volume is chiefly concerned with how the analysts treat the making and conduct of foreign policy. They have abandoned the earlier orthodox penchant for considering American foreign policy to be insatiably imperialistic because of its origins within the arcanae of the innately expansionistic economic elite which totally dominated the political system. They now see policy as a less venturesome and combative, more moderate and realistic product of a clash among opposing political forces and constituencies with the more "sober-minded" among them gaining ascendancy over the past decade or so. The analysts attribute this to a

growing realization in America that the correlation of forces, or balance of power, in the world has been shifting against the United States and toward socialism, that is, toward the forces headed by the Soviet Union. Consequently, in recent years U.S. policy has been less characterized by force, or even by the threat to use it, than heretofore.

The analysts have not seen this tendency as undeviating. They have been troubled perennially by what they term zig-zags in American policy. And following their excited optimism produced by the numerous summit agreements in the Nixon era, in recent years their enthusiasm has been tempered by the tougher policies they have noted in subsequent administrations.

A major motif of the book is policy advocacy by the institute as well by Schwartz himself. He sees the institute as a major supporter of detente and overall moderation in Soviet foreign policy. He feels that the United States ought to act moderately in order to encourage this tendency which, as he notes, leads a precarious existence in the Soviet Union. At the same time he advocates that a strong American military posture be maintained, "which, somewhat paradoxically, is vital to the argument of Soviet moderates. . ." (p. 169).

If this is not the first American book to deal with the broad topic indicated by the title, it is the initial detailed treatment of the thinking characteristic of this important institute which is indubitably influential in shaping Soviet foreign policy. The book's most apparent weakness is that its unusually heavy use of extensive quotations coupled with minimum analysis, interpretation and contextual explanation are more suggestive of a report than a study, and they impart a laconic and abrupt quality to the book.

A book limiting itself to such a narrow purview could be expected to have clarified to a greater extent than is done here the specific purposes and functions of the institute as those become evident in the journal. For example, the overwhelming majority of the articles in the journal concentrates on using the very latest American books, journals, news magazines and newspapers in order to report contending American views on a variety of topics in an unusually even-handed way, for a Soviet publication. This allows the journal to document changing American perceptions, moods, attitudes and institutional relationships and to comment upon the immediate significance of all these things.

Important as that is as an exercise in reporting on the current situation, it is not

necessarily productive of changes in long-term Soviet perspectives on the American political system which are strongly influenced by general theoretical (ideological) considerations. The latter are particularly salient in major policy statements made by the top political leaders. The significance of this important disparity ought to have been probed in a book involving policy advocacy.

The book is clearly useful within its substantial self-imposed limits. Schwartz could have made it more valuable by analyzing the materials more rather than by so literally reflecting them and by considering some of the broader questions raised by the institute's activities and position in Soviet academic and political life.

These criticisms aside, Schwartz has given his readers a great deal of interesting information about the institute and has contributed a unique argument in favor of moderation in American foreign policy.

RICHARD M. MILLS

*Fordham University*

**The Imperialist Revolutionaries: Trends in World Communism in the 1960s and 1970s.** By Hugh Seton-Watson. (Stanford, Calif.: Hoover Institution Press, 1978. Pp. x + 157. \$6.95, paper.)

This volume may be regarded as a companion to the Hoover Institution's annual *Yearbook on International Communist Affairs* which has been praised for the richness of its data and faulted for avoiding systematic analysis (*Slavic Review* 29: 704-05; *The Russian Review* 34: 224-25; and *APSR* 72: 819). Commissioned to write a short volume that would try to identify main trends in communist affairs during the decade of the *Yearbook's* publication, Seton-Watson has produced an essay whose lasting impression is more polemic than analytical.

Since the *Yearbook* is devoted to international communist affairs, Seton-Watson opens with an appropriate question: "Is there a world communist movement?" (p. 1) The answer is "no"; there is no longer in existence a single organized and disciplined world communist movement (p. 5). Instead we have essentially three political phenomena: a Soviet-led movement supported by Communist parties outside Russia proclaiming their devotion to Soviet policy; a Chinese-led movement looking to the theory and practice of Mao Tse-tung as its model; and a miscellaneous collection of Trot-

skyist and other heretical groups professing themselves to be Marxist-Leninists (p. 4). Placing these findings in historical context, the scholar of the international communist movement would be bound to note the demise of "monolithic communism" that served as a centerpiece of American cold war policy. One might conclude that since the unity and discipline of international communism have been shattered, surely the communist menace is not what it used to be. Not at all, according to Seton-Watson, because the three aforementioned groups share a common residue of Marxism-Leninism and regard "U.S. imperialism" as the permanent long-term enemy. Of course intracommunist feuds affect relations between communist parties and the West but to Seton-Watson the ideological factor makes it possible to attempt a meaningful discussion of "communists" in world politics.

On this infrastructure Seton-Watson proceeds to examine four main topics affecting communist fortunes: the attempts by parties to obtain power, by peaceful means; national liberation movements in the Muslim world, Africa and Latin America; communism in power; and communism in the Far East and southern Asia. On a technical level, Seton-Watson's ability to evoke an image of the problems and fortunes of a particular communist party through a capsule history compels admiration. His terse summary of the Eurocommunists' dilemmas of policy, viewed through the prism of the Allende experience, is also nicely drawn (p. 52). For analytical purposes, though, Seton-Watson's three-part model of contemporary international communism leaves us where we were prior to the fragmentation of the movement and the Sino-Soviet split. The Trotskyists and other miscellanea never were part of the world communist movement and in any case were not of much importance. A Chinese-led movement patterned on a Mao Tse-tung model is problematical, given the changes occurring in China itself. Finally, the central phenomenon of communist parties linked to the Soviet state on the basis of their devotion to Soviet policy begs the question of the quality of their devotion, as the author himself observes in his remarks on the dilemmas of policy in Eastern Europe and their own attitude toward national defense. Unquestioning loyalty to the Soviet leadership may have gone out with Stalin. In short, Seton-Watson's model leads to a reductionism that was inherent in the monolithic model: manipulation of the data to underscore the threat of communism. Thus condemnation by the West European communist parties of the

Soviet invasion of Czechoslovakia may have made the parties more popular, in which case it could be argued that the Soviet leaders strengthened their position in West Europe, contrary to conventional wisdom (p. 113). The small Portuguese Communist party "powerfully promoted the cause of communism and of the Soviet empire" by their contribution to Portuguese government policy that made possible the victory of the Liberation Front of Mozambique and the MPLA in Angola (p. 26). The communists are damned if they support their governments and damned if they don't.

If communism is judged weak now, it may well become a future threat. The communist parties in the Arab lands may be weak but the threat to the West is serious. Mass production of the intelligentsia and mass production of skilled workers to promote rapid industrialization, enthusiastically recommended by American or European advisers, may lead to mass production of revolutionaries. Though communist parties in Africa appear insignificant, "it would be wrong to dismiss communism as a force for the future" (p. 76). The potential had existed in Sudan and exists in South Africa, where the racial policies of the government might lead the non-black educated classes as well as the colored and Indian workers into the fold.

It is, however, the threat of the Soviet empire and the failure of nerve of the West, particularly of American policy, that is Seton-Watson's central concern. He deplores the "short-term hedonism" of the "political class" of professionals, business people, medium-rank officials, trade union hierarchs and mass-media manipulators as well as the collective guilt complex of Americans growing out of their treatment of the blacks, the Vietnam War and Allende which has combined to frustrate a "power elite," the top decision makers who "probably still retained more self-confidence and more will to keep power than appeared" (p. 141). He warns us in apocalyptic language of the possibility of the surrender of the "semibarbarous, industrially inferior, but militarily superior and politically ruthless Soviet empire" unless we mend our ways. It is an impassioned cry for an Imperial America to assume its historical obligations in the global contest with the communist Imperialist Revolutionaries.

BERNARD S. MORRIS

*Indiana University, Bloomington*

**Political Socialization in Western Society: An Analysis From a Life-Span Perspective.** By Barrie Stacey. (New York: St. Martin's Press, 1977. Pp. 176. \$17.95.)

Stacey's book is a timely successor to Herbert Hyman's pioneering one of 20 years ago (*Political Socialization*, Glencoe, Ill.: Free Press, 1959). Hyman's book was an inventory of the then-extant literature on the topic, and so is Stacey's. Stacey's review begins where Hyman left off but it differs from Hyman's work in that it purports to adopt a life-span perspective and is not restricted to data from the United States.

Stacey's analysis of socialization in Western society is both thorough and critical. While it includes studies of the early 1960s, especially those written from a system's analytical point of view, his major emphasis is on work done in the late sixties and mid-seventies. This more recent work demonstrates convincingly that "socialization does not necessarily result in civilized values and behaviour or necessarily contribute to the maintenance of political stability" (p. 2). Instead, the overall impression to be gained from the review is that change in political orientations and fluctuating levels of affect for the political system are far more common than one would have suspected on the basis of earlier work. Stacey's book thus goes a long way toward dispelling the charge frequently levelled against socialization research, namely that it is essentially static and conservative.

Stacey also familiarizes the reader with a wide spectrum of literature possibly not familiar to U.S. readers, such as work done in Finland, Italy, and France—to name a few. Here, however, he seems to have restricted himself to work published in English. What is lacking therefore, to cite but one example, is a review of the substantial literature of the last two decades on German youths. Also notable for its absence is material on Western societies which are not democratic. For the most part Stacey seems to equate Western society with democratic societies and more specifically with those under Anglo-Saxon influence, or so it would seem, inasmuch as a preponderance of the reviewed studies deal with England and the United States. In part this no doubt reflects the current "state of the art."

More problematic is Stacey's claim to having adopted a life-span perspective. Such a claim would lead the reader to anticipate a consistently developmental perspective throughout the book. This, however, is not the case. For one thing, it is difficult to detect the specific

developmental and/or life-time perspective to which the author subscribes. For another, there is a curious lack of integration from one chapter to the next. For example, in the first chapter he talks at length about the affective development of the child (spending an inordinate amount on the psychoanalytical transfer thesis) and all but ignores the child's cognitive development. Then in the next chapter, he deals in much detail with the adolescent's cognitive development but ignores emotional development. Development, however, is multidimensional; both the child and the adolescent develop cognitively and emotionally as well as physically. The chapter on radical youth repeats much that is known and dwells at perhaps too much length on non-empirical and frequently pseudo-psychological literature, but the data on youth's attitudes toward the Vietnam war do much to correct a prevalent image of youth as the vanguard of the antiwar movement. Unfortunately—the last chapter notwithstanding—childhood and youth remain the main focus of the book.

The claim to a life-time perspective, moreover, leads me to anticipate an equally thorough analysis of the changes which transpire in adulthood not only those connected with aging itself, but with occupational socialization, social and traumatic experiences, exposure to discrimination, to changing cultural norms. Such material is lacking. Curiously absent are data on subgroups, especially racial and ethnic ones, and on the recent changes in the socialization of women. Despite these shortcomings, it is an achievement in itself that the author recognizes that socialization proceeds through the life process and carries it forward, however modestly, into adulthood.

The last chapter, oddly enough, extends the review to Third World and Eastern Bloc countries. It is unclear why this was attempted here when these countries were ignored in the preceding chapters. Unfortunately, the book ends here, thus leaving us essentially with an important review of the literature but no more. Missing is a concluding chapter in which the author poses the question: just what general conclusions can we draw on the basis of this overview? It is the virtue of Stacey's book that it offers the reader enough material to pose the question for oneself.

ROBERTA SIGEL

*Douglass College, Rutgers University*

**The Politics of Agricultural Mechanization in China.** By Benedict Stavis. (Ithaca, N.Y.: Cornell University Press, 1978. Pp. 288. \$17.50.)

Stavis has written a useful survey of developments in Chinese policy regarding the mechanization of agriculture since 1949. This is an enormously important policy area, intimately related as it is to the key choices of the nation's economic development strategy, the building of rural bureaucratic and mass constituencies, and the real course of elite power struggles over the last 20 years. At various places in the narrative, some light is shed on each of these groups of issues, and on their inter-relationships, making it likely that parts of the book will be very helpful in teaching. However, the work's overall political analysis appears incomplete and, in places, obscure.

The presentation is chronological, from land reform through the cultural revolution, and Stavis is at his best discussing the earlier years. He performs an extremely valuable service in chapter 2 by gathering and evaluating the very wide-ranging estimates that have been put forward on deaths by violence during land reform. This question has provided ammunition for many emotional charges and counter-charges, with figures running up to 14–15 million deaths (p. 26), and so it is important to have Stavis review the evidence dispassionately and to offer this tentative conclusion:

It would appear that somewhere between 400,000 and 800,000 people were killed officially after 1949. . . . The Chinese Communist leadership had estimated that landlords and their families constituted 4–5 percent of the rural population—about 20 million people. This would imply that 1 to 4 percent of landlords' families met death. If a half million people were killed in land reform, this would be .1 percent of the rural population or 2.5 percent of the landlord class and would represent roughly one death in six landlord families (pp. 29–30).

Elsewhere Stavis considers it reasonable to assume that as many people were killed "unofficially" as officially, and so if he is correct, the total for the land reform period (1949–52), including non-landlords, may be somewhere over one million deaths. This is a credible estimate for purposes of comparison with agrarian reform movements in other countries. And in this context Stavis urges his readers to recall that the Chinese land revolution, often vicious and sometimes horrible, did nonetheless effectively put an end to a traditional rural order by no means usually humane or pacific, but itself heavily scarred by unreasoning terror

and violence, both official and unofficial.

In the next chapter on the 1954–55 debates over agricultural collectivization, there is a good summary of the issues at stake at the time, and some interesting statistical material on contemporary social and economic trends illustrating the very hard choices faced by the leadership. Here Stavis also makes the important point, often not apprehended by previous students of the period, that the seemingly solemn invocation of the precedents of the Soviet model of agricultural collectivization and mechanization by all parties to these debates in Peking was neither naive nor doctrinaire, but consciously rhetorical. For Chinese leaders in the 1950s, the issue never was whether or not to follow Soviet experience to the letter. By that time, although they were still obliged to speak of that model in glorious terms, they were all very mindful of past Soviet errors and disasters. The issue they did face, as Stavis shows, was how, if at all, it would be possible to disentangle the positive and progressive elements of what the Soviets had done in agricultural collectivization from the politically and economically devastating mistakes they had made. For while the Soviet economy was in a state to survive mistakes of that order, Chinese leaders well appreciated that their economy was not.

However, at about this point in the study, a disconcerting vagueness sets in. The chapter on the organization of machine tractor stations leaves a surprising number of basic questions dangling. The section on the economic theory of the Great Leap is neither sophisticated nor complete. And the account of the post-cultural revolution reforms in mechanization policy touches only the high points, leaving the reader without a clear understanding of the complexities of making such a system work.

Part of the problem seems to be that Stavis has failed to consult any of the Chinese-language materials available on these topics. Despite the very impressive list of "Chinese Periodicals Cited" at the end of the book, it is in fact quite clear that the author has relied almost entirely on the various English translation series. This is adequate for some kinds of questions, but it cannot yield the depth of on-the-ground detail about policy implementation and reformulation that we expect in a good case study.

Another part of the problem is that Stavis consciously declines to adopt *any* model, or any combination of models, of the Chinese political process (p. 21) to assist in explaining policy change over time. The discussion leaps from the consequences of technological insuffi-



ciencies, to the vagaries of Politburo in-fighting, to the development of middle-level bureaucratic muddles. All are treated more or less equally as "constraints" on the process of China's agricultural mechanization, so that the reader is given almost no help in weighting the import of these various factors, and no clues for the construction of predictive hypotheses. The book is, therefore, more a chronicle than an analysis.

This is unfortunate, because the failure to develop an integrated outline of the political process prevents Stavis from achieving the major goal he sets for the work in the preface. He says there, "What I do hope can be learned from China's experience is that it is possible to impose political will over the path of technological development" (p. 8). But what his study actually illustrates is not clearly an imposition of political will over technological change, but a tortuous process of social and economic experimentation yielding a series of tentative, matter-of-fact, compromise solutions to complex problems only partially understood by decision makers and only partially responsive to their manipulations. The imposition of political will over the path of development may have made more headway in China than most other countries, and with Stavis we may hope that it will yet be decisive there. But the descriptive parts of his own work undermine our conviction in the matter, and this, when coupled with more recent development policy pronouncements emanating from Peking, must keep us skeptical of his book's opening assertion that "the honeymoon with technology is over."

VIVIENNE SHUE

*Yale University*

**The Citizen and Politics: A Comparative Perspective.** Edited by Sidney Verba and Lucian W. Pye. (Stamford, Conn.: Greylock, 1978. Pp. xv + 257. \$14.95.)

This collection of essays, written in honor of Gabriel Almond by several of his students, close associates, and friends, offers ample, if sometimes indirect, evidence of Almond's wide-ranging contributions to the field of comparative politics. The substantive gamut spanned by the essays is so great, in fact, that the editors are hard-pressed to establish an integrative framework for the volume. Political participation, highlighting as it does the citizen's role in socio-political life, is the overarching theme eventually settled upon, yet the separate

threads of the different essays fall short of fashioning any whole analytical cloth.

The essays are organized under three topical subheadings. Part 1, entitled "New Dimensions of Participation," contains three essays which clearly illustrate the broadening scope of inquiry concerning citizen participation. Whereas, as the editors note, earlier participation studies concentrated primarily on electoral activities and their systemic consequences, recent studies have frequently looked at more immediate concerns of citizens and the means employed to deal with them. Thus, in his essay "The Parochial and the Polity," Sidney Verba considers "parochial participation"—actions with narrow, highly particularized goals—and explores the foundations for both citizen perceptions of government as a potential provider for their needs ("parochial awareness") and citizen beliefs that government will actually provide needed services if called upon ("parochial expectation") in seven nations for which data are available. Findings from this inquiry are highly interesting and raise some vexing questions for the social service state since previous experiences with services rendered, especially if they are unsatisfactory, appear to exert a major influence on levels of parochial awareness and expectation.

The other two essays in Part 1 also look at narrower, more particularized forms of citizen activity and their implications. Wayne Cornelius investigates what he calls "demand-making" by the urban migrant poor in Mexico and finds that this activity, which is largely collective (springing from community improvement associations) but still highly parochial and limited in scope, displays little potential for producing a crisis-generating participation demand explosion, contrary to the expectations suggested by some theories of sociopolitical change. These findings corroborate Verba's claim that such parochially oriented activities have little bearing on other forms of political participation; despite their personal importance, these activities apparently do not furnish the basis for generalizing citizen political involvement. Finally, in an article devoted to an imaginative discussion of civil-court actions in Africa and the American South (citizen participation in what he calls "micro-policy formation"), Robert Mundt presents evidence which, due to its mixed character, calls into question several hypotheses concerning possible developmental relationships and sequences involving participatory structures and attitudinal orientations.

Part 2, labeled "Comparing Participation," encompasses four essays of a more varied

nature. Lucian Pye, in his essay "Participation and Authority: A Comparative Perspective," argues that participation can only be properly understood in the context of prevailing attitudes toward authority and political leadership, attitudes which derive from basic socialization experiences. This argument is illustrated by comparison of Western and Asian participatory practices—the former characterized by conflictual relations, the latter by paternalism. On the surface these practices appear unreconcilable, yet Pye suggests they constitute quite natural and historically stable outcomes given the possible combinations of dominant orientations to authority and participation (which he sets forth in an analytical typology) and the salient socialization experiences within each cultural setting. A subsequent essay by Myron Weiner focuses on the concept of citizenship and the recent tendency for this term to be imbued with multiple meanings, particularly in multi-ethnic societies, in contrast to its "classical" universal denotation. This tendency is illustrated by reference to India where a constitutionally guaranteed unitary citizenship principle has been eroded by passage of various domicile rules protecting labor markets and educational opportunities for members of specific ethnic groups. India, Weiner correctly notes, is not unique in this regard, but whenever such developments occur, they pose a critical challenge to national political loyalties and integration.

In the most ambitious essay in the volume, Scott Flanagan returns to the age-old problem of "why some democracies 'work' and others do not" (p. 129). Following in the footsteps of Almond and Verba, Samuel Huntington, and Dankwart Rustow, whose work he makes a heroic effort to integrate, Flanagan argues that successful democracy is dependent above all else on the attitudinal orientations of citizens. He then goes on to suggest a general framework for explaining the genesis of three cultural variants observed in advanced industrial societies. Flanagan's major contribution here is to identify two alternatives to the civic culture—one a "combative culture" characterized by alienated parochial-participant orientations, the other a "spectator culture" characterized by passive parochial-subject orientations—both of which represent potentially unstable products of prior developmental sequences. France and Italy, it is suggested in passing, are primary examples of the combative culture, while Japan (and, one might add, West Germany) is seen as an illustration of the spectator culture, a case which Flanagan discusses at length using as-

sorted survey research evidence.

The last essay in Part 2, by Joseph LaPalombara, is largely a continuation of an earlier polemic. LaPalombara's principal concern is the application of concepts originally formulated with reference to political phenomena in Western liberal democracies—terms such as "political participation" and "pluralism"—to Communist political systems in recent writing and the danger that such usage may leave these terms devoid of meaning. While this concern is legitimate, LaPalombara does not adequately respond to the gauntlet he throws down for others; indeed it would seem he contributes to the very conceptual obfuscation decried in others when he ties political participation to pluralism (pp. 171–72). Why genuine citizen participation, even as LaPalombara defines it, can only occur under conditions of pluralism is neither logically nor empirically clear. The crux of the conceptual problem with political participation is that most citizen activities only achieve their political significance due to the motivational considerations associated with them. Yet, as LaPalombara indirectly acknowledges in an important aside (p. 172), political analysts can do no more than infer the political intent of most citizen acts. Until this methodological obstacle is overcome, and it may well prove to be one of several intractable problems confronting political scientists, the study of citizen participation will continue to be fraught with analytical ambiguities. Despite this weakness in LaPalombara's argument, his basic point remains valid—political scientists must be sensitive to the conditions under which citizen activity takes place—and his discussion of mobilization as a political technique contains a provocative prediction that merits further consideration.

Part 3 of the volume consists of three essays concerning the "Linkage of Citizen and State." In a joint essay, G. Bingham Powell and Lynda W. Powell focus on citizen-elite attitudinal concurrence, exploring new methodological approaches to this question and then applying the techniques discussed to citizen and mayoral opinion data from 48 Austrian communities. The strength of the Powells' approach is that it permits analysis of the structure of opinion representation, not merely the degree of representation alone. The findings presented, moreover, offer some noteworthy evidence regarding the efficacy of elections as mechanisms for transmitting citizen attitudes. Bernard Cohen's essay on "Citizen Participation in Foreign Policy" which follows is largely a retrospective and prospective overview of the topic, an

attempt to assess what we know in fact and what remains to be discovered about the role of citizens in foreign policy making. Cohen's conclusion is that the present state of affairs is akin to an impressionist painting: general features are readily recognizable, especially from a distance, but specific details dissolve on closer examination. In response to this situation, Cohen prescribes a set of criteria which he believes will be useful in guiding further research on the subject and then elaborates upon these suggestions in a discussion devoted to the role of the press. The final essay is one by Robert Packenham on "Social Science and Public Policy" in which he addresses the role of social scientists as citizen participants. Packenham ranges over many issues, including whether social science is genuinely applicable to public policy making; if so, whether it is desirable; and again, if so, under what conditions should such participation take place. These normative questions, of course, do not lend themselves to easy answers. Rather than attempting to stake out any definitive positions, therefore, Packenham contents himself with a lucid discussion of some of the problems involved.

These remarks, to be sure, only skim the surface of the essays in this volume. In summing up, however, two observations are in order. First, readers will find that seldom have the efforts of scholars been subject to worse treatment in publication. Typographical errors are endemic to our profession and should perhaps be stoically endured. But in this book, such errors become epidemic—three or four egregious errors per page are not uncommon—and general editorial sloppiness appears to have achieved virtuoso status. Complete omission of Figure 1 from Mundt's essay, omission of narrative prose from Packenham's essay, an inability to alphabetize reference listings, and inappropriate page repetition are merely some of the more glaring examples of assorted transgressions. It is, therefore, only by virtue of the diversity of the issues raised and the quality of scholarship brought to bear that in spite of these distractions, the volume stands as a fitting tribute to the eclectic influence and inspiration Gabriel Almond's work has provided to others in the discipline.

LAWRENCE E. ROSE

University of Virginia

**Sons of the Soil: Migration and Ethnic Conflict in India.** By Myron Weiner. (Princeton, N.J.: Princeton University Press, 1978. Pp. xviii + 383. \$22.50.)

India is a unique political system. Within that uniqueness there are common elements which it shares with other societies although these common features appear in special arrangements and permutations. The outstanding value of this book lies in its portrayal of the singular interweaving of universal with particular realities and the way it brings to light many problematics of political decision in fullness of detail against the backdrop of general principles. It therefore has value for two different audiences: those interested in the wider ranges of theory who look for ways to incorporate such insights into full-bodied particulars, and, conversely, those specialists in the study of India who have a keen interest in the ethnic conflicts of the subcontinent and who will be led by the author to see this subject as a special case with wider implications.

Since this *Review* is not primarily attuned to the concerns of the area specialist, I will begin with Weiner's focus on comprehensive issues embodied in his last two chapters. The major question he addresses is: when citizenship means equality for every full-fledged member of the national community but at the same time individual states are granted autonomy to enact their own laws freely, how much leeway do states have to protect their citizens from competition by migrants from other states with superior qualifications for positions in the upper echelons in the economy or polity? How much leeway are individual states granted to protect their often less-qualified members in the scramble for middle-level jobs? Since any local protectionism means, in practice, discrimination against migrants as migrants regardless of merit, it violates the migrants' equal status in the national context by forcing them to become unequal in the local context. An important assumption, of course, is that equality entails the citizen's right to move freely throughout the nation without being hampered by "artificial" restrictions.

Realistically, however, such circumstances ebb and flow with the employment market and while it expands or contracts *pari passu* with the economy as a whole, such differentials as industrialization, educational facilities, population characteristics create regional variables. It is a limited range of these regional conditions that occupies the center of attention in Weiner's work.

This is because of India's singular position among the developing nations. No other country in the third world has given as much linguistic autonomy to its component states as India. Combined with this is a national caste elite reinforced by a British colonial system which produced a burgeoning middle class spearheaded more by bureaucrats than by merchants and entrepreneurs. As B. B. Misra has emphasized (*The Indian Middle Classes*, London: Oxford, 1961, p. 251), development of the economy made its way from the top downwards rather than from the bottom upwards. The fiercest competition for jobs was therefore for positions in the middle range where lawyers, accountants, *babus*, bureaucrats, teachers and white-collar workers of all sorts inherited their superabundance from British practice. The royal road to such positions was higher education on the one hand and sponsorship from incumbents in the system on the other.

These conditions set the stage for the special type of ethnic conflict portrayed by Weiner. Typically, the locals with their own language, dubbing themselves "sons of the soil" now react vigorously against the influx of those from other states who speak a different language, have more education, and hence come to fill major positions in the economy, public administration, and the universities (where English is often the lingua franca). The indigenous people retaliate through legislative enactments that legalize protected labor markets (reservations) for home constituents, setting up preferential hiring practices that apply to a wide gamut of occupations. When feelings run high, there are public demonstrations and even riots or violence against outsiders.

Weiner presents three detailed case histories of such conflict, one in Assam, one in Chota Nagpur and one in Andhra Pradesh (the latter a special case where locals, called *mulki*, predominate in the Hyderabad area while the more privileged group migrate to the capital from coastal or near-coastal regions). A fourth case—Bombay—receives only minor attention. Each of the major cases is carefully explored in historical, economic, demographic, and cultural-linguistic terms. In addition, Weiner has done extensive field work with carefully focused interviewing so as to make the viewpoint of antagonists luminous and intelligible. Every careful reader of this volume will come to appreciate fully the insoluble dimensions of public policy revealed by Weiner's analysis and thus concur in his final statement: "One of the major questions for the Indian government is

whether it uses its authority to build an internal common market, with special mobility as one of its features, or lends its support to those groups that want government to pursue internal protectionist policies. India seems likely to develop with either set of policies; the question is, what kind of India will it be?" (p. 371). While this judgment is premised on the assumption that one alternative will be chosen and the other rejected, the book as a whole makes it clear that problematic conditions will remain, regardless of choice.

One noticeable omission in the book is a discussion of the role played by caste nepotism in both higher education and the labor market—a mechanism enabling "outsiders" to establish a foothold and strengthen it. On the other hand, there are illuminating by-products of the research that are noteworthy such as mention of the affinity of locals for socialism rather than capitalism (p. 181) or the generalization that reserved jobs may do more harm in the private sector than in the public sector (p. 204).

*Sons of the Soil* is a splendid addition to Weiner's many volumes on Indian politics and a masterful analysis of nativism as an internal problem of governmental policy.

R. A. SCHERMERHORN

Claremont, California

**Careers in Shanghai: The Social Guidance of Personal Energies in a Developing Chinese City, 1949–1966.** By Lynn T. White III. (Berkeley: University of California Press, 1978. Pp. xiv + 249. \$13.50.)

Lynn White's book on state-steered career patterns and citizen response in socialist Shanghai draws on the voluminous research White has done on this city over many years, beginning with his dissertation work. Its impact lies in its attention to three knotty issues, central to political philosophy, development studies, and planning in socialist states. As White deals with these issues, a theme emerges which centers around the limits on planning, especially centralized planning for cities, in a developing socialist society.

The three issues, then, are these: the relationship between political policy, economic productivity ("material supply," in White's terms) and mass compliance; the balance between the individual's own desires and communitarian benefits, and how the state can handle this conflict; and, finally, the nature of

the interaction between state decisions, on the one hand, and "mass" or grass-roots action and reaction, on the other. In each case, we find that overall state-level political goals (perhaps best summarized in White's words on p. 206: "increasing capital funds and . . . increas[ing] the social efficiency of individual labor") are circumscribed, either by the level of economic prosperity, by individuals' preference for self-fulfillment and material gain, or by local responses that cause central planners to compromise in implementing their policies. This message of the checks on regime resolve, though left largely implicit, permeates the text.

White in fact spends most of his words in fleshing out these more philosophical concerns with his data. He does this by tracing state incentives and their popular effects over time (up to 1966) in four sectors in Shanghai, to each of which he devotes a separate chapter, and each of which he presents as a separate stage in an individual's life: education, the citizenship *rite de passage* of the youth-to-the-countryside (*hsia-hsiang*) movement, job placement, and residential controls. Here, through fascinating detail, we get a real sense of the individual aspirations and resentments connected with each policy sector, as we learn, not to our great surprise, that the Chinese of Shanghai and environs are enthusiastic about education, ambivalent (at best) about rustification, resentful if, as less-skilled and thus less-valued workers they are denied welfare perquisites, and that, if they are from the surrounding countryside, they are anxious to migrate into Shanghai and circumvent the registration and rationing systems whenever possible.

The state in China has attempted to find compromises and inducements to compliance—as by encouraging semi-autonomous spare-time schooling for workers and the unemployed when full-fledged facilities were lacking, and by promising future honors for present rusticated youth. However, after examining the fate of state efforts in directing individuals' urban residence and achievement patterns, White comes to an original and significant conclusion (pp. 216–27):

Individuals had more low-level power and mobility when the government was preoccupied with commodity shortages, or also when real prosperity lowered the value of the material incentives that the regime could offer for socialist careers. . . . The state had most power in daily affairs and the citizens had least, when economic prosperity was medium.

Particular strengths of the book include its wealth of data and statistics on urban strategy

in Shanghai over a 17-year period; its attention to continuities in policy over this period, as contrasted with the over-concern of many China scholars with shifts and cycles; its avoidance of rigid and hackneyed periodicization in decision-making analysis; its heavy reliance on the non-Communist press of the city, an unusual source in a field where governmental media are the data base for most work; and its effort to keep before the reader the perspective of the populace. Sections of the book that contain material difficult to find elsewhere describe illegal channels for subverting state controls, such as "underground factories," "spontaneous enterprises," black markets, and prostitution; and the conflict between state productivity concerns and the provision of welfare benefits. The especially useful chapter on residence includes insightful information on household registration, food rationing, urban transport and housing and birth control.

As is often the case with books that do a good job in presenting a particular viewpoint—in this case, convincing the reader of the limits on state direction—this volume leaves one wondering whether that theme might have been overstated. Thus, we finish with a view of a largely alienated citizenry (as White admits on p. 227) and of policy as very much a dependent variable. If these impressions are valid, how then are we to account for the very real change that has occurred in China over the past 30 years?

Also, three sorts of data, largely missing from this book, might have added to its value. White could have offered us a bit more background, both historical and political, on Shanghai itself, to help set the context for his story; he might have made more explicit the "developing society perspective" to which he refers on p. 10, perhaps by placing beside Shanghai's problems some comparative material on urbanization; and, though he adequately justifies a decision to discuss only the pre-1966 period (p. 4), it still would have added to the book had White noted expressly which of the social strains he identifies fed into the rhetoric and rebelliousness associated with the Cultural Revolution.

But, all said, White is to be commended for collecting, and then sharing with us this largesse of Shanghai lore, and for presenting it in a context of some sensitive and innovative theorizing.

DOROTHY J. SOLINGER

*University of Pittsburgh*

**The Russian New Right: Right Wing Ideologies in the Contemporary USSR.** By Alexander Yanov. (Berkeley: Institute of International Studies, 1978. Pp. xvi + 185. \$3.95, paper.)

Alexander Yanov's study of the resurgence of Russian nationalist thought analyzes both dissident and official nationalist ideologies and attempts to predict their impact on the future of the Soviet political system. His major hypothesis is that the "political meaning of contemporary Russian nationalism consists . . . in the gradual construction of an ideological base for the possible transformation of the regime in the USSR and the restoration of Stalinism" (p. 7). Yanov contends that (1) there are two major schools of nationalist thought in the USSR, the "dissident right" which originally identified with the political opposition to the regime and the "establishment right" supported by the regime, and that (2) although these two schools were originally in violent opposition to each other, they may eventually merge into a "military imperialist nationalism" in support of a future Stalinist type of regime.

Yanov argues that the Soviet leadership may well turn toward "military imperialist nationalism" because of its ostensible inability to cope with intractable foreign and domestic problems and in order to restore the system's capacity for mobilizing the society (p. 19). Yanov also asserts that the dissident right, unable to overcome the Westernism it perceives in the opposition movement and cut off from the masses by the government's control over the media, would repudiate its original support for political and intellectual freedom and endorse the "military imperialist nationalism" sponsored by the regime. While Yanov concludes that such a common doctrine of the Russian right has not yet emerged, he does believe that the dissident right has already shifted from its original liberal oppositionist stance to an "isolationist totalitarian nationalism striving for collaboration with the regime."

Whatever the validity of Yanov's prognosis (and his prediction is presented as a highly likely possibility rather than as the sole outcome of current trends), his discussion of the various strands of contemporary nationalist ideology is fascinating. His study stresses both the complexity and diversity of current nationalist thought as well as its striking resemblance to the Slavophil ideologies of the nineteenth century and the corporatist ideologies of the early twentieth century. Not only does he illustrate the nationalists' increasingly chauvin-

istic and messianic orientation, but he also attempts to unravel the conflict within the leadership which permits nationalistic diatribes to appear regularly in such official journals as *Molodaia Gvardiia*. In conclusion, Yanov maintains that the current leadership is ambivalent and confused in its approach to the revival of nationalist ideology. While it opposes the most strident and extreme appeals to repudiate Marxism-Leninism and adopt a purely nationalist, or nationalist-religious orientation, it has proved unwilling or unable to keep chauvinistic, anti-Semitic, and pro-Stalinist articles out of its journals or to resolve conflicts between the agitprop and cultural divisions of the Central Committee's secretariat over publication of these materials.

Yanov's survey is replete with historical parallels and some of them are very useful. His emphasis on the similarities between the current dispute between nationalists and political liberals in the USSR, on the one hand, and the Slavophil-Westerner debate in nineteenth-century Russia, on the other, effectively highlights the common preoccupation with the questions of Russia's "uniqueness" and the applicability of Western political institutions to Russian political culture.

Far less effective, however, is his effort to predict the nature of the post-Brezhnev regime within a cyclical theory of Russian history. Yanov contends that the current system may give way to a "Stalinist" regime on the grounds that Russian history has always been characterized by alternations between "soft" and "hard" authoritarian regimes. While there is no doubt that each successive regime in Russian history has been obliged to cope with the legacy of its predecessors, this process of adaptation hardly makes a return to the horrors of Stalinism the most likely possibility. Yanov seems to have an apocalyptic theory of historical development, to believe that Stalinism may return because of the Soviet leadership's inability to "solve" fundamental problems, and to reject the more plausible conclusion that the Soviet regime, like all other regimes in the world, does not even attempt to "solve" fundamental problems but simply muddles through.

Nevertheless, prognosis aside, Yanov's discussion of contemporary Russian nationalist ideology is truly essential for all those interested in any way in contemporary Soviet politics.

JONATHAN HARRIS

University of Pittsburgh

## International Politics

**The Pentagon and the Making of US Foreign Policy: A Case Study of Vietnam, 1960-1968.** By Jaya Krishna Baral. (Atlantic Highlands, N.J.: Humanities Press, 1978. Pp. ix + 333. \$15.00.)

This book is an account of Robert S. McNamara's long, dark journey into the Vietnam night. The author, an able young political scientist from India, examines the relationships of the secretary of defense with the joint chiefs of staff, CINCPAC, the military command in Saigon, the State Department, the Central Intelligence Agency, the White House and the Congress. Occasionally, a few of McNamara's close civilian associates come into view, notably John McNaughton and Alain Enthoven, his assistant secretaries for international security affairs and systems analysis, respectively. Yet the principal figure always remains the energetic and controversial McNamara, at first in general accord with the larger policy community, later at odds.

Drawing heavily on the Pentagon Papers and on congressional hearings, Baral examines the defense secretary's role in selected historical episodes, e.g., President Kennedy's decision to send advisors to Saigon, President Diem's demise, the Tonkin Gulf affair, the start of the bombing campaign, and the introduction of American ground combat forces. This method of organizing the evidence requires Baral to lead his readers through some of these episodes more than once as he attempts to come to grips with McNamara's multiple relationships. As a result, the book is repetitive. It is also marred by some awkward word usage, careless proof-reading, and the author's insistence on beginning each part of the argument with a dutiful recital of graduate seminar propositions, all dutifully footnoted.

But those who make it through to the third and fourth chapters, easily the best in the book, will find a number of rewards. First there are useful reminders, such as the observation that Hanoi's major support for the Viet Cong followed, rather than preceded, America's commitment to aid Saigon in the late 1950s. And that the Pentagon must have realized that the suspension of American aid would be interpreted by anti-Diem Vietnamese generals as a sign that we would not oppose a coup in 1963. Second, there are provocative guesses, e.g., that in easing McNamara out of office in 1967,

President Johnson may have intended to soften up the joint chiefs of staff for his own eventual decision to de-escalate. Third, there is the unusual insight that can come to an intelligent observer from another country: the Sino-Soviet rift probably contributed to America's readiness to intervene in Vietnam by allaying its anxiety over a communist threat to Western Europe.

In a less obvious way, Baral's cultural distance from the events under view probably helps explain why his work, with all its surface imperfections, stands up well against, for example, Robert L. Gallucci's *Neither Peace Nor Honor: The Politics of Military Policy in Viet-Nam* (Johns Hopkins University Press, 1975). Gallucci strains to explain as much as possible in terms of bureaucratic politics. Baral, on the other hand, is content to use that framework where it appears to be helpful, for example in explaining why the air force preferred an "enclave strategy" while army generals set out to "search and destroy." But he goes on to insist that major policy was shaped by more fundamental matters, by nothing less than the play of mass opinion or national ideology upon the highest echelons of government. Such a realization, I submit, comes more easily to one who stands outside America's Lockean tradition.

Finally, Baral's work is a useful companion piece to Richard K. Betts' brilliant study, *Soldiers, Statesmen and Cold War Crises* (Harvard University Press, 1977). While Betts demolishes the myth of a monolithic military, Baral documents the disarray of the civilians. Indeed, his most important point is that as McNamara began to disagree with the joint chiefs, he also came into conflict not simply with President Johnson but with elders of the Senate and with ordinary Americans, who remained hawkish long after doubt and disillusion began to trouble the civilian leaders of the Pentagon.

LAURENCE I. RADWAY

*Dartmouth College*

**Moderation from Management: International Organizations and Peace.** By Robert Lyle Butterworth. (Pittsburgh: University Center for International Studies, 1978. Pp. vi + 140. \$4.95, paper.)

This book is dedicated to the proposition that many attempts to evaluate the effectiveness of international organizations (IOs) are misguided because they are based on the assumption that, to be effective, IOs must acquire supranational authority. Robert Butterfield believes that IOs are more likely to exert a beneficial impact through a process of *strategic moderation*. That is, IOs can be effective just by providing opportunities for states to strengthen shared norms, perceptions, and habits of cooperation. Butterfield analyzes the United Nations, the Organization of American States, the League of Arab States, the Council of Europe, and the Organization of African Unity with the aid of a complex set of "operationally" defined concepts in order to determine the extent to which they have achieved strategic moderation.

I say "operationally" because the coding procedures used in this project were *not* "designed by a genius so they can be applied by an idiot." Rather, panels of experts reviewed the 166 conflicts that Butterfield analyzes, and made decisions regarding their "seriousness" (i.e., "how great a threat it posed to international peace and security"), and the degree of impact that the IOs had on them.

Inevitably, in such a process, the experts will make some decisions of debatable validity. For example, in their review of the cases involving the UN, the experts ranked the Soviet-backed coup in Czechoslovakia and the Soviet blockade of Berlin (both of which occurred in 1948) as among the least serious cases in the UN's history (p. 34). More serious in their eyes were, for example, the disputes between Rwanda and Burundi in 1963-64, and the Adeni-Yemeni border dispute in 1964-66 (p. 45).

Butterfield and the panel of experts also have a disconcerting tendency to attribute effectiveness to IOs on the basis of widely disparate kinds of evidence. If the number of disputes with which an IO must deal *declines* over time, Butterfield interprets that as a sign that the IO has successfully managed conflict in its domain, which seems fair enough (p. 75). But if the number of disputes that an organization deals with *increases* over time, that too is interpreted by Butterfield as an indicator of effectiveness, because the organization "has been kept relevant to these problems" (p. 43).

And if the UN fails to inscribe on its agenda the case of "Chinese aggression in Tibet," that is also interpreted as a sign of effectiveness, "because the act of noninscription itself was seen as a management effort" (p. 30).

A skeptic might suggest that this combination of debatable coding decisions and determined optimism accounts for some rather surprising conclusions by Butterfield. He reports that the UN, the OAS, and the OAU (but not the Council of Europe and the League of Arab States) have been quite successful in their efforts to achieve strategic moderation. More intuitive analyses of these organizations have come to more pessimistic conclusions. That could be because, as Butterfield argues, previous analysts have been looking for the wrong things, or because Butterfield has found evidence that is not visible to those who rely on less scientific procedures. Still, I suspect that some readers of this book will ask themselves: "Are these findings nonobvious, or obviously ridiculous?"

The key question that Butterfield addresses here is: "What would have happened if these IOs, which did exist, had not existed?" That is a useful question, but any attempt to answer it will confront terribly complex epistemological and methodological problems. That Butterfield and his colleagues have not solved those problems is not surprising. They have made an intelligent and conscientious effort to deal with them. That they chose to do so in an "operational" manner leaves them wide open to pot shots like those in this review; an analysis of these IOs based on more invisible procedures would have been less vulnerable to criticism. I believe, however, that such an analysis would have been less valuable than *Moderation from Management*.

JAMES LEE RAY

*University of New Mexico*

**Island China.** By Ralph N. Clough. (Cambridge, Mass.: Harvard University Press, 1978. Pp. x + 264. \$12.50.)

There are very few good books about Taiwan, and not very many on the international politics of the Far East. There is ample reason, therefore, to welcome this excellent one sponsored by the Twentieth Century Fund and written by a distinguished retired American Foreign Service officer with extensive experience in the Far East (including Taiwan), a fine professional reputation, and a list of significant



published writings. Although written shortly before the normalization of relations between the United States and the People's Republic of China, it is by no means obsolete or invalidated.

With unusual knowledge and objectivity, Clough analyzes the history and current status of Chinese Nationalist rule on Taiwan, the island's flourishing economy, Peking's attitude and objectives with respect to Taiwan, the bizarre only-in-China contest between the two unequal sides, and the island's unique international position including the interests and role of the United States, the prospects for normalization of relations between the United States and the People's Republic of China, and the likely impact of normalization on Taiwan. Since much of the information and analysis is derived from the author's personal knowledge and experience or from sources not readily available in the public domain, the general thinness of the documentation is both understandable and unimportant.

Clough's conclusions are compatible with what has happened since the publication of his book and with the Carter administration's Taiwan policy. In other words, he favors normalization with some safeguards for Taiwan's future security, especially the continuation of American arms sales to Taiwan, and he believes that Peking will accept, or can be brought to accept, such safeguards. In fact, of course, Peking did agree, evidently on account of alarm over the Vietnamese attack, with Soviet support, on Cambodia in early December, 1978, to ignore (not really to accept) the announced American decision to resume sales of "selected defensive" weapons to Taiwan, with no veto for Peking, after the expiration of the security treaty at the end of 1979. Most of Clough's colleagues would be happy to have come so close to an accurate prediction of the outcome of such a complex and difficult process.

Like most proponents of normalization, in the strict sense of establishing formal diplomatic relations as distinct from merely improving relations, Clough is better at explaining that normalization has been an announced objective of American policy since 1972 than at explaining why it is in the national interest of the United States. On the latter score, he cites the American commitment to normalization (in the Shanghai Communiqué), the presumed contribution of normalization to international stability in the Far East, and the possibility of deterioration of the pre-existing relationship between the United States and the People's Republic of China in the absence of normaliza-

tion. None of these arguments is unanswerable, and even taken together they are not necessarily conclusive; like most major policy decisions, normalization was an act of judgment and a gamble. Peking's attack on Vietnam so soon after normalization can hardly have been pleasing to American advocates of normalization and has certainly not encouraged confidence in Peking's willingness to refrain from the use of force against Taiwan.

As for Taiwan itself, Clough advocates avoidance by the United States of pressure on Taipei to seek an accommodation with the mainland. He believes, however, that such an accommodation is likely to develop gradually in some form or other, even in the absence of such pressure. Perhaps so, but it is worth noting that normalization has deprived Taipei of much of its bargaining power vis-à-vis the mainland.

Although Clough discusses, fairly if somewhat briefly, the critical question of mainland-Taiwanese relations on the island, he does not deal at any length with one of the most interesting aspects of normalization: its potential for exacerbating the rather difficult relationship between the mainland establishment and the Taiwanese elite. The course of this relationship may have a profound effect on the future of Taiwan. It might lead, for example, to the proclamation of a Republic of Taiwan (acronym: ROT), which would create a knotty problem not only for Peking but for those numerous foreign governments, including that of the United States, that have officially stated that there exists only "one China."

HAROLD C. HINTON

*George Washington University*

**The Making of United States International Economic Policy.** By Stephen D. Cohen. (New York: Praeger, 1977. Pp. xxiv + 208. \$18.95, cloth; \$6.95, paper.)

The importance of U.S. international economic policy has increased dramatically during the post-World War II era. International trade has grown more than 50 percent faster than total domestic economic growth. Domestic economic departments in the federal government have been hearing very clearly from their constituents that U.S. international economic policy must reflect the latter's growing vulnerability to the external sector.

As a result of these developments, Steven C. Cohen has written a timely and surprisingly comprehensive study of the decision-making

process in the U.S. international economic policy. The book is carefully written and well researched. The study is written from an "insider-outsider" perspective. Cohen was a recent staff member responsible for international economic policy to the Commission on the Organization of the Government for the Conduct of Foreign Policy. He is now on the faculty at American University.

A Washington anecdote, which the author discusses, illustrates just how important Cohen's topic is. Certain professional staff members of the Senate Finance Committee in 1976 proceeded to visit key executive branch offices and ask what the international economic policies of the U.S. were. "They heard not about specifics or how policy was made," the author writes, "but rather contradictions, disavowals of responsibility, equivocations, and criticisms of other agencies" (p. 204).

Cohen divides his analysis into three parts: "Substance," "Procedure," and "Prescription." Several of the individual chapters are especially well done. Cohen convincingly shows, for example, the significant impact which organization has on policy in chapter 3. He concentrates on the interaction within and between competing bureaucracies; chapter 4 traces the ascendancy of the Treasury Department in foreign economic policy. In chapter 8 Cohen analyzes how America's major economic partners go about organizing their own international economic policies.

In his discussion of the requirements for the optimal system for the making of U.S. international economic policy, Cohen notes that there is no such thing as the single perfect or utopian system. His proposal for a quick, well-defined, well-greased optimal organization is not matched by a persuasive discussion of how the necessary reforms can be brought about.

THOMAS N. THOMPSON

*University of Oregon*

**Arms Across the Sea.** By Phillip J. Farley, Stephen S. Kaplan, and William H. Lewis. (Washington, D.C.: The Brookings Institution, 1978. Pp. x + 134. \$7.95, cloth; \$2.95, paper.)

Any book published by the Brookings Institution demands attention—as would any book by any one of the three authors, all of whom are knowledgeable and experienced policy analysts and/or policy makers. Moreover, the subject of their work, the arms trade in all its

aspects, is both important and timely. It would, therefore, be gratifying to be able to commend this book to the student and teacher of security policy. Alas! I cannot do so.

One reason for this is that the book is far too short for what it attempts to cover: the world arms market; the various forms of U.S. security assistance (private and governmental sales; credits for arms purchases; military aid, education and training; supporting economic assistance; co-production, etc.); the history of these programs over the past 30 years; the criteria governing their use; the details of their application; and recommendations for the future. The result is that important issues such as security supporting assistance are covered in a paragraph and both policy analyses and policy recommendations all too often consist of sweeping generalizations.

A second reason is that the book, despite its brevity, is amazingly repetitious; thus, rationales for security assistance are covered not only on pp. 26–27 and 34–35 but also on pp. 39–41 and 104. More importantly, chapter 5, on the application of arms policy to participating regions and states, not only repeats, in greater detail, much information given earlier, but also is itself partly covered again in chapter 6, which outlines policies for the future.

The third and most important reason is, however, that the book puts forward both contradictory findings and conflicting recommendations; for instance, in the middle of p. 91 the authors call for a reduction of dependency deriving from U.S. security assistance, while on the bottom of the same page they recommend as an alternative economic aid, which may simply substitute one form of dependency for another. Similarly, they call (correctly) for U.S. restraint on "arms sales that might upset . . . [regional] stability" (p. 127), while at the same time sponsoring continuing arms aid to Israel—which some readers, if not the authors, might view as potentially destabilizing. Finally, they argue (persuasively) for measures to reduce U.S. military assistance while noting that this would not be viewed "by the Soviet Union and its arms-producing allies, or France, or other nations in the West as a model to be followed . . . [but] as a unique opportunity to obtain new influence and customers" (p. 50).

This does not mean that *Arms Across the Sea* is without value: it presents some informative data, it dispels myths about the levels of arms purchases by most Third World countries, and it gives a good summary of U.S. policies, both general and applied. Moreover, its trenchant critique of these policies seems justified by

subsequent events as well as by those it examines, and it raises a number of interesting suggestions as to future U.S. programs. One would, however, have expected Farley, Kaplan and Lewis—and the Brookings Institution—to do somewhat more than that.

J. I. COFFEY

*University of Pittsburgh*

**The Foreign Policies of West European Socialist Parties.** Edited by Werner J. Feld. (New York: Praeger, 1978. Pp. xi + 149. \$16.95.)

Werner Feld hoped his anthology would inform the scholar, foreign policy analyst, and interested lay person about an important but neglected topic—the foreign policies of European social democrats (see p. 3). While I agree with Feld that these policies, their roots, and their implications are important and have not received the attention they deserve, his volume does not make the contribution that he—or I—would have liked.

An anthology like Feld's should, at least, have the following characteristics. First, the various contributing authors should test a theory or at least answer a common set of questions in their chapters. Without one or the other, neither the reader nor the editor can confront theory with data, integrate the material presented, and substantially improve our understanding, in this case, of European socialism. Second, each substantive chapter should be well researched, well documented, well written, and well organized. Unfortunately, Feld's volume fails on both counts.

In the introduction, Feld realizes that the most important task of his volume is to document how and then to ask why the European socialists' foreign policies differ dramatically. But, because he did not impose either a theory or a common agenda on his contributors, neither he nor they can really grapple with these issues.

Ironically, the germ of such a framework can be found in the substantive chapters. At one point or another, the contributors discuss the effects of a party's size, its political situation (whether or not it is in or near power), its alliance possibilities, its internal divisions, and the domestic and international political contexts it finds itself in. Each of these factors clearly has an influence on the various socialist parties' behavior, but because there is no common focus to the articles or the volume as a whole, we know little more about the dif-

ferences in party behavior at the end that we did at the beginning of the book.

What we are left with, then, is a series of independent articles on the socialists' foreign policy in the United Kingdom, the German Federal Republic, Sweden, Norway, France, Italy, and Portugal; introductory and concluding chapters by Feld; and a brief analysis of social democratic history by George Windell. Had these been strong chapters, the book might still have been rather successful. But, alas, this is not generally the case.

Feld's introduction hints at a number of the key issues some of the contributors deal with, but by no means sets a coherent agenda. Moreover, he makes hordes of unacceptable substantive errors, as, for example, misspelling the name of French Socialist leader, François Mitterrand (p. 2) and mistakenly placing the death of the SFIO in 1971. His conclusion on international cooperation among European socialists considers an important new phenomenon, but does so too briefly and without any connection with the rest of the volume. Windell's chapter on the evolution of European socialism also deals with an important topic, but, it, too, has little to do with foreign policy, let alone the other articles in the book.

Yet, this book is not an abject disaster. While few of the substantive chapters break new ground, are based on original research, or deal with many issues in much detail, all will interest the interested lay person, if not the scholar of European socialism or the foreign policy analyst. The chapters on the lesser studied, smaller nations—Sweden, Norway, and Portugal—are the most thorough, insightful, and informative.

Still, when all is said and done, this is a frustrating book, both because it need not have been and because European social democracy has been so neglected in recent years.

CHARLES HAUSS

*Colby College*

**Recognizing Foreign Governments: The Practice of the United States.** By L. Thomas Galloway. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1978. Pp. xiv + 191. \$4.75, paper.)

L. Thomas Galloway articulates the change in U.S. recognition policy that has been quietly emerging in recent years, namely the trend to eliminate, in most instances, the act of recognition of foreign governments. From his case-by-case examination of U.S. responses to govern-

ments that came to power through extraconstitutional means between 1961 and 1976, Galloway correctly concludes that today, unless the U.S. perceives a major policy interest to exist, such as in Angola and Cambodia, it will de-emphasize recognition and follow what amounts to the Estrada Doctrine. His study records the growing tendency of the U.S. government to take the position that no question of recognition arises (or that relations are normal or are continuing), thus removing such sensitive issues as whether the act of recognition or nonrecognition constitutes U.S. approval or disapproval of a new government. This policy, while creating a "conceptual muddle" (p. 146), has generally worked well, he contends, whereas efforts to promote political objectives by withholding recognition have largely been ineffective.

Benefiting from information gathered as an extern with the State Department's Legal Advisor's Office in 1972, Galloway, a public interest lawyer at the Center for Law and Social Policy in Washington, presents a brief, historical overview of U.S. practice from Jefferson through Eisenhower. Then, grouping states regionally, he analyzes each administration's response to *coups d'état* and revolutions through Gerald Ford. One chapter centers on the recognition practice of other states, using information provided by U.S. embassies in response to a State Department communication in 1969 and to the author's follow-up inquiry in 1975. In a concluding chapter, Galloway recommends that the Department of State *officially* announce that it has decided to eliminate the concept of recognition of foreign governments from U.S. diplomatic practice. The book includes a foreword by R. R. Baxter, the detailed responses of foreign governments about their recognition policy, and analytical tables on criteria, time span between events, and patterns of consultations. Analyzed also are doctrines which have been prominent recently, including the Estrada Doctrine, the Tobar or Betancourt Doctrine, the 1964 Mann Doctrine, the 1969 Cranston Resolution in the Senate, and the 1965 Rio Resolution providing for hemispheric consultations.

The book's major contribution is its analysis of *practice*, including some insight based on behind-the-scenes information; this raises questions about generalizations which appear in existing literature on recognition. Galloway shows that from the time of Jefferson through 1960, "U.S. policy varied from a *de facto* policy of almost automatic recognition to the active use of recognition to achieve policy

goals" (p. 42). Thus, the application of the traditional criteria—effective control, consent of the people, and willingness to comply with international obligations—changed and was a source of disagreement and confusion among and within varying administrations. Furthermore, the author's data on foreign governments' practice shows that 30 of the 100 states surveyed follow the Estrada Doctrine or something similar, with others following it without being familiar with its name. This is contrary to Ernst B. Haas' observation in a 1969 essay which rather casually examines U.S. recognition policy (in *U.S. Foreign Policy*, Paul Seabury and Aaron Wildavsky, eds. [New York: McGraw-Hill, 1969], p. 75).

A strength of the book—the author's access to State Department records—has been converted (perhaps unavoidably) into a twofold weakness. Galloway fails to identify consistently the portions of his analysis which are based on these materials (it is probably safe to assume that unattributed footnotes are a clue, e.g., Guatemala, 1963; Peru, 1962; Argentina, 1966 and 1971; and Bolivia, 1969); and there is no precise reference to cases from which generalizations are made. These omissions are evident in Galloway's interesting description of the techniques used by press officers in dealing with persistent reporters. "Only if pushed hard, with no avenue of retreat, would the press officer announce that recognition had occurred. And even when this was conceded, the press officer was cautioned not to mention a particular act or point in time when recognition was accorded" (p. 146). Also, in reviewing Latin responses to the Rio Resolution which calls for consultation prior to recognition of governments established by illegal means, Galloway describes some consultation as "perfunctory" and others where "a good faith effort has occurred" (p. 133). In places, Galloway's observations tend to exaggerate. For instance, he writes that "Peru (1962) serves as the best example of the use of recognition and financial aid as bargaining levers to obtain a promise to return to constitutional government through free elections" (p. 49), yet in later paragraphs acknowledges that the U.S. was forced to back down. In the final chapter, Galloway departs from an analysis of practice to respond to arguments advanced against a policy which ducks the question of recognition. Here he only begins to examine the problems which may arise, such as situations where there are rival claimants to power or sustained political stalemates. He raises pertinent issues which should be explored at length by other scholars, and

this was probably his intent.

In spite of certain limitations, some self-imposed, one should acknowledge the fine contribution Galloway makes in providing a record of actual practice in an overlooked area in U.S. diplomacy.

JOAN KRUEGER WADLOW

*University of Nebraska, Lincoln*

**Latin America and World Economy: A Changing International Order.** Edited by Joseph Grunwald. (Beverly Hills, Calif.: Sage Publications, 1978. Pp. 323. \$18.50, cloth; \$8.95, paper.)

This collection of original essays comprises the second volume in the Latin American International Affairs series sponsored by the Center for Inter-American Relations in New York. Twelve chapters and two commentaries by 17 authors deal with the changing character of Latin American economic relationships with developed countries, the rest of the Third World, and among themselves. The mixed nature of the collective authorship (including economists, political scientists, and historians, eight of whom are North Americans, seven Latin Americans, and two Europeans) is salutary. It helps ensure a broad spectrum of perspectives, to include, refreshingly, Latin American perceptions of their own problems and relations. Joseph Grunwald and his coauthors reasonably view the Latin American region as an important and manageable level of analysis while recognizing its heterogeneity. Grunwald sums up Latin America's global economic position with these words: "The poorest countries are among the poor of the Third World, the wealthiest could well be classified among the developed First World" (p. 7).

Virtually all multi-authored works seem to suffer to some degree from scholarly inconsistency and loose conceptual ties among the various contributions. In this instance the quality of effort varies and some discontinuities exist. Furthermore, certain important problems deserve more attention, such as those related to multinational corporations, dependency, and the communist world. But these difficulties are minimized, partly because the topical coverage is generally well conceived, all of the chapters are written for this volume, and Grunwald's opening essay largely succeeds in drawing together the total enterprise.

The opening chapter is a set of "reflections"

by the editor on Latin America in the world economy, setting out the major issues, and concluding that Latin American governments will continue to fight within the Third World for the New International Economic Order. Four subsequent chapters analyze Latin American economic relations with industrial countries. Albert Fishlow's provocative criticism of U.S. policy focuses on restructuring trade, foreign investment, and capital flows. Laurence Whitehead is convincingly pessimistic about Britain's economic prospects in Latin America, foreseeing little increase in trade or investment. Albrecht von Gleich's judicious chapter leaves a picture of declining but still significant trade relations between the Federal Republic of Germany and Latin America, and of inadequate negotiating machinery between the European Community and the region. Claudio Velez argues much too briefly that trade between Latin America and the Western Pacific will almost certainly increase; in my view, he is convincing only with regard to Japan.

Mexico and Brazil receive special attention in separate contributions. A short chapter by Leopoldo Solís on the external sector of the Mexican economy is informative but largely descriptive. Werner Baer and Carlos von Doellinger divide Brazil's foreign economic policy since World War II into two periods, as its concerns shifted from traditional free trade and inflation fighting to incentives to increase and diversify exports. Three further chapters are devoted to regional integration and economic relations with the Third World. Ricardo Ffrench-Davis argues that the Andean Pact is a model of economic integration for developing countries; his solid treatment is more optimistic than the facts of intra-group conflicts would seem to warrant. Clark Reynold's treatment of Central American economic prospects is a sophisticated economic analysis that falls short of sharply linking politics and society to economics as he sets out to do. Felipe Herrera, in a well-informed and provocative analysis of Latin America in the Third World, is too sanguine in his views of the latter as coherent, united, and effective in international councils.

Two major issues are confronted in separate chapters. Luis Escobar addresses external financing in Latin America from a technical administrative perspective. Douglas Bennett, Morris Blachman, and Kenneth Sharpe coauthor a balanced and dispassionate analysis of Mexico and multinational corporations, emphasizing that both operate within important constraints. The book ends with two brief commentaries. Bryce Wood's nicely crafted piece

asserts that the Linowitz Report of 1975 denies the usefulness of historical experience in the formulation of a new U.S. policy but actually restates the substance of President Franklin Roosevelt's Good Neighbor Policy. Gabriel Valdéz S. offers useful descriptive and analytic notes on the new Latin American Economic System (SELA).

Grunwald and his colleagues demonstrate the intimate relationship between economics and politics in international affairs. They clearly illustrate the considerable economic content of today's international politics, and amply reveal economic problems that may be described as political in nature.

G. POPE ATKINS

*United States Naval Academy*

**The European Parliament and the European Community.** By Valentine Herman and Juliet Lodge. (New York: St. Martin's Press, 1978. Pp. ix + 199. \$19.95.)

In their preface the authors of *The European Parliament and the European Community* note that the book "grew out of a conversation over wine in the cafe of the European Parliament. . . ." The book, like many vintages of the Moselle, is best appreciated when sipped. The organization of the text and of its component chapters is highly concentrated. The authors know the European Parliament, and other European parliaments, well and assume that their readers are not beginners.

The book is divided into two major sections and has ten chapters overall. The first section is a careful description and analysis of the current (pre-election) powers of the EP. That section is done with consummate skill and with a verve not frequently seen in legal-formal writing. The second section opens with a review of the notion of legitimacy as viewed in the West and then turns to a consideration of the implications and difficulties of a future (post-election) EP. Specifically, they turn to an examination of public opinion, the involvement of the media, the future of the dual mandate, and the possibility of a bicameral European Parliament.

The authors' view of the EP as it is currently structured takes as its point of departure a current paradox that has surrounded the agonizing decision(s) leading to the promise of direct elections to the EP. On the one hand, some have argued that, since the body is so weak, Europe-wide elections would be a waste of time and effort. On the other hand, the argument is made that without elections the EP

has no justification and no possibility for increased powers. As the authors note, it is a chicken-and-egg standoff. The arguments about what should come first, direct elections or enhanced powers for the EP, though, comprise only the surface aspect of the real problem.

One major concern is based on the notion that political integration of the Community cannot continue (or recommence) without direct popular input—one cannot integrate democracies without democracy. But equally bothersome is the fear in some quarters that participation in direct elections will transfer sovereignty and allegiance to a supranational unit at the expense of national parliaments and member states. The authors treat these concerns with sensitivity—they create no villains and no heroes from European federalists or nationalists. Yet they suggest that the immediate impact of direct elections is easily exaggerated. They argue, relative to the question of the transfer of sovereignty, that there really is not a "European citizenry" that "wishes to give expression to a European identity transcending the nation-states." Furthermore, any growth in the powers of the European Parliament at the expense of Community institutions like the Council of Ministers or national institutions like parliaments "can be almost indefinitely postponed."

The evidence which Herman and Lodge bring to bear in support of these points is largely contained in the first half of the book. After an opening review of the status of parliaments in democratic theory, and a nod to the "decline of parliament" thesis, they turn to a painstaking analysis of the powers and functions of the European Parliament.

It is not opportune in a brief review to trace the development of their reasoning in any detail. Still, to give a broad overview, after examining the legislative, financial, and control powers of the European Parliament, Herman and Lodge conclude in chapter 5 that direct elections to it should not be greatly feared (or welcomed, presumably) because the European Parliament is not a parliament. In fact, they do not merely conclude that it is not a parliament—they give no fewer than nineteen reasons why it is not.

If the European Parliament is not really a parliament, what then is it—or might it become after direct elections? This is the concern of the second half of the book. Herman and Lodge open their response to the question by noting that direct elections will both enhance and exacerbate the legitimacy problems of the European Parliament and Community institu-

tions. Elections clearly add an aura of democracy to the Community institutions, but at the same time they will underscore the intangibility, unintelligibility, and virtual invisibility of the European parliament—in other words, it amounts to a body with symbolic input but neither specific nor diffuse outputs. Most serious of all in the authors' view is the fact that the European Parliament does not promise to behave as a "grand forum" for the European idea.

The second half of the book is devoted to the theme of the European Parliament as a grand forum in the sense of an education device for the European masses. Their argument is detailed, complex, well documented, and in touch with good political sense. Herman and Lodge elaborate on the requirements of "European" parties, devote an interesting chapter to the European media, and elaborate on the enormous difficulties created by the dual mandate of MEPs. Finally, they suggest a restructuring of the Community's institutions from a bicephalous executive (Commission and Council) to a bicameral legislature (Parliament and Council).

However, some students of parliaments in general, and some students of European integration may take exception to the authors' rather single-minded devotion to a European Parliament that serves as a tool to educate European masses—a grand forum of socialization. That is, no real attention is devoted to a European Parliament as an aggregator of popular or corporate interests. Nonetheless, this text's numerous virtues outweigh any vices and it is required reading for anyone interested in a parliament that is most definitely not in decline.

JOHN K. WILDGEN

University of New Orleans

**The Nation-State and the Crisis of World Politics.** By John H. Herz. (New York: David McKay, 1976. Pp. vii + 307. \$5.95, paper.)

John H. Herz is one of a small group of European-American scholars who have played a decisive role in the emerging study of international politics in the United States. Herz's importance is less easily defined than that of Hans J. Morgenthau, Karl Deutsch and Arnold Wolfers, with whom he associates himself in a lengthy autobiographical introduction to this work. That he should feel compelled to write

his own *Festschrift* is in itself revealing. The fundamental question inescapably arises in the mind of the friendly critic: is Herz a first- or second-rank political thinker?

If Herz had devoted himself unreservedly to international politics, his stature as a major figure almost certainly would have been proportionately greater. More than half his writings fall within the field of comparative politics and he is co-editor of the journal *Comparative Politics*. His formation in his native Germany and at the Geneva Graduate Institute of International Studies was in law, not politics, and his mentor was Hans Kelsen, founder of the "Pure Theory of Law." The rise of Hitler constituted for the young Herz "a brutal awakening" and opened his eyes to the role of power in international affairs. His first book (published under a pseudonym) on *The National Socialist Doctrine of International Law* warned of the deception of such writings in obscuring the warlike intentions of early Nazism. Herz's disillusionment with the world had its origins in the failure of collective security, the destruction of socialism in Russia on the anvil of Stalinism and the triumph in Germany of Nazi totalitarianism.

Yet Herz's disappointments failed to obliterate his inborn idealism and the indestructible romanticism which have characterized all his subsequent writings. In the first of a series of essays brought together in the present volume entitled "Power Politics and World Organization," he sought to delineate the "in-between system" between the anarchy of power and an unrealizable world government. His continuing blend of realism and idealism was expressed in one of two seminal works by which he must be judged: *Political Realism and Political Idealism: A Study in Theories and Realities* published in 1951 but written in the interwar period. Its significance lies more in the value of certain original political concepts, for example, the security-power dilemma, than in his political prescience (he acknowledges his failure to foresee the emergence of a bipolar world or the creation of nuclear weapons). His second classic work, *International Politics in the Atomic Age* (1959), not only remedied the deficiencies of the first book in describing postwar realities but added new concepts, for example the "impene-trability" of, first, the territorial state and, in the cold war, the postwar blocs of "client" and "satellite" states. He also set out to describe the strategies of nuclear deterrence thrown up to avert the threat of nuclear annihilation. Taken together, these two books of the 1950s from which valuable excerpts are reproduced in *The*

*Nation State* brought Herz to the threshold of joining other "Master Thinkers in International Politics."

Yet Herz's lasting influence as a theorist has been somewhat marred by three limiting factors. First, he represents, as he has said, the lonely individual scholar at work in a private intellectual laboratory, seldom if ever interacting with other theorists, including those who share his commitments and to whom he may even be indebted. Second, anyone who traces the evolution of his thought is impressed by a tendency to embrace utopian liberal attitudes (he once described himself as a liberal realist) not self-critically or dialectically as Reinhold Niebuhr did, but with his own brand of romantic enthusiasm. Third, Herz's capacity to shift his ground and adjust his thinking to new realities is both a strength and a limitation.

If collective security and problems of the nuclear age as they affect the rise and demise of the territorial state represent the first two stages of his thought (two exceptional articles on appeasement and detente must be mentioned here), his discussion of science and technology and the acceleration of developments in all fields of international relations constitute the final stage of his thought. Not only has attention shifted to the novel problems of food and population, environmental and resource depletion and "survival ethics" in this stage (an area of concern rather consistently overlooked by most political realists) but he also brings the power of a *Kulturkritik* to these vital areas, approaching them on the broadest humanistic front. He postulates that a "minimum ethic of survival" must supercede past reflections on individual or group ethical standards as evidenced in historic international politics. Yet he despairs over the malfunctioning of the nation-state system and the obstacles to realizing "survival ethics" in most foreign policy decisions, especially in the United States. New concepts are introduced, such as external and internal legitimacy, particularly for the 100 or more fledgling states. America's survival is threatened by its internal divisions into two societies, the active and inactive (the underclass or welfare groups) societies and by wasteful consumerism and selfish domination by the establishment. The haunting specter of democracy losing out to "benign authoritarianism" or a "friendly fascism" becomes a growing preoccupation for Herz. He is concerned with the "power surplus" of the great powers, their short-sighted compromises with popular demands for nuclear and industrial supremacy and mankind's unwillingness to contemplate with-

out wincing the possibility of the end of civilization (he calls for reflection on the "ethics of a decent demise" or preparing ourselves for an exit from survival).

Herz's intellectual journey along the pathway of first seeking to construct a logical system of norms, next to his findings about the nature of states (their impermeability and currently their penetrability) and the complexities of nuclear foreign policies and finally to his study of the accelerating rush of mankind through a multitude of threats to survival is a chronicle of the liberal realist responding to successive crises. If he begins as a determined legal idealist, he ends as a despairing and anguished romanticist desperately clinging to Rene Dubos' axiom, "Trend is not Destiny." If the radical transformation of norms, attitudes and policies for which Herz calls is impossible, mankind may indeed have no future. It is also true that he has increased the demands he places on foreign policy and has broadened the agenda of political realism in his later writings. The respectful critic cannot but ask if Herz's despair as his lofty visions have failed is the result of his having abandoned the essence of political realism. Or does his latter-day world view with its stress on world-wide forces of decay foretell the probable end of civilization?

It may be fair to suggest that if Herz were to give his planetary humanism more concreteness, and as a liberal realist design a steady and more practical incremental course for individuals and nations to follow in feeding themselves, holding down population growth and keeping environmental deterioration in check, we might even at this late hour understand more clearly how to move from where we are to where he would have us go.

KENNETH W. THOMPSON

*University of Virginia*

**The United Nations System: Coordinating Its Economic and Social Work.** By Martin Hill. (New York: Cambridge University Press, 1978. Pp. xv + 252. \$31.50.)

For the past 30 years, the economic and social program of the United Nations has gradually expanded in size, complexity and diversity. Agencies have proliferated and have often been granted a degree of autonomy which provides immunity from effective central control. The problem of coordination has been compounded, not only by the autonomy and proliferation of new global agencies and special funds, but also by the necessity to work with



and through regional organizations and country programs. The Economic and Social Council has been poorly equipped as a central coordinating body because of size, lack of authority, and faulty organization. Attempts to fill the void and to bring coherence to the system through administrative consultations and committees have provided only partial solutions to the myriad problems of coordination in a decentralized and fragmented system.

Since 1965 the General Assembly has taken the lead in instigating studies for the purpose of restoring a degree of coherence and unity to the economic and social aspects of the programs of the United Nations family of agencies. By 1969, two reports, one by an Enlarged Committee for Programme and Coordination of the Economic and Social Council, and a Capacity Study of the United Nations Development Program, provided recommendations for the first modest steps in restoring a degree of order out of the existing chaos. In the 1970s, the tempo of interest and concern quickened. The sixth and seventh special sessions of the General Assembly and the pressure for a New International Economic Order focused attention on both the needs of the developing countries and the inadequacies of the United Nations' machinery and programs. In 1974-75, the Economic and Social Council received observations and recommendations from the Secretary-General and the executive heads of the specialized agencies concerning more effective coordination of their programs.

This book by Martin Hill is a final version of a study prepared under the auspices of the United Nations Institute for Training and Research (UNITAR). The preliminary study was submitted to the Economic and Social Council in 1974 and was made available to a committee of 25 experts who, in 1975, produced a report entitled, "A New United Nations Structure for Global Economic Cooperation." This report is the most significant contribution to reform of the UN economic and social system since the charter was written. It proposes the reorganization of the program of the Economic and Social Council, the consolidation or abolition of several committees and agencies, and the creation of a new post of Director-General for Development and International Economic Cooperation.

Hill's study is a valuable companion to the committee's report. As a member of the Secretariats of the League of Nations and United Nations since 1927, he was at the heart of many of the problems of economic and social coordination which he analyzes. Before retirement, he was Assistant Secretary-General for

Inter-Agency Affairs. He writes from a depth of experience and appreciation of needs and problems.

In his postscript, Hill compares his study with the report of the committee of experts. While many of the conclusions are similar, Hill's study is more limited in scope and deals more with attitudes and practices rather than with specific recommendations for structural changes. Hill constantly stresses leadership and personal relationships as keys to effective coordination. His book also is rich in historical development of the system. Of the six appendices, the most valuable is a summary of the conclusions and recommendations of the committee.

The study is thorough and accurate, but, because of organization, often redundant. The style is heavy and sentences are frequently convoluted. However, for the student of international organization, the book represents the most comprehensive analysis to date, from the perspective of a knowledgeable international civil servant, of the key area of coordination of the United Nations economic and social activities.

A. LEROY BENNETT

*University of Delaware*

#### **Social Change in the Capitalist World Economy.**

Edited by Barbara Hockey Kaplan. (Beverly Hills, Calif.: Sage Publications, 1978. Pp. 239. \$18.50, cloth; \$7.95, paper.)

This collection extends the socialist analysis of the political economy of the world system. The major text is Immanuel Wallerstein's *The Modern World System: Capitalist Agriculture and the Origins of the European World Economy in the Sixteenth Century* (Academic Press, 1974). Related work continues in the journal *Review*, in the *Political Economy of the World System Newsletter*, in the present volume's forthcoming sequel: *The World-System of Capitalism*, edited by Walter Goldfrank, and in Immanuel Wallerstein's *The Modern World System*, Vol. 2, as well as his *The Capitalist World-Economy*, both forthcoming in 1979.

This analysis deserves the political science community's closest attention. It fits the larger intellectual move of unseating the sovereign subject or disaggregated particular instance, by recognizing the overarching centrality of *structure* or *system* or *code*. In studying development or comparative and international politics, beneath the level of appearances lie the struc-

tured relations of a capitalist world system, often operating behind the backs of the states as social actors.

Models of accumulation need to embrace the world system in which capitalism has historically developed, and which has created an international stratification of political units into core, peripheral, and semi-peripheral states. As an interdependent and self-contained system, its dynamics are internal, grounded in a complete economic division of labor. Differentiation is the leitmotif: polarization, spatial hierarchization, unequal exchange, unequal distribution of certain types of economic activities and state structures, class formation at the global level, etc.

Market relationships constitute a central dynamic. By creating risks and opportunities, they organize decision-making criteria for economic and (therefore?) state actors. Position within the world economy is hypothesized as determining the distribution of competitive advantage in production, the pattern of economic differentiation, class relations, form of labor control, flow of the surplus, and strength of the individual administrative apparatuses of states.

Familiar shibboleths of political science are attacked. First, nation-states no longer appear as autonomous, self-contained systems, nor as entirely appropriate units of analysis. They are not separate systems of economic production but parts of a single system of production containing multiple political sovereignties. Second, older "ontogenetic" theories of development seem beside the point. Not all countries are capable of following some single path from underdevelopment to self-reliant independent development, no matter what external obstacles to growth are removed. (Notice how this view today inspires the reformism of proposals for a New International Economic Order.) Figure becomes ground; what once seemed like external obstacles now becomes the constitutive system itself.

The ten articles included here extend, systematize, and abridge such a conception. Some of the questions raised concern alternative conceptions of the state, the role of domestic political coalitions, the sources of fascism, of revolution, of counter-revolution, and of modes of core/periphery relationships.

For example: Skocpol and Trimberger, in a comparative study of revolutions, stress the need to complement a world system analysis with comparative historical studies of class relations (for example, the role of the peasantry), the relative autonomy of states (which

have a logic of their own), and the international military rivalries among states. International pressures may determine the outcomes of revolutions which bring about more centralized and bureaucratized state structures which in turn give societies a stronger position from which to cope with the international pressures.

Richard Robinson asks what allows states to move into the core (and achieve major national economic transformation). Along with more and more scholars, he looks at possibilities for building specific political coalitions that could back up or "constitute" such policies. In a careful analysis of Germany and the United States from 1840-1880, he *internationalizes* the stress on political coalitions. Such political opportunities are not merely domestic products; they are a function of conditions in the world economy and the place of a nation within that system.

By stressing the autonomous concern of state actors for preserving attractive investment conditions ("business confidence"), we can drop assumptions about the intentionality of a dominant and unified class. Fred Block extends this theory of the state, but with an added twist: making claims for the self-interest of state actors, resulting even in odd similarities with bureaucratic models. Here, alternative characterizations of the state shed light on the debates about inter-imperialist rivalry (which sometimes hide under the topic of "coordinating the foreign economic policies of advanced industrialized states"). Block's stress on state managers' self-interest best explains conflict resulting from pushes for renegotiating the international rules of the game.

Like innovative structuralisms in other disciplines, these analyses have their limitations. Dotting these conceptions are methodological land mines. We are fruitfully led to ask questions about synchronic analysis, about the stress on exchange relations rather than productive relations, about the theory's ability to account for historical change, in a systematic way, about the neglect of the relations *between* classes domestically and their sources (since institutionalized patterns do not arise, teleologically, "in order to" maximize market opportunities). As usual, to the global "syntactical" relations, we will need to add domestically grounded "semantic" configurations. Without greater sensitivity to these domestic referents, we may be left only with a relatively abstract and static schema of grammatical imperatives. (This has also been a failing of much of the early work on international "regimes" and "regime change.") Even so, from

now on, substantial intellectual achievements—in, for example, international political economy, development, or comparative politics—will likely be “post-structuralist.” We have come this far.

BRUCE ANDREWS

*Fordham University*

**Economic Issues and National Security.** Edited by Klaus Knorr and Frank Traeger. (Lawrence: Regents Press of Kansas, 1977. Pp. vii + 330. \$7.50, paper.)

This interesting new book poses the question: “Will interdependence lead to conflict?” and answers it essentially in the affirmative. The argument subscribed to by most of the contributors is stated in the first chapter by Klaus Knorr. He notes that economic interdependence entails vulnerability of national economies to foreign influences and forces. These influences have required and will continue to require painful adjustments on the part of labor, consumers and businesses. But for a variety of reasons, including the weakening of the Protestant ethic, the electorates in democratic industrialized societies have been reluctant to change their life styles or spending habits at the behest of foreigners. Rather, they have tended to demand that governments solve their problems for them, and if necessary at the expense of foreign nations and exporters. This could lead to policies designed to improve domestic economic conditions “at the expense of employment and price stability in the outside world.”

The international dimensions of the problem become more troublesome in the light of the continued pressure on domestic governments to provide greater social services and benefits to their constituents. Indeed, this pressure will likely lead to greater public control of economic life and to expanding governmental activities in all industrial countries. Negotiations with the developing world may also operate in the same direction, with governments forced to control commodity prices or energy supplies to deal effectively with outside powers. Ideally, the clash between what domestic electorates will accept and what foreign interdependence seems to thrust upon them should lead to new international and multilateral solutions, with each government partly modifying its claims in response to international considerations. Rather, the contributors argue, weak governments cannot afford to

concede to international pressures and may be forced willy-nilly into conflict with one another. Robert Gilpin ably abets this thesis by noting that mercantilism never was entirely superseded by Smithian liberalism and that economic conflict may ultimately be the carrying on of Clausewitzian policy by other means. A number of contributors observe that a “repoliticization” of domestic economic issues may transform traditionally low-politics interdependence issues into questions of high politics.

There are some descants to the dominant melody. Clark Murdoch's chapter presents a useful compendium of measures of domestic economic vulnerability. Two opposed conclusions might plausibly be derived from such data: (1) that states cannot reduce the vulnerability which they currently face; (2) that their very vulnerability will make conflict between them more likely. Ronald Meltzer's chapter concludes that multilateral solutions to the problem of vulnerability and its domestic impact are much preferable to national ones. But he, like others, does not consider them very likely. In a particularly interesting chapter, and one reminiscent of Robert Gilpin's contentions, Cheryl Christensen concludes that the possession of “structural power” has been particularly important to the stability of international political and economic relations. In the past, Britain, and later the United States, exercised structural power (power which changes rules and regimes, which sets the context for future action). Today, on the other hand, not only has U.S. power declined relative to that of other democratic industrial countries, but also new centers of powers, like OPEC, have emerged to challenge the ascendancy of OECD. And there are others in the wings, particularly the Group of 77. Indeed, the demands for a New International Economic Order may make it impossible for OECD and OPEC to reach agreement on their own.

One can certainly accept that the potential for economic conflict and even economic warfare is greater than it has been in the recent past. The question becomes, however, what the longer-term prospects hold. In the past a weak Japanese government, faced with political and military pressures to protect its position in China, attacked the United States and South-east Asia in hopes of solving its short-term problems. But little thought was given to the long run: could Japan win the war? In somewhat the same way weak governments today may wish to be able to cope with the pressures upon them and to reduce international and

transnational influences, but they may realistically be unable to do so. Murdoch's quantitative conclusions point toward continuing vulnerability. Of course it is possible to cut off one's nose to spite one's face (and that is what Japan did), but almost all of the national measures which would improve one's trade or monetary position have international counterparts that reassert the status quo ante with higher costs to trade on both sides. Purely national solutions, then, are not solutions at all. Does that mean then that the vaunted multilateral remedy is more likely than presumed? Probably not, because, as the authors rightly observe, it has been difficult to find a way to share the costs and risks among industrial nations.

What the contributors have failed to do, however, is to consider the realistic alternatives between economic nationalism on the one hand, and a new multilateralism on the other, for that has been the name of the game. What emerges are neither solutions nor trade wars, but informal linkages and rescue operations, where one government helps out another in trouble. The monetary field has seen such partial remedies for more than a decade. Some regional economic nationalism is not all bad, if that is the term we would apply to the relatively high common European tariff. It was, after all, the threatened imposition of this tariff that led to the huge upsurge of international investment in Europe in the 1960s. If the United States now moved to informal controls on Japanese or European exports, the solution would once again be to overleap the tariff with international production and investment, benefiting American consumers and labor.

In talking of greater economic nationalism the contributors have tended to slight the domestic constituencies which strongly support interdependence and an open-trading network, and here one speaks not just of the export industries, but also financial interests, and the multinationals who do not want to bring their capital home. To this point, their argument, not that of George Meany, has prevailed with Congress, and the foreign tax credit looks as solid as it ever did. American business does not want to be hampered in its ability to compete with others overseas, and its position has won support.

It is also arguable that the contributors have got their case wrong, at least as it applies to the United States (and the United States is the only democratic industrial country which could afford to tamper with its foreign trade dependence). Knorr and his colleagues point to a

"repoliticization" of economic issues and an expanding government activity in private markets as the trend of the future. In fact, may it not be the other way around? "Reprivatization" may be more characteristic than "politicization," and we may find the government moving out of areas of traditional investment and expenditure. It may move away from regulation as well. If tax-reduction forces have their way (and perhaps even if they don't) we may find greater use of monetary as opposed to fiscal instruments, and perhaps a lower government deficit. This could actually make for higher growth, lower inflation, and more stability in the balance of payments. The impacts of transnational interdependence would then not be so divisive domestically, nor would they militate against the demands of the citizenry.

In the 1930s a number of countries could contemplate cutting their links to the international economy and risk an attempt at autarchy. The great imperial edifices of France and Britain mobilized great resources and markets. Japan needed only Southeast Asia as a substitute for her links with the United States. Germany could move to the Ukraine and to the oil fields of the east and south, thereby regaining a temporary autonomy. But such efforts depended upon low domestic growth and dormant international trade. It is not clear today how nations (with the possible exception of the Soviet Union and China) could hope to provide the same insulation from the international economy that a number of them temporarily achieved 40 years ago. Thus the question is not whether international interdependence may not lead to conflict; but "who wants to fight a war that he knows he cannot win?"

RICHARD ROSECRANCE

*Cornell University*

**The Panama Canal: The Crisis in Historical Perspective.** By Walter LaFeber. (New York: Oxford University Press, 1978. Pp. xii + 248. \$10.95, cloth; \$3.95, paper.)

The first words in this book are: "In January 1964, four United States soldiers died, 85 others were wounded, and 24 Panamanians were killed with nearly 200 wounded during four days of warfare between the troops and Panamanian mobs. Most North Americans soon forgot about the killings. Vietnam, inflation, and Watergate pre-empted headlines for the next ten years. But Panamanians did not forget" (p. vii). Walter LaFeber then provides

us with a well-written, comprehensive analysis of U.S.-Panamanian relations regarding the canal and its surrounding zone. The book, appropriately published while congressional debates over new treaty arrangements were occurring, represents, in the author's words, "a survey." The book serves as a useful guide, however, through the historical ups and downs of the U.S.-Panamanian relationship.

The book is divided by chapters representing the separate diplomatic phases through which construction and possession of the canal have evolved. LaFeber's emphasis is immediately apparent by his chapter titles that refer to the names of the primary negotiators: "Balboa, DeLesseps, Cromwell," "Roosevelt, Bunau-Varilla, and Taft," "Wilson, Arias, and Roosevelt," "Arias, Remon, and Eisenhower," "Chirari, Johnson and Robles," "Torrijos, Kissinger, and Carter." Much the story told here centers around their political machinations. It is a tale of duplicity, misperception and self-aggrandizement.

The moving spirits who directed the American dream of building a canal between the Caribbean Sea and the Pacific Ocean from Nicaragua to the Panamanian isthmus were primarily self-interested. The three were primarily William Nelson Cromwell, founder of the prestigious Wall Street law firm, Sullivan and Cromwell. Another key lobbyist for the Panamanian Canal was the famous entrepreneur, Senator Mark Hanna. Perhaps the most important of the Panama Canal founders was the corrupt Buneau-Varilla. He represented Panamanian interests during the treaty negotiations leading to the notorious 1903 agreement which leased in perpetuity control over the canal and its zone to the United States "as if it were sovereign." One minor detail was that Buneau-Varilla had nothing less than 40 million dollars to gain from the deal which was almost immediately rejected by the newly established Panamanian government.

LaFeber shows how the terms of the 1903 treaty forever affected Panamanian perceptions of the United States which no amount of technological contribution, administrative expertise or financial largesse could or ever did alter. Throughout all the phases during which the terms of the 1903 treaty were modified—consistently in favor of Panama—there lingered among Panamanians the belief that the United States had cheated them of their national heritage. As this study reveals, Panamanian nationalism is directly linked to this conviction. LaFeber is especially sensitive to the realities of Panamanian life, its racism, class structure and

political dynamics, particularly as they impinged upon the negotiations affecting the economics of the zone and the life of American residents of the zone. The American presence tended to exacerbate these tendencies and also to inflame the nationalist desire to be rid of the North Americans. He writes, for example, "Eisenhower's enlightened policies, produced a tragic irony. After some of the 1955 treaty promises were made real, Panamanian workers in the Zone received higher wages; merchants outside the Zone suddenly could sell goods to the North Americans. The Zone provided the major impetus to the country's economy in the late 1950s. Panamanians therefore grew more dependent on the Zone for their own economic survival. Granting Panama concessions accelerated the country's need to break the colonial relationship and control the entire Canal area, or else face economic stagnation" (p. 130).

In the end, LaFeber's study stands as a justification for the 1978 Torrijos-Carter Treaties. The basic fact is that the canal, at a time of transcontinental transportation, has become increasingly less significant economically for the United States. Its major users today are the Central American states themselves. As LaFeber points out, virtually "10 to 17% of the United States ocean-going commerce, representing less than 1% of the gross national product, uses the Canal" (p. 223). Moreover, in an era of a two-ocean U.S. Navy, the utility of the canal to the defense and security of the United States is marginal. U.S. nuclear submarines, for example, have to surface in order to pass through the canal, diminishing the strategic value of such passage. Several major classes of ships, aircraft carriers, moreover, are too big for the canal. LaFeber describes the machinery of the canal as a "superb antique."

The time had come for the United States and Panama to establish a mature relationship which duly recognized Panamanian sovereignty over the canal and left the Panamanians to their destiny as keepers of the gates and masters of their own fate.

LaFeber includes an excellent bibliographical essay at the end of the book.

EDWARD WEISBAND

*State University of New York, Binghamton*

**Soviet-American Rivalry: An Expert Analysis of the Economic, Political, and Military Competition Which Dominates Foreign Relations.** By Thomas B. Larson. (New York: W. W. Norton, 1978. Pp. xii + 308. \$13.95.)

This systematic survey of the Soviet and U.S. political systems should be required reading for every legislator, citizen, and student concerned with world affairs. Written by a former State Department analyst, *Soviet-American Rivalry* examines the strengths and weaknesses, the rhetoric and the behavior of both superpowers, at home and abroad, focusing on the years 1945–1975.

Manageable in size and smoothly written, Larson's book is more readily digested than Paul Hollander's much longer *Soviet and American Society*; it is more up-to-date and less freighted with detail than Brzezinski and Huntington, *Political Power: USA/USSR*, published in 1963.

Though Larson does not explicitly address the convergence thesis (rebutted in Brzezinski and Huntington), he too finds both Soviet and U.S. societies listening to their own drummers; nor does he offer solace to those who hope detente will come to overshadow conflict in U.S.-Soviet relations.

Though the book is well balanced and based on the most reliable and relevant data, Larson's interpretations often tilt to Moscow's polemical advantage. Item: he extols the theoretical potential of collectivism in agriculture, while saying little about the practical contributions of private farming to Soviet production. Item: he praises the narrowing of income differentials in the USSR, but says little about the material perquisites of elite party members and others, and still less about the rise of a black middle class in the United States. Item: he says little about ethnic problems or declining birth rates within the USSR, and less about the possibilities of centrifugal movements within Eastern Europe. Indeed, his survey of accomplishments by both superpowers slights U.S. leadership in culture and other spheres related to the quality of life, e.g., environmental protection.

These shortcomings, however, are minor compared to the book's common sense appraisal of the military and economic balance sheet, an arena often dominated in many U.S. writings by paranoia or overconfidence. The specialist may well prefer the detailed Brzezinski-Huntington case studies and their stimulating theses, but Larson's book meets a real need as Americans debate how to respond to a mounting Soviet challenge.

Any survey of this kind will trigger disagreements, but this characteristic enhances Larson's book's potential for classroom use, not just in international or Soviet studies but as an essential element in any social science course about the modern world.

WALTER C. CLEMENS, JR.

*Boston University*

**Dragon and Eagle: United States-China Relations: Past and Future.** Edited by Michel Oksenberg and Robert B. Oxnam. (New York: Basic Books, 1978. Pp. xvi + 384. \$13.50.)

This interesting and timely volume has emerged from a 1975–1978 project of the China Council of the Asia Society entitled "Sino-American Relations in Historical Perspective." It is the product of a joint endeavor of 13 China specialists, including the two editors: Michel Oksenberg, Chief China Advisor to the National Security Council, and Robert Oxnam, Program Director of the Asia Society's China Council.

The stated purpose of the book is not to advocate a particular policy but to suggest "a new way of thinking about the relations between the world's most populous society and the world's most affluent society." Their relationship is to be examined in both historical and long-term perspectives. It is within this conceptual framework that the various essays of the volume are organized.

Immediately following the editors' introduction is the section on "mutual perceptions." In the two essays written by Warren Cohen and Tu Wei-ming, attention is given to how Americans and Chinese perceive each other and how their perceptions and images have changed over time in relation to ideologies, politics, and emotions in both countries. The next section deals with the issue of "bilateral interactions." Oksenberg, Lyman Van Slyke, Waldo Heinrichs, and Stanley Lubman examine the various modes of interaction—diplomacy, culture and technology, force, and trade—that have shaped the bilateral relationship between the United States and China.

In the section on "multilateral interactions," the main focus is on the complex quadrangular relationship of the United States, China, Japan, and the Soviet Union. The essays contributed by Allen Whiting and Steven Levine analyze the impact of China's two most important neighbors, Japan and the Soviet Union, upon Sino-

American relations. "Interactions around China's rim" is the topic of the following section. In their respective articles, Peter van Ness, Michael Hunt, and Alexander Woodside discuss Taiwan, Korea, and Southeast Asia as related to American and Chinese interests. The final section of the book is entitled "the legal context." Jerome Cohen's essay shows how international law has been and will continue to be useful in managing relations between China and the United States, whether in cooperation or contention.

By and large, the essays are of high quality and follow a common format. Several of them, notably those by J. Cohen and Levine, are particularly stimulating and perceptive. For a book of this type, however, there are inevitable problems of overlapping and unevenness. Because of the volume's modest size, some issues are treated rather too briefly to do them justice. This is somewhat compensated for by the inclusion of a useful bibliography to help the reader in pursuing topics at greater depth.

Throughout the book there is a general theme calling for the reversal of the past pattern of misunderstanding and confrontation and the initiation of a new era of reconciliation and cooperation between the United States and China. Unquestionably, the Soviet factor has loomed large and will remain so in Sino-American relations. But, as Oxnam rightfully points out, the "mutually perceived enemy" approach cannot be a solid basis for long-term relationships between the two countries. Instead, "we need to look beyond to other powerful potentials of the Peking-Washington connection—greater economic and diplomatic cooperation among the great powers in Asia; . . . joint U.S.-China involvement in multilateral negotiations on arms control, energy resources, food supply, and the international economic system; and bilateral trade and cultural and scientific exchanges" (p. 44).

Over the emotion-packed issue of Taiwan, the book offers no explicit suggestion for its resolution. By implication, the authors who address themselves to this issue appear to be willing to see Washington accept Peking's three conditions for normalization of relations (severing U.S. diplomatic ties with Taipei, withdrawing all American military personnel from Taiwan, and abrogating the Mutual Defense Treaty with the Nationalist government), while retaining economic and cultural ties with Taiwan in the hope that Taiwan's own military strength and the continuing American and Japanese interests there would deter the PRC from using force against the island.

Whether or not one subscribes to this specific policy stand, *Dragon and Eagle* on the whole is a valuable contribution to the studies of Sino-American relations and deserves serious attention from scholars as well as the general public.

SHAO-CHUAN LENG

*University of Virginia*

**Scarcity, Choice, and Public Policy in Middle Africa.** By Donald Rothchild and Robert L. Curry, Jr. (Berkeley: University of California Press, 1978. Pp. viii + 357. \$15.75, cloth; \$4.95, paper.)

This readable and important study generalizes and advises about economic policy making in what the authors call "Middle Africa." Donald Rothchild (a political scientist) and Robert Curry (an economist) focus on the over 30 states lying between the Arab countries to the north and the continent's white-ruled southern tip.

The book's virtues are many. The authors write for the generalist as well as the specialist, restricting technical economic analysis to appendices following individual chapters. Their arguments are richly referenced, with a near-encyclopedic coverage of the relevant literature on African political economy. Their normative point of view includes informed sympathy for the region's problems, optimism about its potentialities, and yet a pragmatic appreciation of the constraints facing African policy makers. The authors wisely concentrate on a limited number of policy areas—those regarding foreign private investment, external trade, and foreign aid—rather than attempting in one modest-sized volume to analyze and prescribe for domestic economic policy as well as for foreign. However, they might have narrowed their range even further, trading off some loss of scope for greater depth of analysis. The occasional tendency toward superficial treatment is particularly conspicuous following the authors' call, early in the book, for a benefit-cost approach to policy making, based on careful assessment of alternatives.

Rothchild and Curry advocate a "political economy framework" for policy choice. By combining political science and economics, this approach is supposed to inform policy makers as to what is socially desired, economically rational, and administratively feasible, as well as how to assess trade-offs among these values. Theirs is an explicitly "rational-choice" approach, which rejects both incrementalism and

any determinism that posits an end-state for African development (e.g., Western capitalism or Marxist socialism). They call, instead, for experimentation which is bold but not naive; they challenge African elites to set "realizable goals" reached through cost-minimizing policies.

The book argues that African states have tended to follow one of three general "strategies of choice" in foreign economic policy. The largest group of states—consisting predominantly of poor, French-speaking countries—is classified as "accommodationist" in that they accept largely intact the foreign economic relations they inherited from the colonial period: openness to new foreign private investment, few restrictions on old or new expatriate enterprise, and reliance on the ex-metropole and its Common Market partners for most trade and aid. Another, smaller group (Ghana, Kenya, Nigeria, Zaire and Zambia) also "accept[s] their structural dependency on the Western capitalist economy" (p. 118). But they seek to reform that relationship, such as through more strictly regulating and taxing foreign enterprises and nationalizing certain sectors or subsectors.

Rothchild's and Curry's third group of Middle African states (Angola, Guinea, Guinea-Bissau, Mozambique, Somalia, and Tanzania) bears the label "transformationist" because they seek "to end their dependence on the Western-dominated economic system" (p. 135). They nationalize and exclude foreign enterprise and work to reduce reliance on foreign aid. Moreover, they are willing to trade off short-term losses in production for the benefits of greater economic independence. Conversely, the other two groups of states have supposedly been sacrificing some meaningful degree of decisional autonomy in order to reap "productionist" benefits (e.g., export industries which expand thanks to foreign private capital and skills). However, the book provides too little evidence of where real gains or losses have occurred in decision-making independence and what their empirical consequences have been. Has skills-scarce Guinea become significantly less dependent on foreign enterprise (private and public)? If so, what net production, legitimacy, and other benefits have resulted? Has Tanzania become any less dependent on foreign aid, or is it that the sources of such aid have been more diverse?

I would hope for more case-study research into the impacts of various degrees of dependence. Such research could assist also in testing the applicability of Rothchild's and Curry's

major policy recommendations regarding investment and aid. African states are urged to develop a common set of conditions on which foreign investment would be welcome, "thereby insuring that each government could undertake to protect its national interests without being played off against other governments" (p. 174). However, if no state is permitted to outbid others in fiscal and other incentives, investment would flow largely or entirely according to criteria on which some countries would be permanently disadvantaged: natural resources endowment, market size, and skills level of labor force, among others. All states would protect their virtue, but some would receive little if any new capital and technology unless, as the authors suggest, the same African investment compact could somehow compel multinational corporations to disperse their investments to the benefit of the disadvantaged.

Rothchild's and Curry's chief recommendation on foreign aid also needs further elaboration before being persuasive. They urge the channeling of aid through multilateral organizations which, supposedly, will provide assistance that meets their definition of appropriate aid: "A concession . . . unencumbered by arbitrary restrictions . . . which fulfills the development objective set by decision-makers in the poorer country" (p. 278). However, in the African experience, has United Nations, World Bank, and other multilateral assistance tended to be superior to bilateral aid? Have not donor restrictions at times been useful correctives to the inexperience and/or opportunism of LDC elites?

The book's treatment of foreign trade policy is more thorough and persuasive. The authors present cogent briefs against the feasibility of both regional common markets and non-petroleum commodity cartels and for the viability of common-front trade barrier negotiations with developed market economies.

Whatever may or may not be the deficiencies of its specific arguments, the book's general approach to African economic policy is valuable: largely non-ideological and stressing the assessment of benefits, costs, and trade-offs.

J. D. ESSEKS

*Northern Illinois University*



**Understanding Conflict and War, Vol. 2: The Conflict Helix.** By R. J. Rummel. (Beverly Hills, Calif.: Halsted Press, 1976. Pp. 400. \$17.50.)

A general understanding of manifest, and particularly violent, conflict at the societal level has been much sought by social scientists. In this, the second of an intended five-volume series, R. J. Rummel attempts to provide a more valid and more satisfactory general understanding than has heretofore been offered. In my judgment, he is not successful. On the contrary, in 375 pages of rather laborious reading, he provides very little in the form of new insights or substantive theory and a great deal of conceptual obfuscation. This is not to say that Rummel's argument is lacking in substance and import. There is much here that is pithy. Unfortunately, it does not add up to a sound general understanding of manifest social conflict. What does this volume provide, and why does it fall short of its goal?

In a nutshell, Rummel's argument is that individuals' interests and expectations are the bases of manifest conflict. Interests and expectations are rooted in and largely derived from a particular sociocultural context of meanings, norms, statuses, classes, and powers, which together define latent conflict structures and individuals' perceptions of situations therein. Latent structures of conflict are socially universal because of differences among individuals and because of the existence of organizations (called "anti-fields") among individuals. But not all latent structures of conflict are expressed as manifest conflict. They become manifest, according to Rummel, as individuals become *aware* of their opposing interests and attempt to *create a new balance* of social powers and expectations because of some sudden change in social relationships or circumstances. To understand manifest conflict in general, then, is to understand the social-psychological connection between changing structural elements of sociocultural context and the interests and expectations of individuals within that changing context.

So far, so good. Indeed, as a basic perspective this is an argument far superior to many popular conflict theories. While the latter tend to be simplistic in being based on one or another single causal factor, whether psychological or social-structural, and mechanical in postulating that manifest conflict is caused by certain conditions regardless of societal context, Rummel's argument, by contrast, is neither. It is contextually grounded and integrated

across individual and social-structural levels. If the theory incipient in this core argument were adequately developed, knowledge of manifest social conflict would be substantially advanced. And Rummel sets himself on a path to its adequate development. He attempts the construction of an hypothetico-deductive system beginning with the elaboration of basic psychological concepts and progressing through the conceptualization of the elements and structure of sociocultural systems. In this, he contributes some important conceptual/theoretical material. In particular, chapter 17 on social status differences and chapters 26 through 28 on conflict are quite good and well worth reading. In chapter 17, Rummel presents and develops the analytical implications of an argument very important to explanations of conflict based on social inequality:

The difference in status between two people therefore cannot be an absolute measurement, like the miles between two landmarks. Rather, two people differ by virtue of not only their own wealth and power, but also *as a function of the distribution of these statuses in society*. We cannot gauge the similarity and differences without considering the standards, desires, distribution of resources of society as a whole. . . . Man's location in this social space, his status position, is thus relative to societal context, standards, and desires (p. 138).

In chapters 26 through 28, he presents a conceptualization of conflict—as a structure of opposing dispositions, a situation of opposing interests, and a manifestation of a balancing of powers—that is potentially important to all students of that phenomenon.

But not so much can be said for other parts of the book. The first two-thirds (25 chapters) represent Rummel's efforts to lay out the bases for a general theory of human behavior. Here are presented the premises and conceptions out of which Rummel's arguments about violent societal conflict are purportedly developed in the last third of the book (10 chapters). Unfortunately the lengthy presentation of theoretical building blocks is of little positive consequence as a whole. On the contrary, the reader is beset by terminological overkill, is subjected to a pretense of rigor and precision, and confronts a perspective based on nineteenth-century Aristotelian vitalist philosophical assumptions. The following passage is typical of the material through which the reader must struggle in attempting to come to grips with the purported bases of Rummel's conflict theory:

Power is first an ontological concept basic to understanding a reality of potentialities and actualities. Our world is a flux of possibilities becoming actual, dispositions becoming manifest, determinables becoming specific. Reality is not only being, but becoming. It is active, a field of potentialities and tendencies struggling against each other to become actual, completed, determinate. . . . What is power within this view? First, power is the linkage between different states of being; between potentialities and actualities, between dispositions and manifestations, and between determinables and determinants (specifics). Second, that linkage is a strength-of-becoming, an active will-to-completeness. It is a push from the level of pure potentiality, of mere possibility, to ever greater levels of clarity and definiteness. Third, it is an imminent energy, an inherent force-towards-identity of all beings. . . . Finally, in essence, power is a *vector-towards-manifestation* (p. 165).

This quotation does more than I could possibly do in a short review to convey the nature and character of the first two-thirds of the book.

And sadly, the aggregation of such passages does not really constitute a solid basis for Rummel's arguments about violent conflict. That is partly owing to the fact that the attempted comprehensiveness and precision of the system of basic conceptions and premises is not accomplished. Instead, the composite impact is of an understandably contorted and sometimes inconsistent admixture of Aristotle, Kant, Schutz, Lewin, Sorokin, and Hegel! But the failure of the first part to provide a solid basis for the second part is due also to the fact that the argument about violent conflict is hardly new or systematic. Rather than drawing directly on the elaborate theoretical structure that precedes, it consists of seven fairly standard hypotheses:

- (1) Conflict manifestations are random regarding specific societal structures of expectations.
- (2) Change produces societal conflict.
- (3) Power shapes conflict.
- (4) Overt conflict has three manifest dimensions.
- (5) Exchange societies manifest pluralistic conflict.
- (6) Authoritative societies manifest communal/traditional conflict.
- (7) Coercive societies manifest elite repression/purges (p. 321).

Such do not require 375 pages of laborious reading to develop!

In all, then, this volume consists of a grand and elaborate theoretical structure which does not produce new or compelling claims about

manifest conflict. Rather, it is a pretentious presentation that provides, at most, an interpretive basis for a few common-sense hypotheses which are, themselves, only weakly "tested" in the concluding pages of the volume. The book is not a model of social theory development, nor is it an important contribution to a general understanding of manifest social conflict.

RAYMOND D. DUVAL

*University of Minnesota*

**Superships and Nation-States: The Transnational Policies of the Intergovernmental Maritime Consultative Organization.** By Harvey B. Silverstein. (Boulder, Colo.: Westview Press, 1978. Pp. xiii + 251. \$17.50.)

This book analyzes the organization, decision making and impact of the International Maritime Consultative Organization (IMCO) on international affairs. Participating nations are listed, their ocean-related capabilities are explained, and their negotiating activity is weighed against their capacity for influence in and outside the organization.

The volume is well organized, logical and cohesive. It offers a unique perspective to students of international organization because it employs statistical techniques in a matter-of-fact way without stumbling over theories of world order or paradigms of vote counting. Rather, this is a good old-fashioned case study in modern form that tells us what, how and why this particular organization operates. It is no surprise to learn that the nations with the greatest fiscal interest in shipping lead the organization. After all we have heard about the Third World in the past decade, it is relief to learn that some elements of national power still count for something.

This book will be particularly useful for the policy maker who wants specific questions analyzed and who has little time for seeking answers. It will be less helpful to the theorist because, notwithstanding the author's assurances, there is thankfully little space devoted to integrating the conclusions into the proverbial vast body of wider knowledge. What the book does admirably is answer serious questions about how one important technology-oriented body operates; and in so doing, tells much about the relative importance of technology in international decision making.

Given the importance of the nations with greater financial and tonnage interests in shipping, more attention might have been devoted

to the impact of rate-setting conferences on decisions taken by IMCO. Silverstein places the conferences on a line with other organizations—G.O. and N.G.O.—that link to and influence IMCO, but the suspicion remains that private decisions taken by conferences are more important than Silverstein's coding of interactions reveals. Perhaps influence is all the same no matter where it is exercised.

Silverstein makes the most interesting observation that important IMCO decisions are made by specialized personnel with particular interests in shipping and not by traditional foreign offices. On top of this, the very dynamics of the organization itself count for something. Still, these are seen as less salient than the influence of the dominant members. One wonders whether or not the degree of professionalism that Silverstein found in IMCO accounts for the relative good this body has accomplished in serving international shipping as opposed to those UN bodies which seem to run more on radical rhetoric than common sense.

Something must be said about the physical nature of the volume. How sad it is that a highly trained but apparently impoverished Academe is grateful to be published this way. The book is a do-it-yourself, photo-reproduced job which just looks sloppy. Many of Silverstein's charts lose their informational value because they were photographed from a typed page instead of being professionally drafted. Moreover, the price is outrageous considering the poor quality of reproduction and the uneven margins! The publisher, of course, will protest that the cost of publication is high and the sales potential of scholarly works is low. If that is true, perhaps scholars should do some serious reflecting on their attitudes toward publication and its vaunted place in achieving recognition and advancement. What, in fact, is being rewarded: scholarly merit or marginal fiscal approval granted by publishers?

Silverstein has achieved something worthwhile, however; he has clearly described an international organization and explained its actions. Aside from the professional necessity of excessive citation, this is a lean, well-written and highly informative book. When you have finished reading it you will know more about how functional international organizations work and that is worth a lot.

DONALD E. MILSTEN

*Maryland Energy Policy Office*

**The Limits of Military Intervention.** Edited by Ellen P. Stern. (Beverly Hills, Calif.: Sage Publications, 1977. Pp. 399. \$25.00, cloth; \$8.95, paper.)

The twelfth volume in the Sage Series on Armed Forces and Society, this is a work prepared with the exquisite care taken by Field Marshall Montgomery before Alemeine. First, there was a planning conference to outline issues; second, a conference to receive and debate papers; third, a seminar to evaluate the papers once again. From this elaborate procedure emerge 18 chapters, mostly by political scientists who address an issue of the day in the language of the hour (e.g., "players" handle "options").

The impression that this is a period piece of American social science is strengthened by the scope and focus of the book. With a few notable exceptions, the time reference "is to the immediate present and the years ahead" (p. 9). This eliminates France's role in the American revolution, Italy's in the Spanish Civil War, and America's in the Chinese Civil War. Absent are the rich historical and comparative data encountered in Walter Lacquer's recent work on guerilla warfare (*Guerilla: A Historical and Comparative Study*, Boston: Little, Brown, 1976). Moreover, although mention is made of opportunities and rewards, the emphasis is on limits and restraints. This is defended as an analytical device but it also reflects the chastened mood of post-Vietnam citizen-researchers.

Nonetheless, the quality of individual contributions is high. Roger Hamburg writes perceptively of Soviet views about intervention and about American motives. In a concise essay on military demonstrations, Maury Feld contrasts the classical desire to show the flag with more recent impulses to conceal it behind covert operatives.

A sampling of other interesting findings follows:

1. A preference for complex and costly weapons, e.g., nuclear super-carriers, could inhibit intervention by encouraging us to keep them in central reserve—from which they would not be moved lightly. Conversely, a preference for a "high-low mix" could raise the odds of intervention by encouraging us to risk cheaper weapons in forward deployment (James Digby).

2. Some liberals fear that an all-volunteer force may encourage intervention by attracting bellicose or wholly manipulable personnel. But the AVF is certain to be costly, therefore likely to be small. The smaller the ready force, the

greater the probability that a president would have to mobilize the reserves to cope with even a modest crisis. But a decision to mobilize reserves raises such political risks that it may deter intervention in any but the gravest crisis (Thomas Fabyanic).

3. Not only are Soviet strategic forces kept at a lower state of readiness than ours (Roger Hamburg) but also Soviet military leaders, drawn mostly from ground forces, doubt the value of overseas intervention. As a result, despite recent growth, the Soviet navy lacks a distant water intervention capability (Michael McGuire).

4. In the Middle East both the United States and the Soviet Union have been relatively forbearing. Each has supported a client on the strategic defensive to the point of enabling it to restore the balance but not to the point of assisting it to a conclusive victory (Roger Hamburg).

As noted earlier, most authors are skeptical of the likelihood and utility of intervention, Richard Smoke less so, perhaps because he looks beyond great power politics. Smoke argues that the world as a whole is growing more intervention-prone, and that overt military action "generally succeeds against opposition currently in existence at the time of the interventional act" (p. 29). Thus the United States stopped North Korea before China entered the war. But Ellen Stern, the volume's able editor, states that the limitations "are increasing" (p. 24). McGuire argues that major countries have succeeded only when they exerted force in continuous areas (Cuba, Hungary) where both their incentive and power were great. Morris Janowitz asserts that "the marginal utility of the military function has declined" (p. 374), and proceeds insightfully to explore the implications for military morale and civil-military relations.

Other authors doubt the utility of military aid and covert operations. Aid, Caesar Sereseres states, cannot be counted on to enhance security, protect private investment, or promote internal stability. In the long run, American aid to Saigon probably promoted instability by decreasing our leverage for reform. Covert operations, Paul Blackstock contends, too often end by sabotaging official policy, forcing the hand of policy makers, or deluding them by generating biased reports from the field.

Implicit in most such analyses is a scenario in which a great power, usually the United States, confronts a Third World crisis. While a few writers stress technical difficulties (e.g., Third World opponents are increasingly likely

to possess precision-guided munitions or nuclear arms or both), most stress political constraints at home. Domestic priorities appear more urgent. The fear of external threat diminishes. Guilt-ridden egalitarianism replaces the cultural and religious arrogance of yesterday.

Curiously, one contributor whose evidence appears not to support this assessment is John Mueller, whose essay deals most directly with American opinion. Mueller observes that support for intervention has rebounded from the low point it reached just after Vietnam. On the other hand, his discussion of this point does not distinguish the general public from the better-educated strata from which national leaders are drawn. The better-educated are more skeptical towards intervention, just as they are more skeptical—a point Mueller *does* make—towards defense spending. Domestic political constraints, one must conclude, will remain weighty.

So will foreign political constraints. Neutrals get fussier about rights of passage and overflight. Beneficiaries and victims are less obliging than in the salad days of colonialism. Chiang, Rhee, Thieu, and Park are unlikely to be the last clients reluctant to follow our advice. Hanoi remains an inspiration for regimes more willing to accept pain than we are to inflict it. None of this proves that intervention will become a minor-league sport, but it suggests some of the inhibitions under which the great powers will labor.

LAURENCE I. RADWAY

*Dartmouth College*

**War and Domestic Political Violence: The American Capacity for Repression and Reaction.** By Michael Stohl. (Beverly Hills, Calif.: Sage Publications, 1976. Pp. 153. \$14.00, cloth; \$6.95, paper.)

This small volume—the outgrowth of a doctoral dissertation—is an empirical study of the impact of five wars on domestic violence within the United States. Michael Stohl selects a series of domestic violence indicators that reflect the two main dimensions of "issue area" and "systemic intent" and examines the trends in these variables before and after the Spanish American War, the First and Second World Wars, Korea and Vietnam. The research design follows the format of the Campbell and Stanley "interrupted time series" version of quasi-ex-

perimental designs and uses the standard single and double mood tests together with two Walker Lev tests to determine whether or not the war in question did indeed have an effect on domestic violence. These tests are done for the onset of the war and the conclusion of the war; i.e., the "experiment" that interrupts the time series is first analyzed as the onset of the war and then analyzed as the conclusion of the war. The results show that there are significant intercept changes in domestic violence both with respect to the onset and conclusion of a war; that is, the war "experiment" does make a difference. For the onset of World War I, and the wars in Korea and Vietnam, domestic violence rises, while for the onset of the Spanish American War and World War II it decreases; for the conclusion of the Spanish American War, World Wars I and II, and the Korean War domestic violence increases. To shed further light on these analyses Stohl examines three additional hypotheses concerning wartime economic and social changes.

The volume is generally well written although it contains the typical introductory chapters (three in this case) which review literature somewhat related to but only tangentially integrated with the following research design and analysis. Thus, following an introduction, the two subsequent chapters describe the conflict literature à la Simmel and Coser and some empirical analyses on the linkage between domestic and foreign conflict. While the overview provided is concise, its explicit contribution to what follows is left to the reader. The discussion of research design, analysis and results is ably presented, though some may find the description of the indicators and variables too brief.

All in all, this is a nicely executed study of a significant question that has not received adequate research attention. While a number of scholars have examined the impact of domestic violence on foreign violence, too few have turned the question around to ask about the impact of foreign violence on domestic unrest. This study makes an important contribution towards the understanding of this problem.

DINA A. ZINNES

*Indiana University*

**The Defence of India: A Budgetary Perspective of Strategy and Politics.** By Raju G. C. Thomas. (Delhi, Bombay, Calcutta and Madras: Macmillan, 1978. Pp. vi + 245. Rs. 55.00.)

This book will interest students of Indian politics and scholars concerned with the foreign policy of India as it affects and is affected by the balance of power among nations of the Indian Ocean basin. Raju Thomas states that "the size and spread of the annual Indian defence budgets form the theme" (p. 4) of his inquiry. Indeed, the greater part of the analysis is devoted to a careful examination of those factors which determine how much of the Indian budget is allocated to defense as well as to those factors which affect its distribution to the Indian army, air force and navy. What is perhaps of broader interest to policy analysts, however, is the implication inherent in the book's argument: that budgetary constraints and considerations shape a nation's foreign policy and its strategy in pursuing that policy.

The *Defence of India* is divided into eight chapters. An introductory chapter details the substance, but not the argument of the work. Three subsequent chapters are devoted to an examination of macro-level variables which determine the size of defense allocations while another three chapters explore micro-level distributive variables important to the determination of appropriations for each of the Armed Services. A final chapter deals with Indian defense planning in the Post-Bangladesh phase.

Thomas stipulates that there are three macro-level variables important to the politics of allocation. The first of these is labeled perceptions of threat. Exactly what those perceived threats are depends upon the activity and military posture of potential adversaries, namely, Pakistan and China. Basing Indian defense needs on the actions of possible opponents is highly complex. For example, according to the author, Chinese efforts "to secure itself from a more powerful Soviet Union" (p. 8) poses a threat to India because China could also use those arms against India. India's policy of non-alignment was developed in order to fill the gap between *needed* military capability and *actual* capability" (p. 9), as in the case of China.

A second allocative variable is the political structure with primary focus on the Indian parliament. Due to the strength of India's Congress party during the period of this study, the opposition had relatively little impact on defense questions, allowing the Executive or Cabinet a very strong hand in the policy-making process. As in many democracies, Indian defense decisions are greatly influenced by political personalities and bureaucracies. India lacks a large-scale private defense industry so that lobbying by a military-industrial complex is

absent from Indian defense politics.

The third variable pertains to the economic constraints and consequences of defense spending: in what way will defense spending retard or abet economic development? This question can be broken into two parts: what resources are available for defense and how much of the available resources can be allocated to defense without wreaking havoc upon the economy? Thomas indicates that the real debates among policy makers have concerned the latter dimension of this two-part question.

The second theme of the book concerns the determination of how monies will be distributed among the three departments of defense. Thomas argues that policy at the micro level depends on four factors: (1) projections about the nature and duration of wars; (2) who is perceiving the threats; (3) personalities of the service heads and their impact upon the Cabinet; and (4) the economic viability and trade-offs among the types of weapons systems proposed by the three services.

Historically, the Indian army has received the bulk of India's defense allocation. This is due to factors stemming from the British Colonial period. Also India has in the past fought only land wars, thus limiting the need for a large navy. Finally, investments in the army are attractive because the army is labor-intensive whereas both the navy and air force require large amounts of capital for sophisticated weapons, necessitating expenditure of scarce foreign currency reserves and/or dependence on foreign suppliers.

In the course of his examination of the size and shape of the Indian defense budget, Thomas demonstrates that defense planning always prepares a country to fight a past war but leaves it vulnerable in the prosecution of future wars. Thomas implies that India's role on the subcontinent as well as its future needs are now uncertain because Pakistan is no longer as serious a threat to India's security and China is preoccupied elsewhere. With the 1971 withdrawal of the British East of Suez, a power vacuum was created in the Indian Ocean. What will the Soviet Union and the United States do to fill that vacuum? Will the oil- and dollar-rich Moslem nations of the Mideast form an alliance with Pakistan? These are the questions currently of concern to Indian defense planners.

*The Defense of India* is a solid and useful work. It has two important shortcomings, however. There is a lack of theoretical development, which results in an overly descriptive and at times repetitive account of the policy-making process. Thomas clearly states that he is looking

at defense from a budgetary perspective without clearly denoting what that perspective is or why it is to be preferred to other modes of analysis. The second failing of this work results from the inability of the author to obtain information and interviews with key decision makers. Thus, the analysis of the role of various individuals and interest groups within the Indian defense establishment is superficial and incomplete. Finally, two minor complaints. This book could have benefited from editing to rid it of some of its typographical errors, i.e., "Indan" (p. 1). Secondly, for readers not familiar with Indian monetary terms, some indication of the meaning of the words "crores" and "laks" would be useful. The failings of this book do not detract seriously from its worth. They are more in the nature of sins of omission rather than commission.

STEPHAN J. COHEN

*University of California, Davis*

**U.S.-Japan Relations and the Security of East Asia: The Next Decade.** Edited by Franklin B. Weinstein. (Boulder, Colo.: Westview Press, 1978. Pp. xii + 318. \$14.00, cloth; \$7.00, paper.)

This book emanates from the Stanford University Project on United States-Japan Relations which, since 1974, has held discussions on security and arms control problems among non-governmental specialists of both countries. Strategies for the defense of Japan, the role of nuclear deterrence and the Self-Defense Forces (SDF), Korea, arms control and nuclear proliferation receive detailed consideration in this volume. Notably missing is any article specifically concerned with the role of China or the Soviet Union, an omission which, in the case of China, we are told, is derived from the views of Japanese participants about arms control.

For all the past and impending changes in the context of the security relationship (set forth in a contribution by Weinstein and John W. Lewis), remarkably few significant modifications are recommended. No voice is raised for ending or even revising the security treaty, for eliminating the SDF or for a major expansion of it. The American deterrent is deemed sufficiently credible to deal with any nuclear threat, whereas no independent Japanese capability could be (Morton Halperin); there is no need for Japan to abandon her three non-nuclear principles (Tokuya Kubo). Recommendations for greater use of Japanese economic power, for

more coordination and for exchange of intelligence information do not represent new departures.

Perhaps the change most clearly reflected here is the emerging consensus in Japan on foreign and defense policies. Acceptance of the treaty and of the SDF at roughly its present level of strength has been growing in spite of the declining fortunes of the Liberal Democrats. With the recent agreements concluded with China, and with the Chinese even encouraging support for the security alliance, China policy seems to have become a part of the consensus.

Differences among the participants are those of emphasis and degree rather than of kind. Should Japan increase its defense forces or rely on a non-military approach to security? Elements of both are needed, according to Makoto Momoi, while Henry Rowen urges strengthened self-defense, especially of air and naval forces. Weinstein takes strong exception to what he terms the "fallacies of burden-sharing." James Morley's view is very balanced: if the United States has overemphasized the military approach to security, Japan has slighted it. A security policy needs to be based on such political realities as American acceptance of very limited Japanese rearmament and Japanese recognition of the need to provide convincing evidence of its determination to defend itself. The recent move to increase substantially Japan's contribution toward the cost of maintaining American bases represents a useful step in accord with those realities.

The editor argues for a phased withdrawal of all American forces from South Korea, a policy which he feels can find support in Japan. Other proposals advocate cross-recognition by the four major powers (Fuji Kamiya), and unilater-

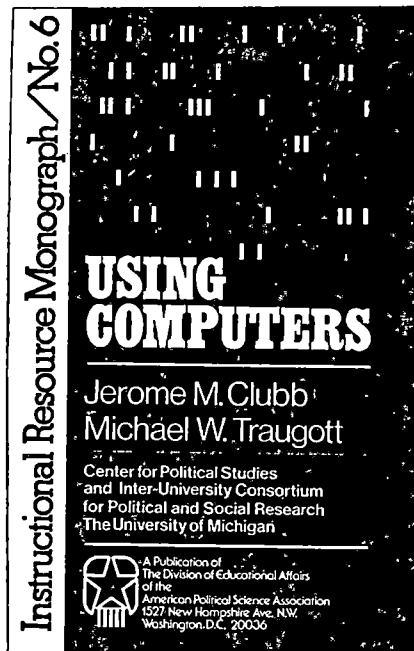
al withdrawal with a symmetrical policy toward both North and South (Selig Harrison); the drawbacks of the latter proposal are pointed out in a critical commentary (by Ralph Clough). Again, the differences are ones of implementation rather than basic policy. No one sees war as imminent or questions Korea's strategic importance. Ultimately, one gets down to the matter of judgment and differing perceptions of the drives, motives, probable reactions of the North Korean regime, in particular, its leader.

The discussion of arms control and nuclear proliferation by Ryukichi Imai illustrates the problems caused by differing perceptions. At least until recently, the Japanese have not perceived this as a subject warranting serious attention; their approach has been from a regional, not global, perspective, and nuclear energy bulks large in their calculations. Other, more general problems of differing perspectives are indicated by Daniel Okimoto.

The quality of these individual presentations is uniformly high, although coverage and treatment of issues is uneven. If the range of opinions is limited—the neutralist position of orthodox Japanese socialism is not present, for example—this reflects the widening consensus noted above. A decade ago, the focal points of argument undoubtedly would have been different. For the present, the Japanese-American security relationship appears in sound shape. To sustain it will require continued attention to economic controversies lest they become politically divisive and to the pervasive sources of misperception and misunderstanding which can undermine both political and economic ties.

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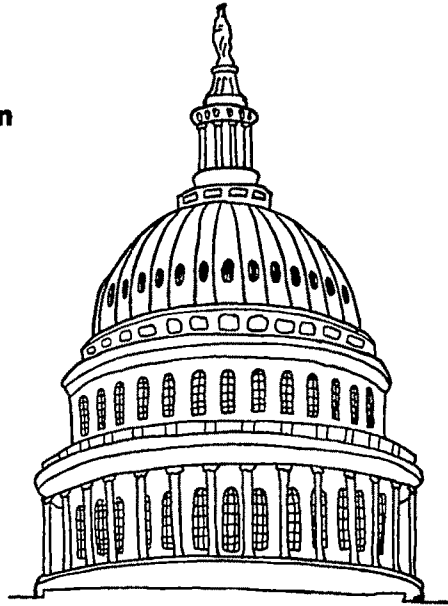
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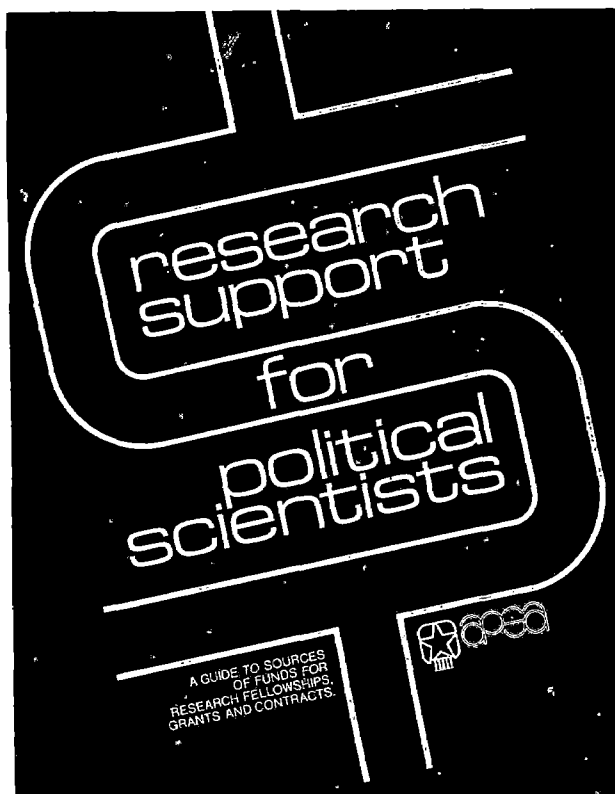
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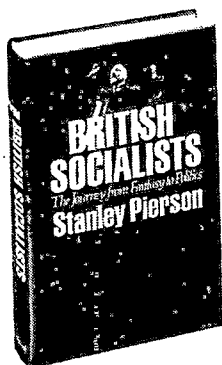
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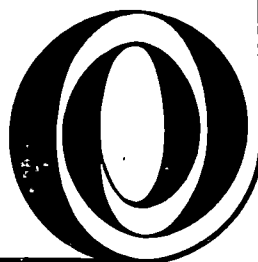
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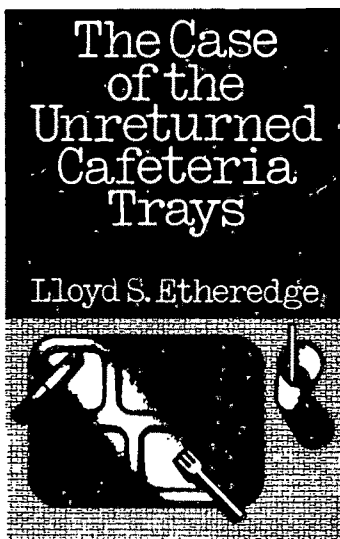
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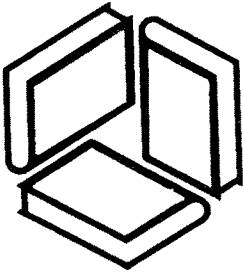
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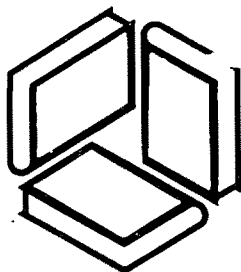
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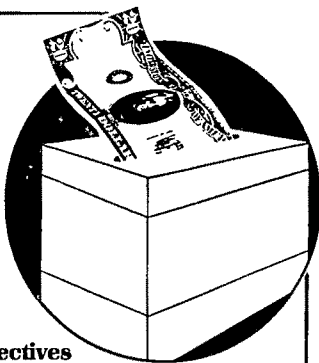
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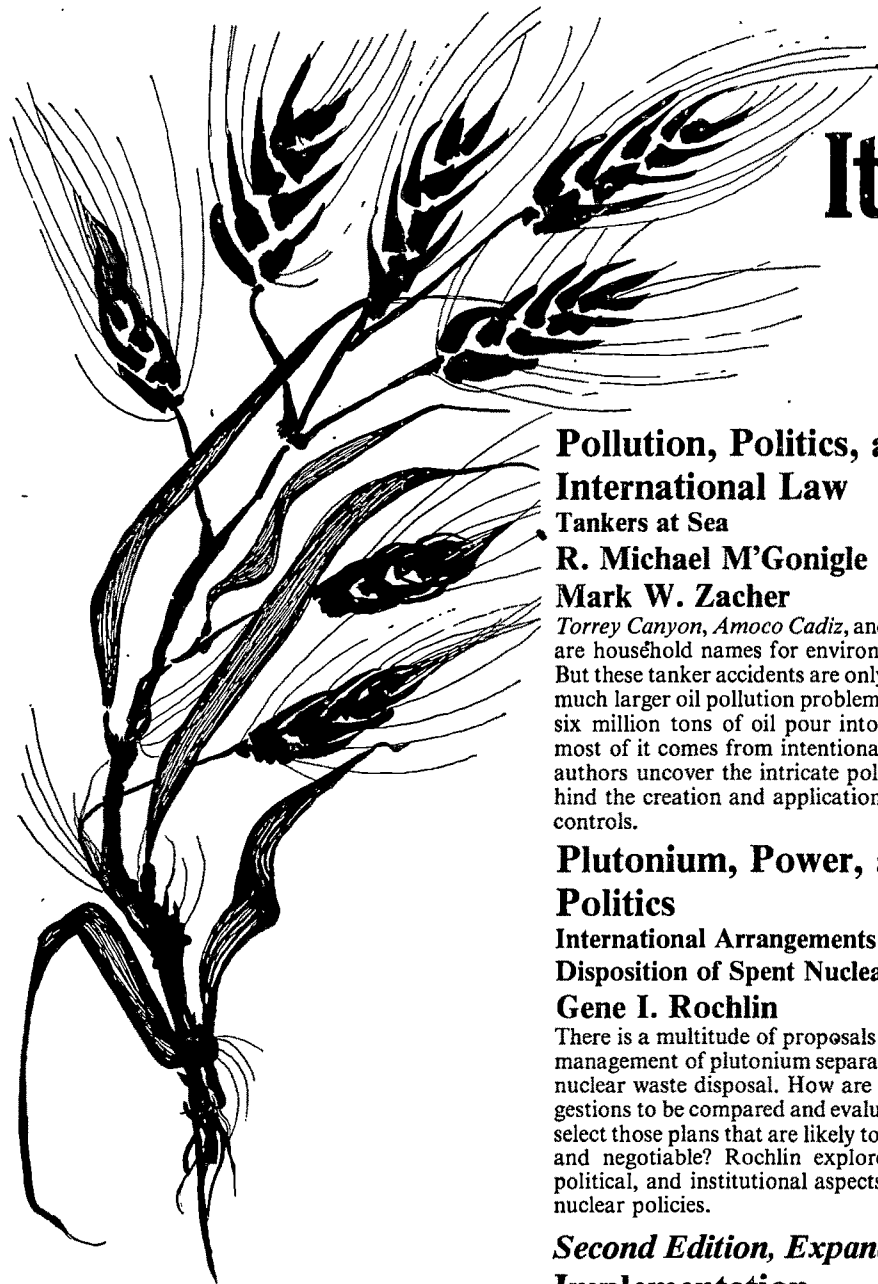
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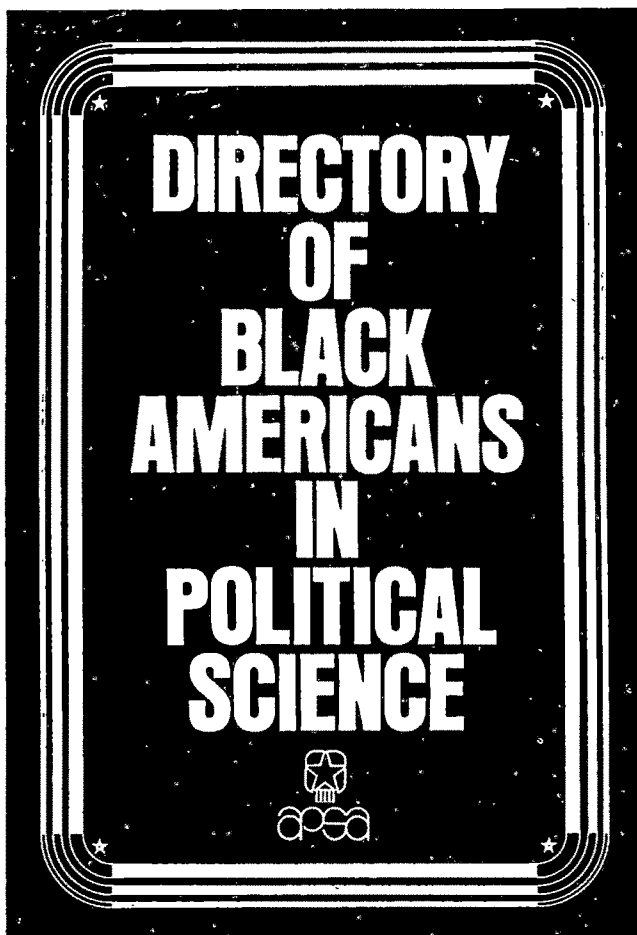
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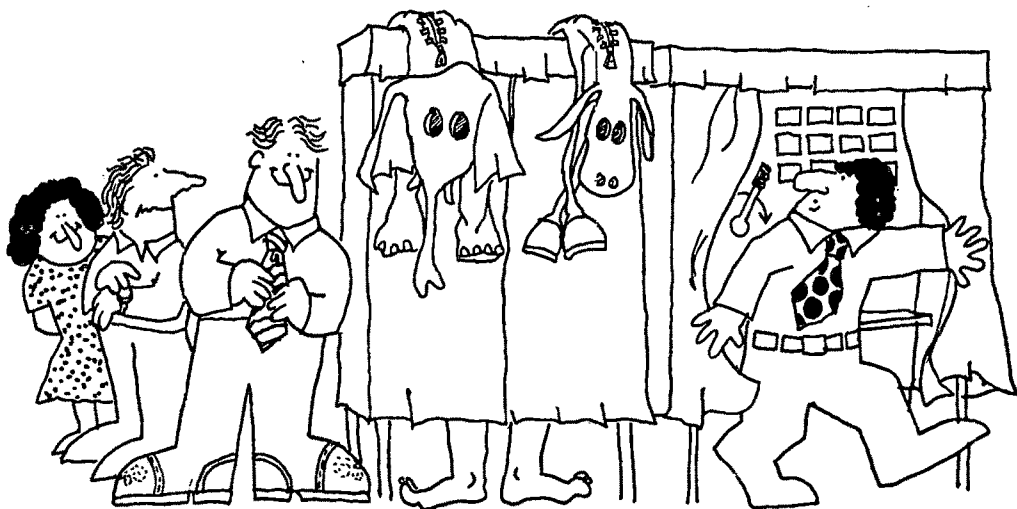
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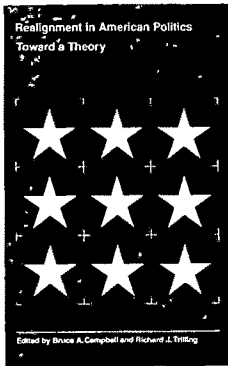
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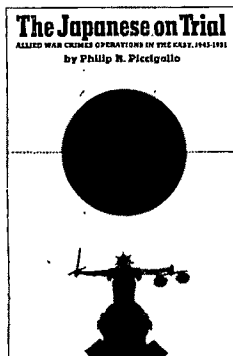
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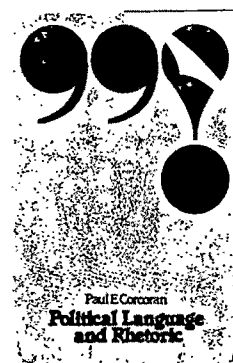
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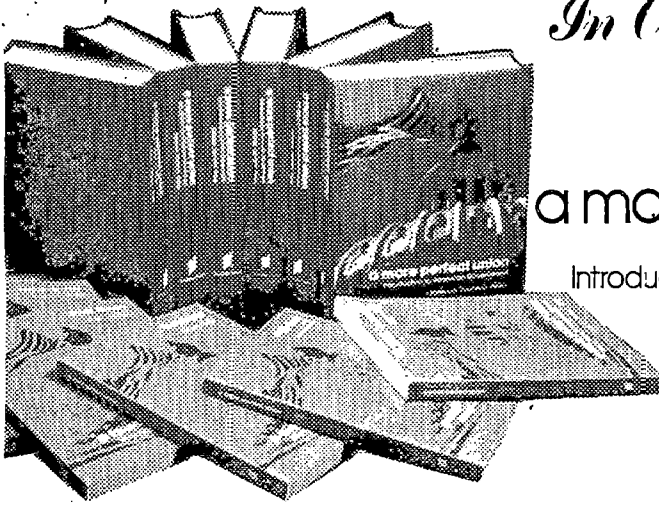


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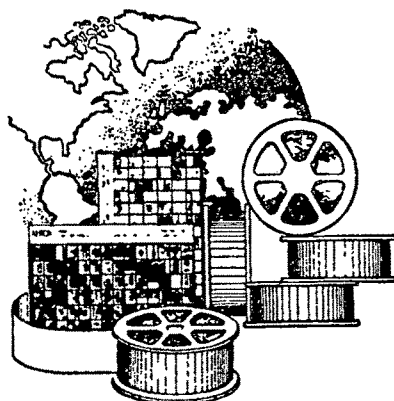


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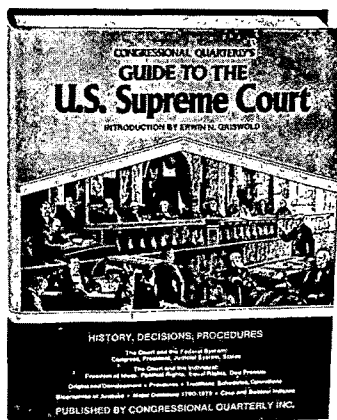
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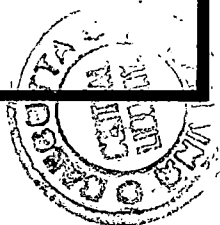
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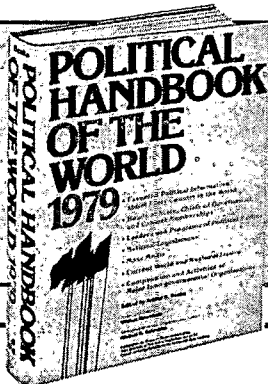
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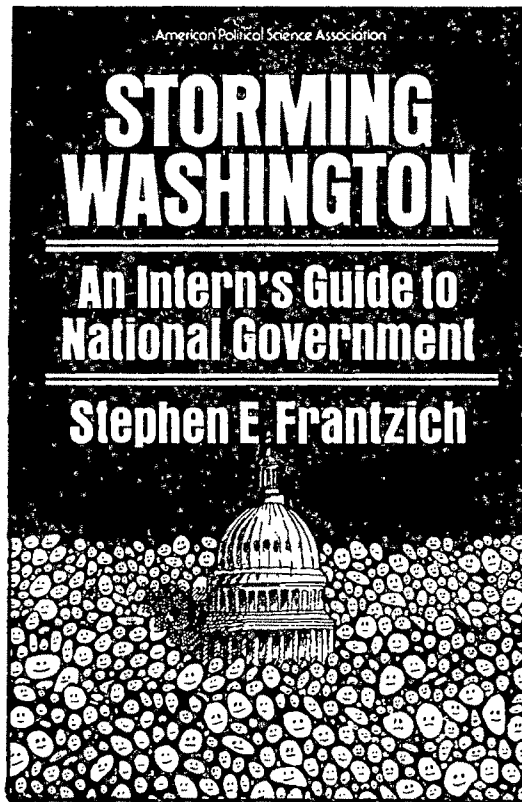
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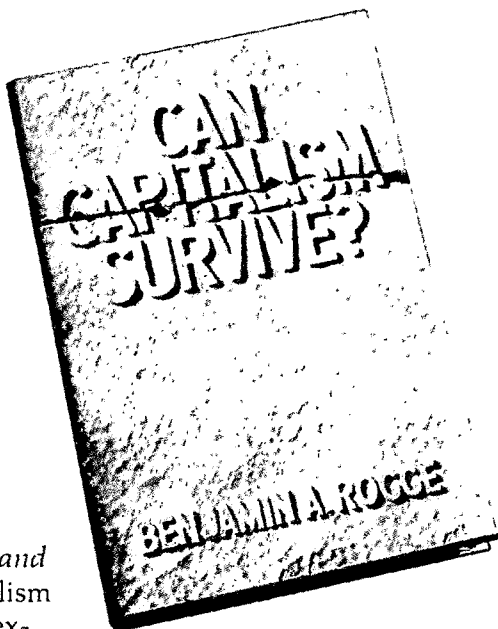
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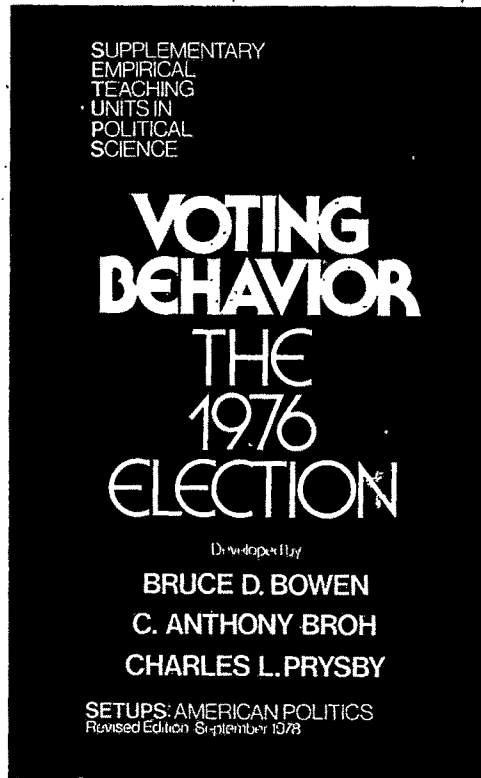
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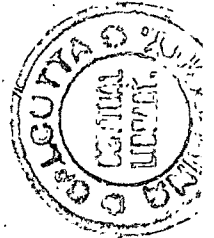
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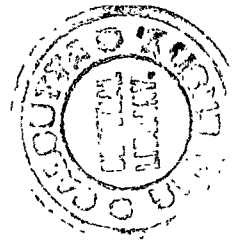
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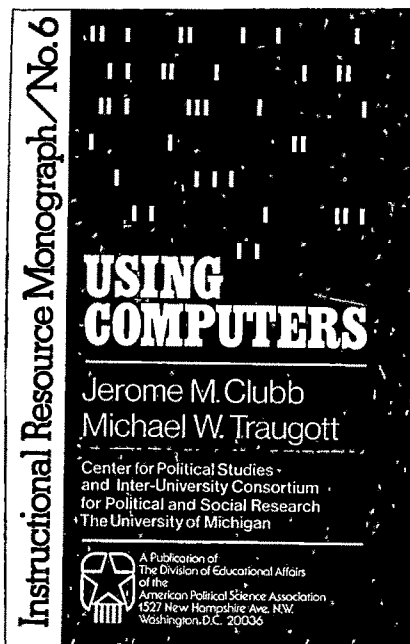
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# Inducements versus Constraints: Disaggregating "Corporatism"

RUTH BERINS COLLIER

DAVID COLLIER

*University of California, Berkeley*

*The concept of "corporatism" has usefully called attention to the importance of systems of interest representation based on non-competing groups that are officially sanctioned, subsidized, and supervised by the state. Yet these patterns have appeared in such a remarkable variety of political contexts that this concept may be too broad to be useful. On the basis of an analysis of the relationship between the state and organized labor in Latin America, this article argues that the concept of corporatism can be disaggregated so that it sheds light on rather than obscures the different power relationships and political contexts with which it is associated. The analysis focuses on the distinction between "inducements" extended by the state to win the cooperation of groups and "constraints" through which the state directly controls groups. This disaggregated approach enables one to distinguish more subtly among systems of group representation, to conceive of state-group relations in more interactive terms, and to gain insights into the larger political context.*

The concept of "corporatism" has recently emerged as a central point of reference in research on interest representation. More broadly, this concept has played a central role in the renewed effort to discover more adequate ways of conceptualizing alternative patterns of state-society relations and alternative modes of political domination.<sup>1</sup> Scholars concerned with various world regions have called attention to the tendency toward a corporative ordering of interest politics and of state-society relations around non-competing groups which are officially sanctioned, closely supervised, and often subsidized by the state.

Corporatism has received particularly wide-

spread attention in analyses of the relationship between the state and organized labor in Latin America. It is argued that Latin American governments have commonly sought to exercise control over labor movements and that within this context of control, the concept of corporatism captures an important aspect of the network of hierarchical relationships through which labor organizations come to be dependent upon and penetrated by the state.<sup>2</sup>

This focus on corporatism provides an important alternative to earlier pluralist perspectives in that it takes as a starting point the role of the state in shaping interest representation. Yet a closer examination of the contexts in which corporative patterns of state-labor relations have emerged in Latin America reveals such a diversity of political relationships and of goals on the part of elites who introduce corporative provisions that it seems reasonable to ask whether all these cases should be grouped under a single concept. While the concept of corporatism may be valuable as a first approximation, it appears to miss much of the give-and-take of politics.

We propose here a new approach to conceptualizing corporative patterns of state-group relations that will make it possible to deal more

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<sup>1</sup>One indicator of the rising importance of this focus is the appearance of special issues of two political science journals devoted exclusively to this topic: the January 1974 issue of *The Review of Politics* and the April 1977 issue of *Comparative Political Studies*. Two of the most widely cited articles that provide an overview of this theme are Wiarda (1973) and Schmitter (1974). A valuable recent discussion is Stepan (1978).

<sup>2</sup>The focus on corporative structures as mechanisms of control is a central theme in the literature on Latin America (see footnote 5). However, it should be stressed that some forms of corporatism do not primarily involve state control over groups. The discussion of "state" versus "societal" corporatism at the end of this article addresses this issue.

adequately with this diversity of power relationships. This approach provides a better basis for analyzing contrasting patterns of state-group relations—including both differences among countries and patterns of change within countries. Though our immediate empirical referent is the relationship between the state and organized labor in Latin America, this modified conceptualization may be extended to the analysis of other types of groups and other regional contexts.

### Corporatism

The term corporatism has been applied to a wide variety of phenomena, including modes of political participation, types of political action, ideologies, and broad cultural traditions (Wiarda, 1974; Rogowski and Wasserspring, 1971; and Palmer and Middlebrook, 1976). At the same time, there has emerged a common usage in which systems of interest representation—and more specifically, different patterns of state-group relations—are the central issue. Drawing on the shared usage in this literature,<sup>3</sup> one may define a system of state-group relations as corporative to the degree that there is (1) state *structuring* of groups that produces a system of officially sanctioned, non-competitive, compulsory interest associations; (2) state *subsidy* of these groups; and (3) state-imposed *constraints*<sup>4</sup> on demand-making, leadership, and internal governance. Corporatism is thus a *non-pluralist* system of *group* representation. In contrast to the pattern of interest politics based on autonomous, competing groups and to the total suppression of groups, in the case of corporatism the state encourages the formation of a limited number of officially recognized, non-competing, state-supervised groups.

This usage has been particularly common in research on state-labor relations in Latin America. Analysts have frequently viewed these relations as involving corporatism, with a widespread use of corporative mechanisms by the state to shape and control labor organizations.<sup>5</sup>

<sup>3</sup>Schmitter, 1974, pp. 93–94; Wiarda, 1974, p. 6; O'Donnell, 1977; Cotler, 1972; Schwartzman, 1977; Kaufman, 1977; Mericle, 1977; Stepan, 1978, Ch. 2; Reyna, 1977; Malloy, 1977.

<sup>4</sup>In Collier and Collier (1977) we referred to these types of restrictions as controls. For the sake of clarity, however, we will here refer to them as constraints and use the term control somewhat more broadly (see below).

<sup>5</sup>O'Donnell, 1977; Kaufman, 1977; Collier and Collier, 1977; Wiarda, 1976; Erickson, 1977; Harding,

And while there are unquestionably important periods in which state-labor relations in Latin America involve outright repression, as well as a very few cases of a fair degree of pluralism, one may accurately characterize the predominant pattern during much of the twentieth century as corporative.

At the same time, we have argued elsewhere (Collier and Collier, 1977) that in Latin America, corporative patterns of state intervention in organized labor have been introduced in the context of a striking diversity of power relationships and policy goals. Corporative provisions have been used in some cases to strengthen the position of workers and unions in relation to employers, whereas in others they have been used to weaken their position. Corporative provisions have sometimes been used by political parties to win workers' support, and at other times to insulate workers' associations from involvement with parties as a means of restricting their political power. In some contexts members of the military elite have seized the government and used corporative provisions to aid labor organizations and mobilize their support, whereas in others such elites have used these provisions to control labor sharply. Corporative provisions have been promoted by an extraordinary spectrum of governments, ranging from repressive, right-wing governments, through "populist" governments such as the Cárdenas government in Mexico and the first Perón government in Argentina, to Castro's Cuba (Wiarda, 1974, p. 4; Collier and Collier, 1977).

These observations suggest that the concept of corporatism may apply to so many different cases that it often tells one little or nothing. If such a diversity of cases can be found even within just one world region and considering only the relationship between the state and one class group, organized labor, perhaps this concept simply casts too broad a net to be useful.

There are two ways of avoiding this problem. The first is to treat corporatism explicitly as a dimension, or a set of dimensions, along which cases may be arrayed. Since "real pluralism" is relatively rare in the contemporary world, there may be a tendency to find corporatism almost everywhere. But, in fact, systems of interest representation are not identical everywhere; there are major differences in the degree of structuring, subsidy, and constraints introduced by the state. Corporative

1973; Schmitter, 1971, 1974; Mericle, 1977; Córdoba, 1974; Reyna, 1977; Corradi, 1974; and Petras, 1969.

patterns of state intervention, like pluralism, should thus not be conceived narrowly as either present or absent, but rather as a variable that may assume different values, as a phenomenon that may be present to varying degrees (Collier and Collier, 1977).

The second means involves disaggregation. Because corporative intervention in interest representation appears in the context of very different relationships of economic and political power, one may reasonably ask whether the corporative patterns in these different contexts really are the same. Since few countries have the full complement of corporative provisions typically identified in the literature on this topic, perhaps different combinations of provisions appear in these different contexts. Though at a high level of aggregation these cases may all be corporative, at a more disaggregated level there may be striking differences among them.

### Inducements versus Constraints

As a first step toward disaggregating corporatism, one may note that some corporative provisions bestow advantages upon the labor organizations that receive them, whereas others do not. The *structuring* of group representation through provisions that provide for such things as official recognition, monopoly of representation, and compulsory membership—as well as the *subsidy* of groups—provide important organizational benefits. In this sense these provisions are quite distinct from the *constraints*, which directly control labor organizations and labor leaders.

The idea that structuring and subsidy are benefits is supported by analysts of political organizations, who suggest that these provisions do in fact address basic organizational needs of labor unions (Bendix, 1964, pp. 80–97; Olson, 1971, Ch. 3; and Wilson, 1973, Ch. 3). These include the need to compete successfully with rival groups that seek to represent the same constituency; the need to be recognized as the legitimate representative of their constituency in their dealings with other sectors of society; the need to recruit and retain members; and the need for stable sources of income. Because structuring and subsidy help to meet these needs, they confer significant advantages on the organizations that receive them.

Though structuring and subsidy thus provide important organizational benefits, one must understand the political context in which these provisions appear in order to interpret their significance. Corporative policies toward or-

ganized labor in Latin America have been introduced from above by elites, acting through the state, who have used these policies to help them pursue a variety of goals—involving an effort to shape the behavior of the labor movement and/or to win its political support. It therefore seems appropriate, within the Latin American setting, to view structuring and subsidy not simply as benefits but as *inducements* through which the elite attempts to motivate organized labor to support the state, to cooperate with its goals, and to accept the constraints it imposes. In this context, corporatism may thus be viewed as involving an interplay between inducements and constraints.

This idea of an interplay between inducements and constraints is consistent with standard discussions of the dialectical nature of state-labor relations in Latin America. Goodman (1972, p. 232) has interpreted Latin American labor law, the most important formal expression of the relationship between the state and organized labor, as containing both a “carrot and a stick” for labor. Spalding (1972, p. 211) has analyzed the tendency of the state and elite groups in Latin America to “seduce and control” organized labor. The terminology employed in a standard manual of labor relations in the United States suggests that the inducement/constraint distinction is salient outside of Latin America as well. This manual contrasts provisions in labor law that involve “labor sweeteners” sought by unions with those involving “restrictions” on unions sought by employers (Bureau of National Affairs, 1972, p. 4). More broadly, Schmitter (1974, p. 92) hinted at this distinction when he suggested, without elaboration, that corporative provisions which we have referred to as involving constraints may be accepted by groups “in exchange for” the types of provisions we have identified as involving the structuring of groups.

Though one can thus distinguish between inducements and constraints, it is important to emphasize that these are not diametrically opposed phenomena. Analysts of power and influence such as Lasswell and Kaplan (1950, pp. 97–98) and Gamson (1968) distinguish between inducements and constraints but view both as mechanisms that serve to influence behavior. Constraints are seen as producing compliance by the application, or threat of application, of negative sanctions or “disadvantages.” Inducements, by contrast, involve the application of “advantages” (Gamson, 1968, pp. 74–77). Yet in this literature inducements are also viewed as mechanisms of co-optation. As such, though they involve “advantages,” they also lead to social control.

This dual nature of inducements is clearly evident in the specific mechanisms of structuring and subsidy discussed above. These inducements may, like the constraints, ultimately lead to state penetration and domination of labor organizations, for at least three reasons. First, an inducement such as monopoly of representation is by its nature offered to some labor organizations and withheld from others. This provision has commonly been used in Latin America to undermine radical unions and to promote those favored by the government (Harding, 1973, p. 71; Kenworthy, 1970, pp. 159–60; and Silverman, 1967, pp. 137–54). Second, unions receiving inducements must commonly meet various formal requirements in order to receive them. Finally, the granting of official recognition, monopoly of representation, compulsory membership, or subsidy by the state may make the leadership dependent on the state, rather than on union members, for the union's legitimacy and viability. This dependency accelerates the tendency for labor leadership to become an oligarchy less responsive to the needs of the workers than to the concerns of state agencies or the political elite with which the leaders interact. This dual nature of the inducements explains why high levels of inducements, as well as of constraints, are often instituted by members of the elite whose goal is to produce a docile, controlled labor movement.

If both inducements and constraints ultimately lead to social control, it remains to be demonstrated that labor organizations really desire to receive the inducements—that these provisions in fact induce labor organizations to cooperate with the state and to accept the constraints. A preliminary examination of the evidence suggests that this is often the case.

For example, these assessments are often expressed at the time of the enactment of the first major law that provides a basis for legalizing unions and that commonly includes a number of inducements and constraints for the unions which become legally incorporated. An important example is found in Argentina. The dominant sector of the Argentine labor movement initially rejected Juan Perón's initiatives to gain the cooperation and support of the labor movement in the 1940s. Only when Perón began to adopt the program of this sector of the movement, i.e., to support the *organizational* goals of labor as well as their substantive demands on bread and butter issues—in part through a labor law that placed heavy emphasis on inducements—did major sectors of the labor movement begin to accept his offers of cooperation (Silverman, 1967, pp. 134–35).

In Mexico the reaction of labor to the first national labor law in 1931 again reflected the dual nature of the law, encompassing both inducements and constraints. Labor leaders objected to certain constraints—the provisions for federal supervision of their records, finances, and membership lists—whereas they accepted the provisions for the recognition of unions—defined above as an inducement. Furthermore, they were dissatisfied over the *absence* of a provision that is clearly an inducement—compulsory membership (Clark, 1934, p. 215; Harker, 1937, p. 95).

The debate within the labor movement concerning the passage of the 1924 labor law in Chile reflects this same pattern. The dominant Marxist sector of the labor movement generally accepted the new system, arguing that it had to “use all the social legislation of the capitalist state to fight capitalism itself” (Morris, 1966, p. 246). The debate within the labor movement showed that though this sector opposed the constraints contained in the law, it was clearly attracted by the law's provisions that would help it to extend its organization to new economic sectors and would allow it to receive a state-administered financial subsidy derived from profit-sharing. The inducements contained in the law were thus sufficient to motivate the dominant sector of the labor movement to cooperate with the state.

The 1924 Chilean law is useful for underlining another point as well. Though the inducements offered by the state have often been sufficient to win the cooperation of labor, this has not always been the case. Historically, the anarchists were acutely aware not only of the costs of the constraints that accompany the inducements, but also of the tendency of the inducements themselves to lead to social control. Thus, following the traditional anarchist position regarding the risks of co-optation arising from cooperation with the state, the anarchist sector of the Chilean labor movement rejected the 1924 law completely. Another example is the 1943 law in Argentina, which was widely opposed by organized labor. At that point, the state was not willing to extend sufficient inducements to win the cooperation of labor, which rejected the constraints. It is noteworthy that the Peronist law of 1945 (see above) provided the necessary level of inducements and was accepted by organized labor, despite its similarly high level of constraints.

These examples suggest that while some groups will resist these inducements, the inducements have, in fact, often served to win the cooperation of labor groups and to persuade them to accept the constraints. Furthermore,

the distinction between inducements and constraints is not merely an analytic point of concern only to social scientists, but rather a vital political issue in the history of state-labor relations in Latin America.

### Different Political Contexts

Do inducements and constraints occur in distinct patterns in different political contexts? If so, does the analysis of these different patterns contribute to understanding the different settings in which corporatism has appeared? To address these questions, we must focus in greater detail on specific corporative provisions that have typically characterized state-labor relations in Latin America.

**An Operationalization of Inducements and Constraints.** In the context of state-labor relations, inducements and constraints of course take many forms—including bribery and overt repression. Yet many important inducements and constraints—such as those conventionally referred to in conceptual discussions of corporatism—are found in labor law, which will be used here as a basis for illustrating the interplay between these two dimensions. We hardly need emphasize that law does not, by itself, reflect the full reality of state intervention in labor organizations or labor relations. Laws may not be applied, or they may be applied differentially.

Yet law is important. It is commonly asserted by specialists in organized labor in Latin America that labor law is indeed one of the crucial factors that shape labor movements (Miller, 1966, p. 11; Erickson, 1977, p. 29; Wiarda, 1976, p. 11; International Labor Office, 1961, p. 269; and Valenzuela, 1976, p. 151). Furthermore, the adoption of laws is a major step in the decision process through which state intervention in labor representation crystallizes. Labor law is a highly visible and concrete policy statement around which political battles are fought, won, and lost, and around which political support is attracted, granted, and withheld. Especially for the years in which labor law is promulgated or modified, law thus provides a valuable point of reference for analyzing the larger political context. Of course one must be cautious in using an older law which has been left on the books as a basis for interpreting the politics of a subsequent period. We therefore focus particularly on the years in which laws are adopted—though in some cases, as in the interpretation most notably of Mexico, long periods of stability of

law do point to an important continuity in the political context. Despite this caveat, however, law provides a useful source of data for the comparative analysis of the different approaches to shaping labor organizations and labor relations that are grouped together by policy makers in this crucial phase of the policy process. For the analyst concerned with whether different patterns of inducements and constraints appear in different political contexts, law thus represents a valuable source of data.

In order to apply the inducement-constraint distinction in a comparative/longitudinal analysis of state-labor relations in Latin America, we scored a series of legal provisions corresponding to the different elements in standard definitions of corporatism discussed above for 20 Latin American countries for each year over the period 1901 to 1975.<sup>6</sup> Under the heading of inducements, the scoring focused on provisions regarding registration, right of combination, monopoly of representation, compulsory membership, and subsidy of unions. The heading of constraints included provisions regulating collective bargaining and strikes, other controls on demand-making, controls on leadership, and provisions for state monitoring and intervention in internal union affairs. While these provisions obviously do not include all inducements and constraints that may appear in labor law, they represent a constructive starting point for analyzing this distinction.<sup>7</sup> Statistical analysis of these provisions indicated that it was appropriate to group them into two overall scales that reflected the degree to which inducements and constraints were present in the labor legislation of each country. These scales are used as the

<sup>6</sup>We used International Labor Office 1919–1975 and 1930, as well as a wide variety of other secondary sources, for identifying relevant statutes and for scoring the statutes. For most of the statutes, the original text was consulted as well.

<sup>7</sup>The scoring was restricted to labor law as it is formally defined in Latin American legal systems. Though the discussion does at a few points take into consideration other major legal provisions, such as states of siege, that may supersede labor law, the formal scoring does not attempt to cover all provisions relevant to labor organizations. It does not, for instance, consider provisions contained in criminal codes in the earlier part of this century that were used to restrict labor organizations in the period before the advent of formal labor legislation. The goal is not to provide a definitive assessment of all legal provisions relevant to labor organizations, but rather to see if certain patterns emerge among the provisions that have particularly concerned analysts of corporatism.

basis of the analysis presented below. Appendices A and B explain the scoring of these legal provisions and the construction of these scales.

**Contrasting Patterns of Change.** The distinct patterns of change in inducements and constraints in Figure 1 provide a useful starting point for exploring the relationship between these dimensions and the larger political context. In Argentina, for instance, one finds a volatile pattern of change that reflects frequent shifts in the coalitional position of organized labor in Argentine politics—as well as, overall, a relatively greater emphasis on inducements in relation to constraints.<sup>8</sup> The dramatic shift to a high level of inducements in 1945 has already been noted. The context of this shift was the effort by Perón to gain the support of the large, well-established, and autonomous Argentine labor movement as he attempted to rise from a subordinate position within the military government that came to power in 1943. With the help of labor support, he was elected president in 1946. One of Perón's most visible acts in his attempt to court labor support was the abrogation of the unpopular 1943 law, which was heavily oriented toward constraints. In 1945 a Peronist labor law was introduced which included a similar level of constraints but which attracted overwhelming labor support in part because of the high level of inducements. Though Perón's most pro-labor period might be said to have ended in 1946 and though Perón became increasingly preoccupied with curbing labor and its demands as early as 1948–49, this policy shift proved quite difficult, since Perón remained heavily dependent on labor support. This combination of dependence on labor and concern with restraining its demands is reflected in the more "balanced" addition to labor law in 1953. The years following the ouster of Perón in 1955 were characterized by ongoing shifts in labor law that correspond to changes in the political context. For instance, in 1956 the anti-Peronist government which sought to undermine the dominant Peronist segment of the labor movement added constraints and dropped inducements. In 1958 inducements increased and constraints decreased as Peronists bargained with Frondizi over the terms under which they would grant him their electoral support.

The relationship between the state and

organized labor in Brazil has been different from that in Argentina, and a different pattern of labor law has evolved.<sup>9</sup> Organized labor has been relatively weak in Brazil and played a marginal role in the rise to power of Vargas in 1930. In fact, a major concern of the leaders of the "Revolution" of 1930 which brought Vargas to power was to preempt the emerging labor movement and the "Bolshevik threat" which its connection with the Communist party seemed to imply. Once in power, Vargas sought to dismantle this labor movement and replace it with a state-controlled system of labor representation. Though his labor and welfare policies eventually won him the support of much of the working class, Vargas was not dependent on the working class for political support in the way that Perón was in Argentina. Correspondingly, within the framework of a more full-blown corporatist system, Brazil moved to high levels of both inducements and constraints.

Within this overall pattern, there are interesting short-term changes in Brazil. The rise in influence of anti-corporative, liberal groups in the mid-1930s is reflected in a brief reduction in the level of inducements. The earlier level was restored in 1937, and by 1939, under the explicitly corporatist *Estado Novo* (New State), Brazil moved to an even higher level of both inducements and constraints. By 1943 the *Estado Novo* was on the defensive and Vargas began laying the groundwork for the more active electoral support that he would need with the introduction of competitive politics after 1945. At this point he assumed a more populist stance, sponsored a political party to mobilize labor support, and introduced a more inducement-oriented labor law. The period following the fall of the *Estado Novo* in 1945 was characterized by shifting power relationships which produced, as in Argentina, a "circular" pattern of change in law (see Figure 1) as provisions for inducements and constraints were promulgated and abrogated. For instance, after the fall of Vargas in 1945 there was a brief reduction of inducements as the new government sought to undermine the position of the dominant sector of the labor movement, which was linked politically to Vargas. These provisions were restored within a few months in the face of protests from labor leaders.

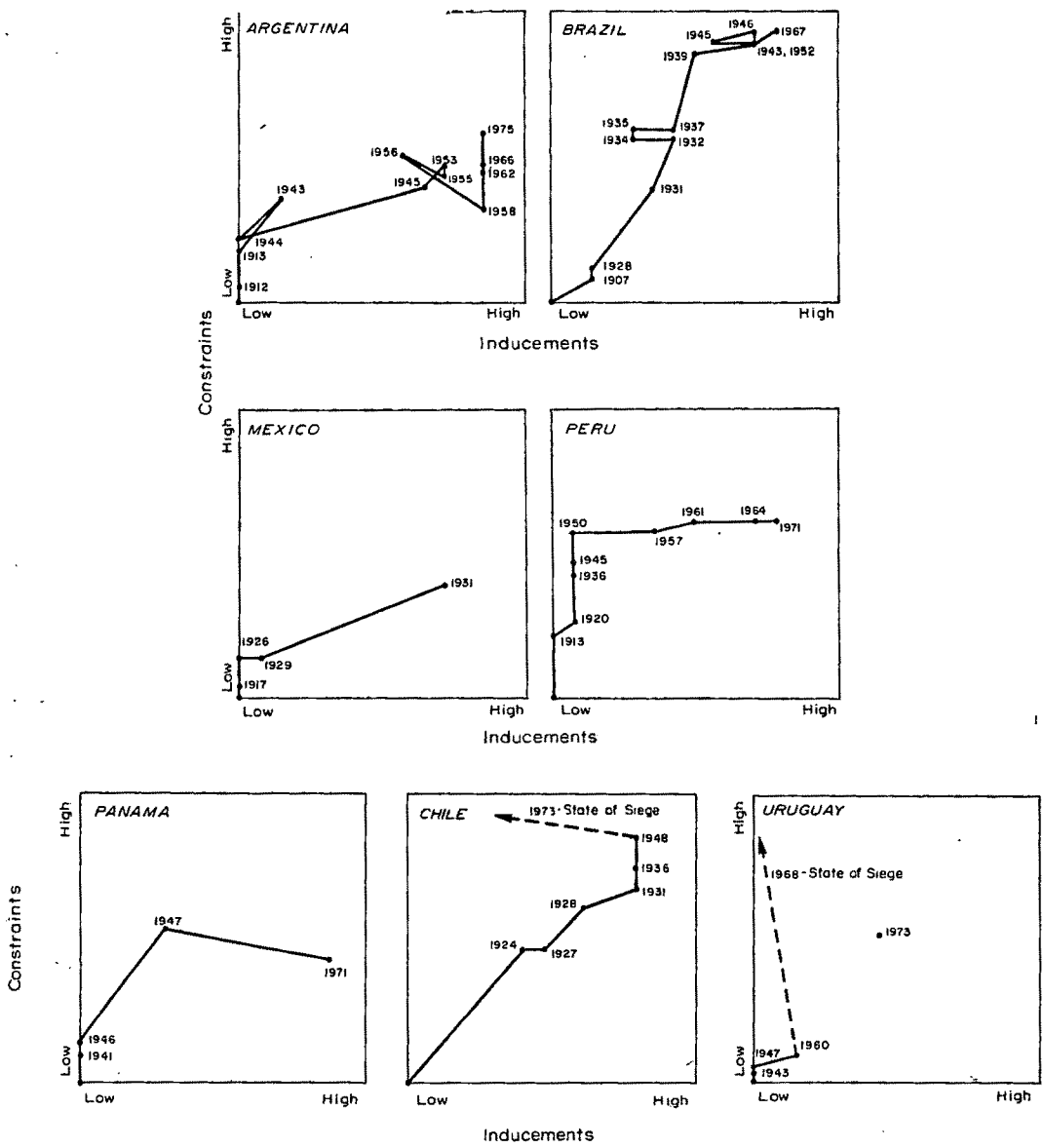
The link between the larger political context and the pattern of inducements and constraints is clear in other cases as well. In Mexico, the

<sup>8</sup>This discussion of Argentina draws on Kenworthy, 1970, Ch. 5; Silverman, 1967, pp. 134 ff., 194–210, 221; Baily, 1967, Ch. 7; and Most, 1978, Ch. 4.

<sup>9</sup>This discussion of Brazil draws on Skidmore, 1967; Harding, 1973; Schmitter, 1971; and Erickson, 1977.

labor movement has since an early phase of the Mexican Revolution been an important, though co-opted, actor within the dominant national coalition (Brandenburg, 1964; Everett, 1967; Stevens, 1974; Purcell, 1975). Correspondingly, the first major national labor law in 1931 placed heavy emphasis on inducements. Since

then, labor law, like the dominant political coalition itself, has been relatively stable. The clearest case of a separate elaboration of constraints and inducements and of a dramatic "right-angled" shift from one to the other is Peru. Until the late 1950s, Peruvian labor law had been shaped by a series of conservative



Source: Scored on the basis of data derived from I.L.O., 1919-1975 and 1930; other I.L.O. sources; and the statutes of individual countries.

Figure 1. Patterns of Change in Inducements and Constraints, 1901 to 1975

governments concerned with curbing the powerful, labor-based Apra party. Labor law was heavily oriented toward constraints, and the 1950 law in Peru was nearer the "high constraint/low inducement" corner of the diagram than any other labor law in Latin America.<sup>10</sup> Starting in 1957, after Apra entered into an alliance with the Peruvian elite, there occurred a series of increases in inducements, with little further increase in constraints. Another case of a particularly dramatic shift to the inducement side is Panama, which until the 1970s had a constraint-oriented law. In 1971 the populist/nationalist government of Torrijos made a strong appeal for labor support (*Latin America Political Report*, 1970–1971, passim) and promulgated a law which decreased the level of constraints at the same time that it increased inducement provisions to a level as high as any in Latin America.

Though the recent history of Chile saw the emergence of a powerful political left that enjoyed crucial support from the working class, the standard interpretation of the earlier history of state-labor relations emphasizes the preemptive, co-optive role of the state in attempting to create a weak, dependent labor movement (Morris, 1966; Peppe, 1971; Angell, 1972; and Valenzuela, 1976). Correspondingly, Chile—like Brazil—followed in this earlier period a relatively balanced pattern of inducements and constraints and moved to a high level of each. In 1973, with the fall of Allende and the onset of the violently anti-labor policies of the military government, the existing system of inducements and constraints was superseded by a state of siege and a variety of other legal and extra-legal measures, and Chile shifted to a system that predominantly involved repression, with little use of co-optation during the first years of military rule (I.L.O., 1975). This in effect involves an extreme movement upward and to the left in the diagram. Another example of a dramatic shift of this type is Uruguay. After many years as the most pluralistic system in Latin America with few legal provisions relating to labor organizations, Uruguay adopted in 1973, in the midst of a severe political crisis, a law that had a rough balance of inducements and constraints within the framework of a relatively low overall level of corporatism. However, state-labor relations have in fact been governed almost continuously during the violently anti-labor period since 1968 by the legal framework of a state of siege (Handelman, 1977, p. 11).

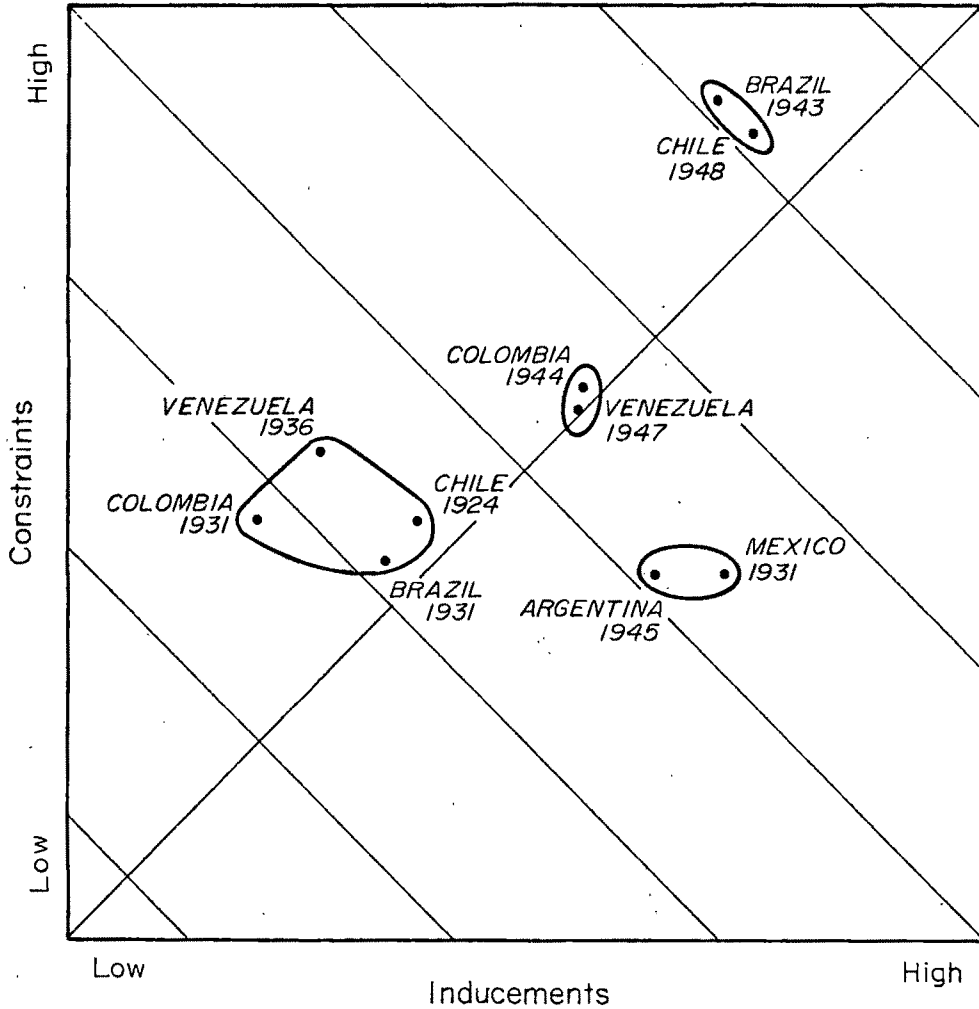
<sup>10</sup>This discussion of Peru draws on Sulmont, 1975, pp. 188–89, 239; Bourricaud, 1967; and Payne, 1965.

**Comparisons across Countries.** The comparison of major laws in Argentina, Brazil, Chile, Colombia, Mexico, and Venezuela sheds further light on the relationship between the larger political context and different patterns of inducements and constraints.<sup>11</sup> Four of these laws (1924 in Chile, 1931 in Brazil and Colombia, and 1936 in Venezuela) were for each country the first major labor law that legalized the trade union movement. These laws were adopted by governments that might be called "conservative modernizers." In these cases, a multi-class, modernizing coalition came to power, but organized labor did not play a critical role in building the coalition or providing support for it. Instead, the coalition derived its legitimacy largely from other sources. These labor laws tended to be the vehicle through which the government addressed the "social question." These governments sought to limit the radicalization of the working class by addressing themselves to the worst abuses to which this class was subjected and by seeking to integrate labor into the established order within a framework of substantial emphasis on constraints. This grouping includes the laws promulgated under the Liberal party in Colombia, which in 1930 came to power after a long period of Conservative party rule; López Contreras in Venezuela, who came to power after Gómez's repressive, dictatorial rule; and Vargas in Brazil and Alessandri in Chile, both of whom came to power at a point when traditional oligarchic rule had broken down and become discredited.

These four laws have relatively similar levels of inducements and constraints (see Figure 2). The laws did not contain as many provisions for either inducements or constraints as the later laws, and like many early laws placed a greater relative emphasis on constraints than on inducements. Hence they are located above (i.e., on the constraint side of) the hypothetical line of relative "balance" in Figure 2 (see note at bottom of figure).

<sup>11</sup>We selected the larger, more industrially advanced countries of Latin America, in part because they represent a coherent group and in part because we have analyzed them closely elsewhere (see R. Collier, 1978; and D. Collier, 1978). In addition to the sources cited above, the discussion in this section draws on Ashby, 1967; Blank, 1973; Brown, 1964; Caicedo, 1971; Cornelius, 1973; Dix, 1967; Drake, 1971; Fluharty, 1957; Imaz, 1967; Levine, 1973; Martz, 1966; Michaels, 1966; Nunn, 1970; Powell, 1971; Ruiz, 1976; Spalding, 1977; Stevenson, 1942; and Urrutia, 1969.





Source: Scored on the basis of data derived from I.L.O., 1919–1975 and 1930; other I.L.O. sources; and the statutes of individual countries.

\*Parallel lines from upper left to lower right correspond to equal levels of inducements *plus* constraints. In one sense, they could thus be interpreted as reflecting the “over-all” level of corporatism. The single line from lower left to upper right can be used as a basis of reference in interpreting the relative “balance” between inducements and constraints. This is not to say that laws which are close to this line are in some substantive sense “balanced.” Rather, comparing laws in terms of their distance from this line provides a basis for assessing the *relative* balance between inducements and constraints.

Figure 2. Inducements and Constraints in Selected Major Laws\*

The second group of cases includes Colombia and Venezuela at a subsequent point in time (1944 and 1946, respectively). Here the labor movement was also weak, but the activation of labor played a more central role in legitimating the dominant coalition. These “populist” coalitions were put together from above by Acción Democrática in Venezuela and the pro-labor

wing of the Liberal party in Colombia. Both of these parties sought to create and mobilize an organized labor sector, and both were dependent on labor as an essential support group for the government. Correspondingly, the new laws in both countries involved primarily the addition of new inducement provisions, and both countries moved from their earlier position in

Figure 2 over to the "balance" line, reflecting a greater relative emphasis on inducements in relation to constraints. At the same time that the addition of inducements is consistent with the greater dependence of these governments on labor, their level of constraints is consistent with the fact that these are far from radical, labor-dominated governments. Rather, they were multi-class coalitions dominated by middle-sector groups who needed the mobilization and support of labor.

The most inducement-oriented group includes Argentina and Mexico, the two countries which had the strongest labor movements when the major laws were promulgated. While the populists in Colombia and Venezuela were trying to create a labor movement which would form a support group for the party, in Mexico and Argentina the task was to gain or sustain the support of an already existing and relatively powerful labor movement. The early political strength of organized labor in Mexico grew out of the role of the Red Battalions in the Mexican Revolution, the ideology and expectations that derived from the 1917 Constitution, and the subsequent role of labor as a major, though co-opted, support group for the early revolutionary governments. The strength of Argentine labor derived from quite different sources. In this case, the major law came late—both chronologically and, even more so, relative to the level of industrialization. As a result, when Perón appeared on the scene, he faced a labor movement which, though subjected to repression, had for many years been developing autonomous associations. Correspondingly, the legal relationships which emerged in these contexts in which organized labor was relatively strong were different from those in the other four countries. One finds relatively low levels of constraints and high levels of inducements.

The final group includes Brazil and Chile, which started out—along with Venezuela and Colombia—in the group of conservative modernizers. Unlike these latter two countries, however, Brazil and Chile did not subsequently have comparable populist periods in the 1940s.<sup>12</sup> Instead, they continued and further

elaborated their earlier efforts to co-opt and control the labor movement. Correspondingly, in terms of both inducements and constraints, Brazil and Chile had by the 1940s moved to the highest levels in the region.

**Identifying Patterns.** On the basis of these findings, it is possible to identify recurring patterns in the relationship between the political context and different combinations of inducements and constraints. Salient features of the political context include the degree of elite concern with winning the political support of organized labor, the degree of concern with controlling labor, and the strength and autonomy of the labor movement.

It appears that a higher level of inducements and a lower level of constraints tend to occur in contexts in which the government seeks to gain or retain the political support of labor and in which unions are relatively powerful and/or autonomous. In these cases labor has a greater capacity to resist the imposition of constraints and/or the state has a greater need to extend inducements in order to gain the support and cooperation of labor.

A higher level of both inducements and constraints is more likely in contexts in which the government is less concerned with gaining labor's support and more concerned with controlling labor through creating organizationally viable unions that are co-opted by and dependent on the state. This is often done to preempt the emergence of autonomous unions that are not dependent on the state.

A higher level of constraints combined with a lower level of inducements tends to appear in contexts in which the primary concern of the government is with control, to such an extent that it does not seek even the passive support from organized labor that it may receive in the other cases and does not mind risking the outright opposition of labor. Rather than rely-

<sup>12</sup>The analysis of Brazil presented above referred to a shift to more populist policies in 1943. However, this was hardly comparable to these periods in Venezuela and Colombia. Chile likewise had a certain type of populist period in the form of the Popular Front government which came to power in 1938. What distinguishes Brazil and Chile from the other cases is the absence of a major mobilization of popular sector groups by a centrist party. In the other countries, the mobilization of the popular sector

culminated in a period of "radical populism" in which the interests of the popular sectors were championed and the conservative elements (major elite economic interests) within the party became alienated and went into opposition. The result of such populist periods was polarization along class lines. In Brazil and Chile the subsequent "populist" periods (at least through the 1950s) were quite different, involving a coalition of parties based on agreement among party leaders rather than a popular sector mobilization by the dominant party. As a result, the party did not move to the left in the same way, there was less polarization, and conservative interests were generally better served during these periods in Brazil and Chile.

ing on co-optation, this control is based primarily on direct constraints on unions and is backed by considerable force and repression. This pattern is seen in contexts in which labor is strong, as when an extremely anti-labor government attempts to deactivate and impose severe controls on a highly developed labor movement.

Most cases do not, of course, fall at the extreme values of either dimension. For the numerous intermediate cases, however, this discussion of patterns nonetheless points to some of the underlying issues that lead countries to position themselves at different points along these dimensions.

In summary, whether one considers patterns of change within countries or comparisons across countries, one may reasonably argue that there is an important relationship between different patterns of inducements and constraints and different political contexts. Whereas with a unitary concept of corporatism one could only note changes in the overall level of corporatism, this disaggregated approach permits more differentiated observations and comparisons concerning what the government is doing and what is happening to organized labor.

#### Conceptualizing State-Society Relationships

We have suggested that recent discussions of corporatism have played a central role in the renewed effort to discover more adequate ways of conceptualizing alternative patterns of state-society relations. How does a focus on inducements and constraints contribute to this larger effort?

First, this focus has the advantage of being interactive, of pointing to an implicit or explicit bargain or transaction that is struck at a particular time, reflecting the existing constellation of power relationships and the goals of relevant actors. This idea of a bargain is not intended to imply that the corporatized group, such as labor, is always actively involved in a formal process of bargaining. In many cases, labor is only a passive participant, and the degree to which labor is actively involved is indeed one of the factors that affects the balance that is struck between inducements and constraints.

Second, this interactive approach is dynamic in that it encourages the analyst to look for patterns of change over time. Once one has conceptualized state-group relations in terms of a bargain or transaction that reflects the existing configuration of power relationships and political goals, it become obvious that in the

context of changing power relationships and changing goals, the terms of the bargain may be renegotiated. This tendency is well illustrated by the volatile pattern of change in inducements and constraints in Argentina noted above.

Third, because the political role of organized labor has been treated as a central issue in recent efforts to develop broad typologies of national political systems in Latin America, the distinction between inducements and constraints can make a useful contribution to refining these typologies. For instance, authors such as O'Donnell (1973, 1978) argue that the repressive authoritarian governments which have recently emerged in the industrially more advanced countries of Latin America represent a new type of political system—referred to as “bureaucratic-authoritarian.” These systems are seen as involving a complex constellation of characteristics, including the political and economic “exclusion” of organized labor, i.e., the exercise of strong control over both the organizations and the income of this sector.

More recent studies have suggested that while the concept of bureaucratic-authoritarianism has made a major contribution, a more adequate analysis can be achieved if this concept is disaggregated and important variations among its component elements are examined separately (D. Collier, 1978; Cardoso, 1979; and Kaufman, 1979). The distinction between inducements and constraints provides a useful starting point for carrying out a disaggregated analysis of a crucial feature of bureaucratic-authoritarianism: the approach adopted for controlling organized labor. This may be illustrated through a comparative discussion of four contemporary cases—Brazil, Mexico, Chile, and Uruguay—which have been identified as bureaucratic-authoritarian.

In Brazil, the high-inducements/high-constraints pattern noted in Figure 2 persists. That is to say, the state exercises sharp control over labor organizations, in important measure through attempting to maintain organizationally viable unions that are co-opted by and dependent on the state—but that are of virtually no importance as coalition partners for the government. Though important periods of worker protest in both the late 1960s and late 1970s threatened this system of control, it has been the predominant approach in the post-1964 bureaucratic-authoritarian period (Erickson, 1977). Contemporary Mexico has likewise been characterized as bureaucratic-authoritarian (O'Donnell, 1978), yet the distinct pattern of inducements and constraints in Mexico noted in Figure 2 persists to the present day. Wage

policy has been exclusionary—as reflected in the decline in the real income of workers after 1976. Yet in contrast to the Brazilian pattern, organized labor remains a crucial partner in the dominant coalition. In exchange for this support, major organizational inducements have been extended to labor leaders—for instance, in the post-1976 period—in order to secure co-operation with the wage policies.<sup>13</sup> Thus, the Mexican system has been characterized as involving “two carrots, then a stick” (Smith, 1979, p. 57). Finally, as we indicated in the discussion of Figure 1, in Chile and Uruguay one finds a still different pattern, involving a system of pure constraints. Here, the existing labor organizations played no role in the support coalition of bureaucratic-authoritarianism, and there was virtually no reliance—at least in the initial period of military rule—on an effort to maintain a system of organizationally viable, co-opted unions.

Different combinations of inducements and constraints are thus found in these countries, and the analysis of the relationship between these two dimensions helps to bring into focus important differences in the contemporary pattern of state-labor relations among these four cases. The analysis of differences such as these can play a useful role in achieving a more adequate description and dynamic analysis of the emergence and evolution of bureaucratic-authoritarianism.

A fourth issue concerns whether this distinction between inducements and constraints may be applied to the relationship between the state and other types of groups. In light of O'Donnell's (1977) important argument that corporatism is “segmental,” in the sense that it means different things for different class groups, this issue merits attention here. Certain aspects of structuring and subsidy are, of course, especially salient to labor organizations, and one should not assume that the particular inducements considered in this article will be equally relevant to all types of groups. For instance, because of the economic position of labor unions in society and the organizational requirements for effectively engaging in strikes and collective bargaining, such provisions as subsidy, monopoly of representation, and compulsory membership may be far more relevant to unions than to groups such as business

associations. However, though the particular inducements—and constraints—relevant to other types of associations may vary, it seems likely that at a more generic level, the perspective of viewing state-group relations in terms of an interplay between inducements and constraints will be relevant for other types of groups as well.

Fifth, though initially formulated with reference to Latin America, this focus on inducements and constraints can contribute to the broader comparative analysis of different patterns of state-society relationships. This broader application may be illustrated by examining the relationship between the inducements-constraints distinction and Schmitter's (1974, 1977) distinction between “state” corporatism and “societal” corporatism. He argues that the Latin American cases considered in this article, and more generally other cases of corporatism in the Third World and Southern Europe, involve *state* corporatism, in that the corporatized groups “are created by and kept as auxiliary and dependent organs of the state which founds its legitimacy and effective functioning on other bases” (Schmitter, 1974, pp. 102–03). Schmitter uses the expression *societal* corporatism, by contrast, to describe systems of post-pluralist interest representation in advanced industrial societies in which corporative patterns of state-group relations have emerged in contexts in which “the legitimacy and functioning of the state [are] primarily or exclusively dependent on the activity” of the corporatized groups. In the first case, interest associations are thus “dependent and penetrated”; in the second case, they are “autonomous and penetrative” (1974, pp. 102–03).<sup>14</sup>

The distinction between state and societal corporatism involves the same issues of power relationships and bases of political support that we have used as a basis for distinguishing among Latin American cases. Hence, while as a first approximation it seems appropriate to characterize state-labor relations in Latin America as

<sup>13</sup>See Stefanowicz, 1979. We acknowledge our debt to Susan Kaufman Purcell for calling our attention to the importance of these measures. It should be stressed that these particular inducements were not among those included in the scoring for Figure 1.

<sup>14</sup>Elsewhere Schmitter seems to point to a somewhat different definition. Whereas on pp. 102–03 he stresses these issues of dependency and autonomy, on pp. 103–04 he stresses the issue of whether corporative provisions are initiated from above or from below. The locus of initiative can vary independently from the pattern of dependency and autonomy. The question of the locus of initiative is obviously important, but within the framework of Schmitter's discussion it appears to be a subordinate issue. We therefore focus our analysis on the larger issue of autonomy and dependency.

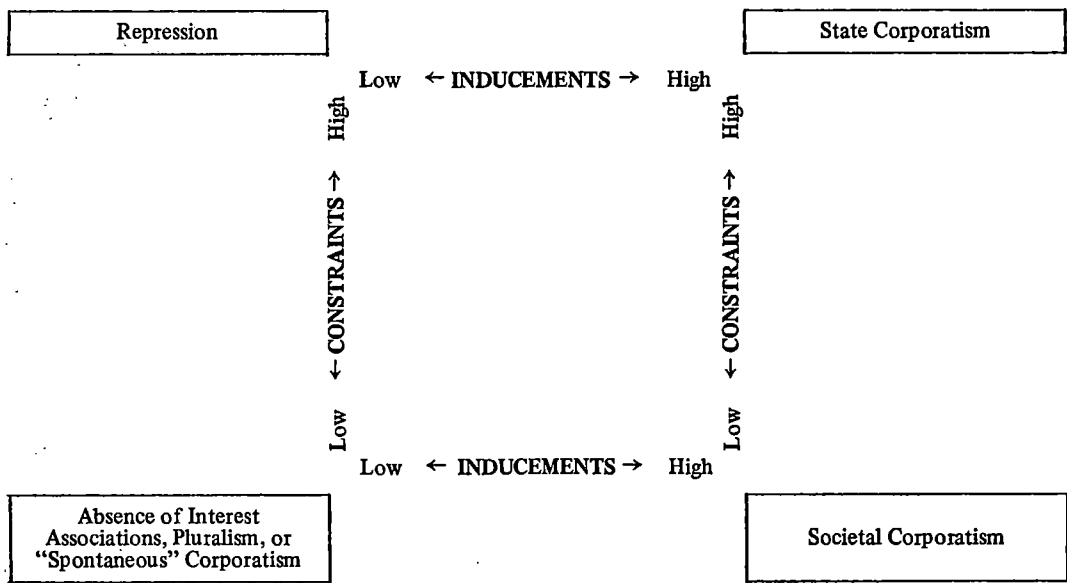
involving state corporatism, it is useful to go beyond this assertion and treat the distinction between state and societal corporatism not as a dichotomy but as a continuum, with some of the Latin American cases located at least part-way along this continuum toward societal corporatism. While Brazil stands out as one of the clearest cases of state corporatism, the discussion of the interplay between control and support in Argentina and Mexico suggest that these cases, at least during certain periods, are nearer to the middle of a state-societal continuum.

As one moves beyond the variations within Latin America to contexts that involve more nearly pure cases of societal corporatism, one might expect greater emphasis on inducements and less on constraints, since these would be contexts in which the state was more dependent upon the corporatized groups. These are commonly situations in which the state ratifies patterns of non-competitive interest representation that emerged "from below" involving strong, autonomous interest groups. Within the European context, a major inducement that has appeared in such cases has been the opportunity for certain groups to be represented on functionally organized, semi-public entities such as wage-price councils and eco-

nomic planning boards.<sup>15</sup> This contrast between the patterns of inducements and constraints that one might expect in hypothetically "pure" cases of state and societal corporatism is reflected in the right side of Figure 3.

The left side of Figure 3 suggests the hypothetical relationship between the other two combinations of "extreme" values of inducements and constraints and other types of group representation. High levels of constraints and low levels of inducements by definition involve a situation of outright control or repression of groups—cases of which have already been noted above. Low levels of inducements and constraints involve situations in which the state does not attempt to shape

<sup>15</sup>Schmitter excluded these entities from his definition of corporatism because he observed that they did not consistently occur in conjunction with the set of characteristics that he defined as corporatist (1974, p. 94, n. 24). We have likewise excluded them in the operationalization presented above, partly for the same reason and partly because of the extreme difficulty of establishing a reliable scoring procedure for such entities. For a discussion of their importance within the Latin American setting, see O'Donnell, 1977.



Source: Compiled by the authors.

Figure 3. Hypothetical Relationship between Inducements-Constraints Distinction and Broad Types of Group Representation

interest politics through these kinds of provisions aimed at interest associations. This pattern may correspond to situations in which interest associations do not exist, to situations of pluralism, or to situations in which pluralism may have been eroded "from below" through the oligarchic tendencies of group interaction but in which the state has not become involved in ratifying or reinforcing this erosion of pluralism. This is a substantial "residual category," which reflects the obvious fact that a focus on inducements and constraints does not allow one to distinguish among all different patterns of group representation. This focus does, however, provide a useful perspective for looking at variations in the state role in group representation.

In addition to stressing the utility of this distinction between inducements and constraints, we should note the limitations of this perspective as it has been presented here. First, the particular operationalization of inducements and constraints we have proposed is not intended to encompass all of the inducements and constraints formalized in labor law—not to mention those found in other areas of law or those not embodied in law at all. The purpose of this operationalization is to show that there are certain recurring patterns of inducements and constraints. The particular scoring of inducements and constraints presented above is not intended to be a definitive assessment of these two dimensions.

Second, the analysis has focused on two crucial actors—the state and labor organizations. Obviously, other actors are closely involved in the interplay of inducements and constraints that we have analyzed—most immediately the workers themselves (as opposed to labor *organizations*) and employers. What is ultimately called for is a far more complex analysis that encompasses, at the very least, all four of these actors. Thus, in one context, the state may extend important benefits to labor organizations to strengthen the position of these organizations and of workers vis-à-vis employers. In another context, both the state and employers may extend inducements to labor organizations in order to secure their aid in enforcing regressive income policies on the workers. At this level, highly complex relationships may be involved. The goal of the present analysis has simply been to propose a conceptual distinction that will make it easier to analyze these more complex patterns.

Finally, this attempt to conceptualize more adequately different patterns of state-society relations is a building-block in a larger analytic effort in another sense as well. A more sharply

focused *description* of these relationships should ultimately contribute to a more adequate *explanation* of differences among countries and change over time within countries. It should help to address questions such as: Why is the pattern of state-labor relations that emerges at the time of the initial "incorporation" of organized labor sustained over many years in some countries, whereas in others it is not? Why have such different systems of labor control recently emerged in the context of "bureaucratic-authoritarianism," and what are the implications of these different systems of control for the ability of labor to achieve a more favorable distribution of political power and economic resources?

The answers to these questions can help us to understand certain anomalies in the long-term patterns of change followed by different countries. For instance, Figure 2 pointed to similarities in patterns of inducements and constraints at an earlier point in this century between Argentina and Mexico, on the one hand, and between Brazil and Chile, on the other. Yet in the more recent period, if one examines the degree to which different countries have well-institutionalized systems of labor control, there appears to be a significant regrouping of cases. It might be argued that Mexico and Brazil now have more institutionalized systems of control, whereas Argentina and Chile have less well-institutionalized systems of control. How does one explain this shift? What are the "transformation rules" that account for these changing patterns? The analysis of inducements and constraints will have proved useful if it can help to answer questions such as these.

#### Appendix A.

##### Overview of Legal Provisions for Inducements and Constraints

#### Inducements

**Registration.** The first inducement, both in terms of the timing of its appearance in each country and in terms of the low level of "corporatism" it represents by itself, is the registration or official sanctioning of unions by the state. This has appeared in every country in Latin America. Registration confers specified rights, including typically juridical personality and the right to represent workers' interests before the employer and before the state. Prior to the appearance of registration in each country, unions had either been repressed or could become legally incorporated only under general legislation concerned with freedom of associa-

tion. In some important cases this mode of incorporation was not an attractive alternative to unions because the unlimited civil liability it imposed appeared undesirable in contexts in which unions might be held responsible for damages incurred during a strike (I.L.O., 1930, p. 163). The provisions for official registration considered here created a separate type of incorporation that was particularly suited to the needs of unions. The point at which registration first appears is, in most cases, the point at which labor law first emerges as a distinct body of legislation designed to encourage the formation of worker organizations.<sup>16</sup>

**Right of Combination.**<sup>17</sup> These provisions facilitate the formation of unions, primarily by protecting them from various forms of harassment by employers. Because they protect the right of unions to exist as organizations, these provisions might be seen as providing a basis for either corporatism or pluralism. However, the notion that the state has to intervene actively in society in order to make it possible for workers' organizations to exist already takes one beyond the vision of interest politics contained in the conventional version of pluralism and into the sphere of state involvement in shaping interest politics that is the focus of our analysis.

**Monopoly of Representation.** This involves the issue of the degree to which there is an absence of competition among unions to represent workers in a particular occupational grouping. The relevant provisions range from the few cases in which several unions within a given occupational grouping are allowed to compete for members and to bargain with employers to the exclusive granting of the right of representation to a single union.

**Compulsory Membership.** This rarely exists in the form of an outright legal requirement that all workers must become union members. Ra-

ther, one finds a series of partial approximations. Some countries have legal provisions that permit collective bargaining agreements to include clauses requiring all workers to be union members. In other cases there are requirements that nonmembers be subject to the same authority or obligations as members with regard to specific issues. For instance, collective bargaining agreements may apply to nonmembers as well as members, or nonmembers may be subject to a dues check-off.

**Subsidy.** Worker organizations are of course subsidized in a great variety of ways, both formal and informal. The provisions of interest here are those which involve the state directly in helping unions receive a regular source of revenue.

### Constraints

**Collective Bargaining and Strikes.** Collective bargaining over wages and working conditions is one of the most important areas of activity and demand-making of labor organizations, and the strike is labor's most important weapon. One of the most significant types of state control involves intervention in collective bargaining to avoid class conflict and the disruption of economic activity. In recent years the state has become involved in setting wages and in decreeing other policies that have removed much of the substance of collective bargaining to the area of administrative and/or judicial decision. This state role has become so complex that it was not feasible to score all aspects of it, particularly the state role in wage-setting. However, the provisions considered in the present analysis, which regulate the conditions of collective bargaining and the use of the strike, represent a large proportion of the important regulations in this area, particularly for analyzing the earlier phases in the emergence of labor law in Latin America.

**Other Constraints.** A series of additional constraints are commonly found which (a) limit the kinds of demands unions may make and the kinds of demand-making activities in which they may engage; (b) control leadership and leadership recruitment with the goal of restricting the role of radical political groups, or earlier in this century, radical foreign immigrants; and (c) give legal sanction to direct state monitoring and intervention in internal union affairs.

<sup>16</sup>In light of the importance of registration, it might have been given more than a single point in constructing the scales (see Table 1). However, because all countries adopted it at a relatively early point in the evolution of their labor legislation and because no countries rescinded it, this somewhat arbitrary choice to give it only one point had little effect on the analysis presented below.

<sup>17</sup>For a discussion of the "right of combination" of workers, as opposed to the "freedom of association" of groups in general, see Bendix (1964, pp. 80-87).



### Appendix B. Coding and Scaling of Labor Law

Table 1 presents an outline of the categories used in the actual coding. The first step in scaling the data generated by this coding involved constructing "sub-scales" that corresponded to the nine headings in Table 1. Considering first the five subscales concerned

with registration, monopoly of representation, compulsory membership, subsidy, and control of collective bargaining and strikes, one can see that there is an inherent ordering among the categories under each of these headings and that at any given time, each country can logically be in only one of the categories under each heading. Hence, as they stand, the categories under each of these five headings repre-

Table 1. Outline of Categories Used in Scoring Labor Law

Inducements	
1. <i>Registration.</i>	Countries were scored zero or one according to whether there was a provision for the registration of labor unions.
2. <i>Right of Combination.</i>	Countries were scored zero or one according to whether they had each of the following provisions: <ul style="list-style-type: none"> <li>a. Employers may not make membership in a union an obstacle for obtaining or retaining employment.</li> <li>b. Employers cannot refuse to participate in collective bargaining with a legitimate representative of the workers.</li> <li>c. Unions may form into federations and confederations.</li> <li>d. Employers may not be organizationally involved in unions.</li> <li>e. Union leaders have some form of job security.</li> </ul>
3. <i>Monopoly of Representation.</i>	An ordered scale was constructed on the basis of the following values: <ul style="list-style-type: none"> <li>0 = No provision.</li> <li>1 = Either multiple, competing unions are allowed to register and to represent members and bargain collectively, or there is the single restriction that for the purpose of collective bargaining, competing unions must form an inter-association committee.</li> <li>2 = Competing trade or works unions may exist, but only the largest union among those representing the same sector may enter into collective agreements.</li> <li>3 = Within a given sector, only singular unions are permitted.</li> <li>4 = Prohibition of all unions except for one works union per enterprise.</li> </ul>
4. <i>Compulsory Membership.</i>	An ordered scale was constructed on the basis of the following values: <ul style="list-style-type: none"> <li>0 = No provisions.</li> <li>1 = Either dues collection (or its equivalent) or collective bargaining agreements apply to both members and non-members.</li> <li>2 = Both dues collection (or its equivalent) and collective bargaining apply to members and non-members.</li> <li>3 = Closed shop or union shop clauses are permitted in collective bargaining agreements.</li> <li>3.5 = Once a union is legally formed, membership is compulsory.</li> <li>4 = Legal requirement of universal membership.</li> </ul>
5. <i>Subsidy.</i>	An ordered scale was constructed on the basis of the following values: <ul style="list-style-type: none"> <li>0 = No provision.</li> <li>1 = Dues check-off permitted if union requests it.</li> <li>2 = Dues check-off combined with closed shop provision or compulsory membership, or dues check-off that applies to members and non-members.</li> <li>2.5 = Some form of on-going, state involvement in the financing of unions, such as the provision in Chile for a form of profit-sharing in which a portion of the profits of the enterprise are paid to the union via a state agency.</li> <li>3 = Syndical tax.</li> </ul>

(continued on next page)



Table 1 (continued)

Constraints
1. <i>Collective Bargaining and Strikes.</i> An ordered scale was constructed on the basis of the following values.
0 = No provision.
1 = Voluntary conciliation and/or arbitration preceding a strike.
2 = Compulsory conciliation preceding a strike.
4 = Compulsory arbitration to be initiated at a specified time after a strike begins.
5 = Compulsory arbitration before a strike.
7 = Strikes outlawed.
Because there are often special conciliation, arbitration, and strike provisions that apply to the public sector, to public services, or to other sectors viewed as crucial to the economy, additional points were added to this score as follows: one point if strikes were outlawed in the public sector; one point if strikes were outlawed in public services or other "strategic" sectors; half a point if the score for the public sector on the above scale was higher than the score for industrial disputes in general (unless it was equal to seven, in which case it got a whole point); and half a point if the score for public services or other strategic sectors on the above scale was higher than the score for disputes in general (again, unless it was seven). The highest possible score was thus nine.
2. <i>Other Constraints on Demand-Making.</i> Countries were scored zero or one according to whether they had each of the following provisions:
a. Collective bargaining agreements must be approved in order to be legal.
b. Political activities of unions prohibited.
c. Political and/or solidarity strikes prohibited.
d. Boycotts and/or picketing prohibited.
e. Calling an illegal strike constitutes grounds for dissolving a union.
f. Union officials can be removed for calling an illegal strike.
3. <i>Leadership.</i> Countries were scored zero or one according to whether they had each of the following provisions:
a. Salaries of union officials regulated.
b. Union officers must be workers in the occupational grouping which their union represents.
c. Union leaders must meet citizenship and/or residence requirements.
d. Political activists associated with certain political parties or ideologies are excluded from union office.
4. <i>Internal Governance.</i> Countries were scored zero or one according to whether they had each of the following provisions:
a. The state can audit union financial records.
b. Expenditures of union funds regulated.
c. State official may attend union meetings.
d. State authorized to assume direct control of unions (to "intervene" them).
e. State may dissolve unions. For this crucial provision a score of zero was assigned if there was no provision; a score of one if it could be done by judicial decision only; and a score of two if it could be done through a more discretionary administrative or combined administrative and judicial decision. A further point was added if the permissible causes for dissolution went beyond narrow procedural criteria to include broader political criteria. The maximum possible score for this trait was thus three.

sent unidimensional scales.<sup>18</sup>

The provisions under the remaining four headings—right of combination, other con-

p. 646) of assigning slightly different intervals on the basis of a substantive interpretation of the relative "distance" between the different provisions. If slightly different choices were made regarding these weightings, or if no weightings were used, it did not significantly change the results of the scaling analysis.

<sup>18</sup>For three of these five scales (see Table 1) we followed the practice recommended by Tufte (1969,

straints on demand-making, leadership, and internal governance—are not inherently mutually exclusive. Rather, it is logically possible for any one country to have several of the provisions simultaneously. Guttman scaling was therefore used to discover if cumulative, uni-dimensional scales were present. The provisions did in fact follow a Guttman scale pattern,<sup>19</sup> and for each of the scales each nation was given a score according to the number of provisions in the scale that it possessed.

The relationships among these nine subscales were then analyzed to determine the degree to which it was statistically appropriate to form an overall index of inducements and of constraints. On the basis of cluster and factor analysis we concluded that there were strong associations both among the five subscales under the inducement heading and among the four subscales under the constraint heading, and that it was therefore appropriate to group the scales in this way.<sup>20</sup> Aggregate measures of

inducements and constraints were then derived on the basis of these analyses.<sup>21</sup> It should be noted that the composite scales of inducements and constraints are themselves strongly correlated (.84). Hence, it would also be meaningful to form an additional composite scale that combines all nine subscales which could be referred to as an overall measure of "corporatism." In any particular analysis, whether one analyzes the two dimensions separately or together thus depends on the theoretical concerns of the study. In the present article, our goal is to discover whether interesting patterns emerge if these two logically distinct dimensions are treated separately, and therefore we have employed them as separate indices.

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<sup>19</sup>Guttman scale analysis was selected as a technique for aggregating the component elements of these subscales because of an important property of these elements that was quickly evident from an inspection of the data. It was clear that while some of the elements were "easier," in that they appeared earlier and in more countries, others were "harder," in that they appeared later and in fewer countries. This resulted in a "non-linear" pattern of relationships among the elements that made statistics such as product-moment correlations or tau less appropriate. The question remained, however, as to how regular this ordering from "easier" to "harder" in fact was. This is precisely the question addressed by Guttman scale analysis. The elements involved in the four subscales under consideration here do, in fact, follow a fairly regular Guttman scale pattern. The coefficients of reproducibility are in all cases above .92 and the coefficients of Guttman scalability are above .8 for two of the scales, .72 for the third, and .63 for the fourth.

<sup>20</sup>The cluster analysis was based on the coefficient gamma, which was deemed appropriate because of the "nonlinear" property of the relationships referred to above in the discussion of Guttman scale analysis. Among the inducements subscales the mean value of gamma was .82, with the lowest coefficient being .74. Among the constraints subscales the mean value was .80, with the lowest coefficient being .69. Though the data do not meet all of the assumptions of factor analysis, it is useful to note as well the findings derived from this technique because the percent of variance explained by the first general factor provides a convenient summary of the degree to which a set of items go together. For the inducements subscales, the first general factor (using the principal factor method with iteration) explained 73 percent of the variance, with the lowest loading being .74. For the constraints

subscales, the first general factor explained 82 percent of the variance, with the lowest loading being .81.

<sup>21</sup>Cases were assigned overall scores for inducements and constraints by summing the appropriate subscales. Though, as noted above, the data do not fully meet the assumptions of factor analysis, it is again useful to note the results that would have been provided by this alternative method, since they suggest that the findings reported here are not narrowly dependent on any one technique. The general factors that were extracted (see above) both had correlations of over .96 with the combined scales that were formed by summing the subscales.

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# Criticism, Cynicism, and Political Evaluation: A Venezuelan Example

ENRIQUE A. BALOYRA  
*University of North Carolina, Chapel Hill*

*Political trust and political efficacy are concepts currently undergoing considerable discussion and revision. The discussion of voting behavior in Campbell et al. (1954), Almond's and Verba's (1965) version of the participation hypothesis, Gamson's (1968) trust-efficacy hypothesis, and Verba's and Nie's (1972) standard socioeconomic model represent quite distinct and contrasting stages in the treatment of these concepts, but share some assumptions requiring additional discussion and clarification. One of these pertains to treatment of political criticism, measured with low scores of trust in government, as political cynicism. This is not so much an operational or methodological question as a theoretical framework that has never had much to say about the role of criticism in contemporary democratic societies. This essay proffers a case in which, high levels of interpersonal distrust and high levels of government criticism notwithstanding, it is possible to make this basic distinction. Path analyses of data from a national-sample survey show that criticism is triggered by predominantly political orientations, while cynicism is a rigidified form of criticism inseparable from the social circumstances of individuals. In addition, the evidence suggests that the sense of political efficacy does not play the pivotal role assigned to it in the literature.*

Discussions of political criticism seldom take into account the *context* in which individuals relate to the political system. Instead, the paradigm dominating the literature until recently (Almond and Verba, 1965; Campbell et al., 1954) assumed that political criticism is part of a cluster of attitudes characterized by political apathy, low "trust in government," and a low "sense of political efficacy" (see Balch, 1974, p. 2 on Campbell et al., 1960; Verba and Nie, 1972). Criticism of political institutions was believed to originate in a marginal socioeconomic status which obscures or prevents clear understanding of the workings of the political system, or in a very atypical pattern of socialization which, coupled with somewhat exotic ideological orientations, considerably distort an individual's perception of the system. Therefore, criticism has been treated as a symptom of individual frustration, and not as a result of shortcomings in the performance of the political system which may be perceived by individuals. Whether conceived in terms of the "participation hypothesis" (Almond and Verba, 1965; Verba and Nie, 1972), or of the "trust-efficacy hypothesis" (Gamson, 1968; Fraser, 1970; Sel-

igson, 1977), the tendency has been to treat political criticism as uninformed (Converse, 1964) or subversive (Lane, 1962).<sup>1</sup>

Recent efforts to evolve more sophisticated measures of "sense of political efficacy" (Seligson, 1978), to evaluate the validity and reliability of the traditional measures of the sense of efficacy (Asher, 1974; Balch, 1974; McPherson et al., 1977), and to discuss the conceptual and practical implications of the decline of trust and efficacy scores in the United States (Miller, 1974; Citrin, 1974; and Niemi, 1975) suggest that a revision of the dominant paradigm is afoot. However, most of these contributions are concerned with attitudes assumed to accompany criticism, and not with criticism itself.

This essay is an attempt to introduce a new element into the ongoing revision, namely, the distinction between *criticism* (from Webster's: "judging with knowledge and propriety") and *cynicism* (again from Webster's: faultfinding, captious criticism). In trying to establish this distinction, I will treat both as aspects of political evaluation, albeit with different causal origins. More specifically, I interpret the evidence to be presented and discussed as showing that cynicism originates in the social circumstances of individuals, while criticism has explicitly political origin. The ability to make this distinction would, or perhaps should, have

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<sup>1</sup>Even Lane, who thought that ordinary citizens may come to the conclusion that the government is untrustworthy, treats political criticism as political alienation (1962, p. 473).

theoretical relevance in evaluating "the interaction between citizen and government . . . that democratic institutions become in practice" (Converse, 1975, p. 87). More importantly, the distinction should enable us to take political criticism out of the subversive context to which it has been relegated, and to facilitate interpretation of the functions of criticism in contemporary democratic societies (Inglehart, 1977, pp. 303-07).

In choosing the case of Venezuela to illustrate the distinction, I am not simply making opportunistic use of data collected there, in a national sample survey of 1,521 individuals interviewed in the fall of 1973.<sup>2</sup> To be sure, in using Venezuela I have a little more latitude to try to add a new outside element to a discussion taking place among students of public opinion in the United States. I can put some distance between this presentation and that discussion, pretending that I am dealing with a different case. However, the substantive issues remain the same, the items that measure criticism and cynicism for the Venezuelan case are essentially the same traditional measures of political trust and sense of efficacy found in the literature,<sup>3</sup> and the high levels of public criticism found in Venezuela demand a careful methodological treatment.

#### Context and Case: Political Criticism in Venezuela

Venezuela boasts a democratic regime anchored by a multiparty system dominated by the Social Christian (COPEI) and the Social Democratic (AD) parties. In the last three elections of 1968, 1973, and 1978, the incum-

bent party has lost the presidential race and power has been transferred without incident. The Left is not persecuted by "death squads" operating under official protection; instead, the Left is represented in Congress by three different parties which seek to transform "the establishment" by constitutional means. The reality of turnover is not lost on the electorate. Support for the institution of elections is very, very high and since 1958, when the present democratic regime was inaugurated, voting turnout has hovered around 90 percent (Martz and Baloyra, 1976, pp. 46-47, 281).

In Venezuela, democratic governments have done a better job than military regimes in maximizing fiscal revenues and allocating these resources to the development of the country's human and natural resources (Baloyra, 1974; Hanson, 1977; Kuczynski, 1977). However, there is great discontent with the performance of the managers of the regime. Many citizens find fault with the regime's policies, with the role of parties and politicians in the political process, and with the performance of the bureaucracy, although this criticism falls considerably short of generating support for a return of the military to power (Martz and Baloyra, 1976, 1976, pp. 48-51).

The fact that the Venezuelan democratic regime is able to function in a context of substantial public criticism suggests that such criticism is not so destructive, for if a relatively fragile and young regime can cope with critical opposition, then there is reason to believe that more institutionalized regimes can cope with highly critical, mobilized electorates. Therefore, as Citrin (1974) has suggested, it is not just the incidence of criticism—or cynicism, as he calls it—but, in addition, that organizational alternatives are available to critics. In Venezuela there are such alternatives.

However, it could be argued that the high levels of political criticism found in Venezuela, which is after all one of the more democratic and open societies of Latin America, are the result of cultural nuances and individual psychological states. For example, levels of interpersonal trust are very low in Venezuela. Most respondents in the 1973 sample believed that one should "peel the eye" instead of trusting others, that people mind their own business and are not helpful, and that if one is careless, others take advantage.<sup>4</sup> Scaling these responses with Guttman criteria, one finds that

<sup>2</sup>For a description of the research design of the study and the characteristics of the sample utilized, see Baloyra and Martz (1979, pp. 195-206).

<sup>3</sup>There is no question that extant measures of trust and efficacy could use some improvement. I am aware that Converse suspects the dimensionality of the scale of sense of political efficacy since the "voting is the only way" item no longer scales with the other three (1972). For the Venezuelan data to be utilized here, the four-item battery of efficacy items did not scale. Following Balch (1974, pp. 22-26) the items in "pair B," "government officials don't care" and "people like you have no say" were combined with two items dealing with the perceived influence (present and in the near future) of the respondent's reference group (people like yourself). Therefore, even though I use traditional items purported to measure the sense of political efficacy, I am sensitive to the problems discussed in the literature concerning the validity and reliability of the items.

<sup>4</sup>The percentage of respondents proffering these opinions are 91, 82 and 84 percent, respectively.

75 percent of the public belongs in the highest category of what has to be treated as a measure of interpersonal distrust. In addition, Venezuelan political culture has been described as imbued with a generalized sense of distrust and suspicion which lend it a paranoid character (Silva Michelena, 1970, p. 240).

These characteristics of the Venezuelan political context could, according to this alternative interpretation, explain why 56 percent of the public are very critical or critical of the policies of the democratic regime, why 49 percent are very critical of party politicians, and why 52 percent gave very low marks to the incumbent Christian Democratic administration.<sup>5</sup> The reasoning here would be that the high levels of criticism found in Venezuela are simply a reflection of generalized individual distrust which predisposes the Venezuelan public to adopt a very cynical perspective toward the political process. Therefore, cultural and psychological factors are the main determinants of the high levels of dissatisfaction found in Venezuela. By contrast, the interpretation followed in this article is that one should differentiate between cynicism and criticism in order to make such an appraisal, and that if one is to find any guidance in the literature concerning this distinction, one should attribute greater importance to the impact of cultural and psychological factors on cynicism, since political criticism is the result of predominantly political factors.

### Criticism and Cynicism in Venezuela

One way to make a distinction between cynics and critics is by their responses to statements containing sweeping generalizations. One can phrase items in a manner in which both the item and the individual are on the underlying attitudinal continuum. By phrasing items in a very extreme fashion one overfills the underlying attitudes and there is no one-to-one correspondence between response categories and the attitudinal continuum (Torgerson, 1957, p. 305). Other items measuring similar attitudes with less extreme language yield categories which can be ordered, assigning their boundaries to positions on the underlying continuum; by assigning the subjects to the ordered combinations of these boundaries one

builds a cumulative, unidimensional Guttman scale (Torgerson, 1957, pp. 302-04). One could identify critics as those respondents in intermediate Guttman-scale categories of measures of political evaluation and cynics as those who are in the more negative categories of such measures. In addition, cynics would be more likely to agree with extreme, maximum-probability items which cannot be fitted into Guttman scales.

Political criticism is measured with four Guttman scales. The first deals with criticism of the incumbent administration and includes opinion about the impact of administration policies on (1) "people like yourself," and (2) on the respondent's immediate family; (3) the sympathy felt by the respondent toward president Rafael Caldera; and (4) opinion about whether the administration of a different party could do a better job solving the nation's problems. A second Guttman scale measures criticism of the policies of the democratic regime from the standpoint of (1) whether the democratic governments of the last 15 years (1958-1973) have been beneficial to Venezuela, (2) whether they have spent public monies judiciously or have wasted them, (3) whether they have served the public interest or the interests of very powerful groups, and (4) whether they have been staffed with honest people or crooks. Criticism of the role of politicians is treated with a Guttman scale incorporating opinion about whether politicians (1) "worry about the problems of people like yourself," (2) "strive to solve the country's problems," (3) "perform a job of great benefit to the community," and (4) "talk much and do nothing." Finally, criticism of the institution of elections is measured with a Guttman scale which posits whether (1) "voting is a very important factor in politics," (2) "elections are necessary in order to have a democracy," (3) "elections force the government to worry about people's problems," and (4) "elected officials worry about the problems of the people who elected them." These four scales representing four different *modes* of political criticism will also be considered globally, combined into a composite index of political criticism. The distribution of respondents along the categories of the four scales of political criticism are reproduced in Table 1, below, together with the coefficients of reproducibility and scalability of each of the scales.

Political cynicism is measured with items gathering more extreme reactions to the political process. Although related to the continua of criticism, these items can and must be dealt with separately. For the data at hand none of

<sup>5</sup>For additional information concerning levels of dissatisfaction about different aspects of the political process in Venezuela, see Baloyra and Martz (1979, pp. 52-55).

the following items could be scaled with Guttman criteria: opinion about how frequently government officials make appropriate decisions, opinion about the capacity of government officials (bureaucratic competence), opinion about how much attention government officials pay to ordinary citizens (bureaucratic indolence), the belief that government would be better off without (the interference of) politicians, and the belief that parties only care about (winning) elections. These statements invite agreement with very cynical propositions, even in the very critical context of Venezuela, and seem to point to the conclusion that politics is a big waste of time. They will be treated as a composite index of cynicism, derived from a linear combination of the standardized scores of the five measures. Inter-item and scale-item correlations for the standardized scale of political cynicism are presented in matrix form in Table 2, below.

### Efficacy and Criticism

Most versions of the participation hypothesis treat the sense of political efficacy as a result of socialization experiences, including those reflected on the social circumstances of the individual and those stemming from contacts with public officials. The path diagram presented in Figure 1 deals with this aspect of the participation hypothesis. Sense of political efficacy appears to be determined, primarily, by the social circumstances of individuals and by one mode of political criticism—namely, criticism of government policies. The model suggests that upper-status individuals feel more efficacious, subject to some deflation as a result of aging. On the other hand, higher levels of criticism found among lower-strata individuals seem to decrease their sense of efficacy even further.

Table 1. Guttman Scale Measures of Political Criticism

Scales of	Missing Cases	Adjusted Frequencies				Coefficients of:	
		Very Low	Low	Moderate	High	Reproducibility	Scalability
Criticism of the Incumbent Administration	78	18.1	30.3	38.7	12.7	.91	.65
Criticism of the Policies of the Democratic Regime	299	23.6	20.0	27.0	29.4	.90	.67
Criticism of the Role of Politicians	149	25.5	20.5	19.5	34.5	.89	.70
Criticism of the Institution of Elections	142	43.7	29.7	18.6	6.2	.94	.73

Source: Computed from data collected by Baloyra and Martz (1979, pp. 195–206).

Table 2. Composite Index of Political Cynicism

	Incompetence	Indolence	Parties	Politicians	Scale-Item
Government (always) fails to do what is right (29.7)	.23	.21	.25	.14	.52
Bureaucratic incompetence (27.7)		.15	.22	.23	.63
Bureaucratic indolence (78.5)			.22	.22	.57
Parties only concerned with winning elections (70.9)				.24	.60
Government better off without politicians (60.8)					.56

Source: Computed from data collected by Baloyra and Martz (1979, pp. 195–206).

Note: Figures in parentheses represent the percentage of respondents expressing agreement with the most cynical option offered in the statements described in the text. All other figures are Pearson's correlation coefficients and are significant at the .01 level.



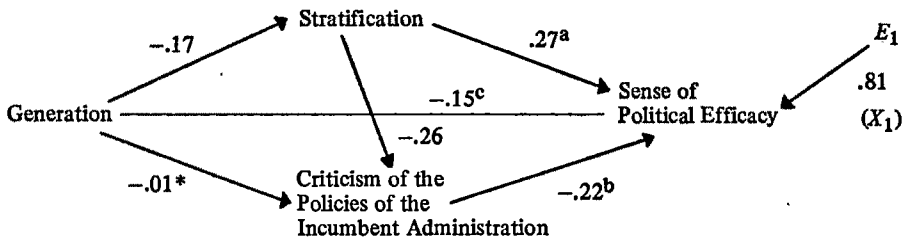
But, shouldn't the sense of efficacy determine criticism, instead of the other way around? In Venezuela, the sense of political efficacy depends on the individual's social circumstances and position in the life cycle; there is nothing unique about this. However, the sense of efficacy seems to depend also on the individual's evaluations of the policies of the incumbent administration independently of the former. Notice that the introduction of controls does not decrease the magnitude of the original correlation between sense of efficacy and criticism (−.26, at the bottom of Figure 1). Obviously, the sense of efficacy fluctuates with the level of criticism of the incumbent government regardless of one's age or social standing. Thus the degree to which individuals feel that they can influence the government is not a reflection of class ethos (optimistic/pessimistic) or generational perspective alone but also of one's evaluation of what the government is doing.

In the United States power is fragmented and individuals may acquire a sense of political efficacy with greater ease since there are more levels of government wielding effective power. In Venezuela, where power is more concentrated in the national executive, it may be more difficult to derive a sense of efficacy from contacts with public officials who do not have

so much power after all. Whatever the real import of the differences in context between the two countries, it is clear that in Venezuela an individual's sense of efficacy springs from a baseline of class experiences and maturation, subject to fluctuations produced by the individual's evaluation of the performance of the government. More importantly, the sense of political efficacy appears as subject to change as any other attitude, although constrained by the factors described above.

Additional evidence suggests that it is more appropriate to treat the sense of efficacy as dependent on criticism rather than the other way around. Involved here is the question of whether the sense of efficacy increases one's level of satisfaction with the system and also individual allegiance to the system as suggested by Almond and Verba (1965, p. 191). These propositions are tested with the path diagrams presented in Figure 2.

It should be emphasized that, with the exception of the model involving criticism of administration policies, sense of political efficacy was included among the set of possible explanatory factors (or "dependent variables") used for *all* models. However, sense of political efficacy does not appear in *any* of the models presented in Figure 2, which inventories the more relevant sources of these different modes



Source: Computed from data collected by Baloyra and Martz (1979, pp. 195–206).

Note: In this and subsequent figures I follow the rules of graphic representation for path analytic models, discussed by Kim and Kohout in Nie et al. (1975, pp. 389–92). All paths from exogenous (independent) variables to the dependent variable are standardized (beta) regression coefficients. All paths between exogenous variables are zero-order product-moment (Pearson's) correlation coefficients. Curved lines without arrows represent non-causal covariation. Disturbance (error) terms ( $E_s$ ) are calculated for the lowest-order variable only. For simplification, nonsignificant and/or theoretically uninteresting relationships are not always represented.

<sup>a</sup>Originally .35

<sup>b</sup>Originally −.26

<sup>c</sup>Originally −.01\*

\*Not significant

N = 629

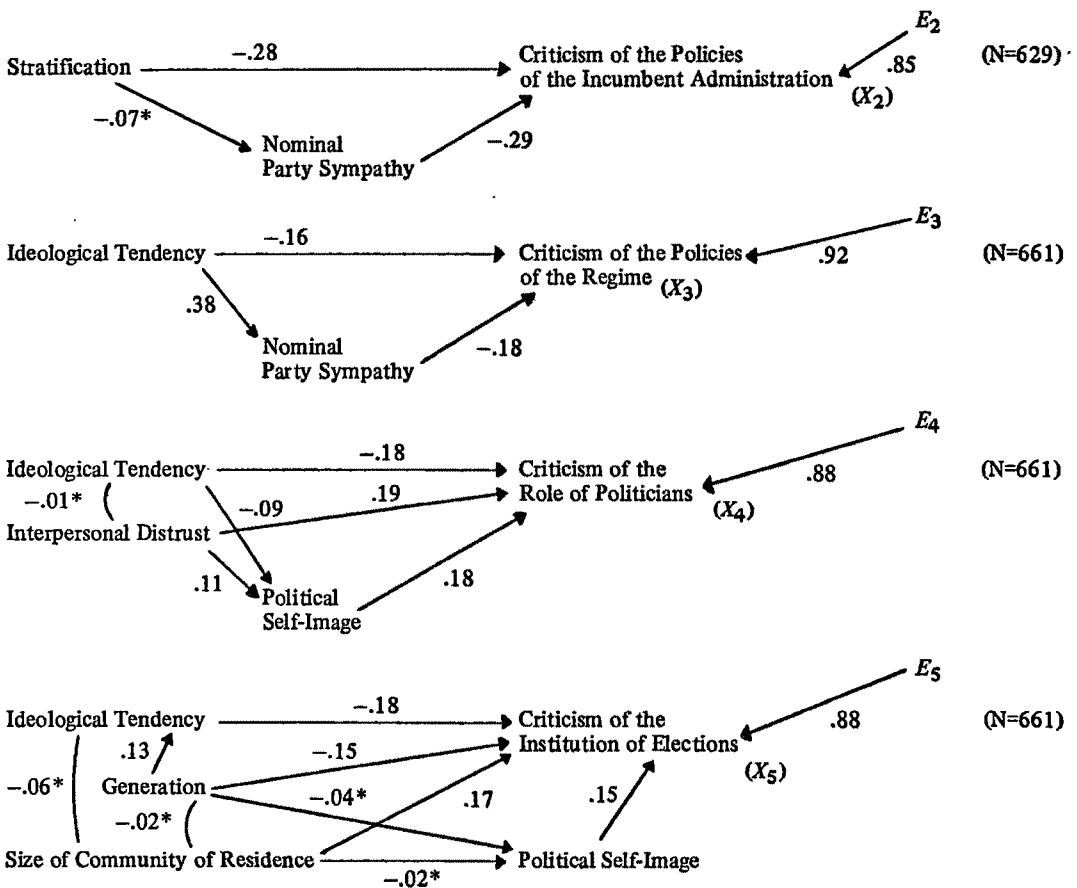
Figure 1. Sources of Sense of Political Efficacy in the Venezuelan Electorate

of political criticism. The first three models deal with the relationship between efficacy and evaluation of the performance of three different political actors: the incumbent administration, the regime, and the party politicians. The fourth model taps one aspect of system allegiance, measured by evaluation of the institution of elections.

The models do not completely rule out the possibility of a causal effect of the sense of efficacy on modes of political criticism. However, they suggest that sense of political efficacy is not a relevant causal agent of criticism. On the other hand, the impact of one or another mode of political criticism on the sense of efficacy should not be exaggerated: only one of the four aspects of criticism examined in Figure 2 could be included in the model presented in Figure 1. Once the impact of the three causal factors included therein is accounted for, other modes of political criticism are trivial or irrele-

vant.<sup>6</sup> One other related point should not be overlooked: given the difficulties encountered by those trying to measure the reality bases of

<sup>6</sup>The zero-order correlations between those modes of criticism and efficacy are not very robust. Expressed as Pearson's  $r$ s they are:  $-.07$  for regime criticism,  $-.01$  for criticism of elections, and  $-.14$  for criticism of politicians, respectively. I am aware of Paige's (1971) contention that traditional measures of efficacy are "contaminated" by the individual's perception of government performance. This is precisely the point; the relationship exists where it should and, apparently, nowhere else. In the Venezuelan case it is an entirely reasonable assumption for a member of an opposition party to make that it will not be very easy to influence government policy. Looking at the Mexican case, Coleman and Davis conclude that it is evaluation of regime performance, and not socioeconomic status, that determines one's sense of efficacy (1976, p. 203).



Source: Computed from data collected by Baloyra and Martz (1979, pp. 195-206).

Figure 2. Modes of Political Criticism in the Venezuelan Electorate

the sense of political efficacy (Weissberg, 1975), one could attempt to measure them indirectly, by looking at the separate causal effect of different modes of political criticism on an individual's sense of efficacy. Obviously, this would require an attempt to break down that *overall* sense of efficacy into its more specific components.<sup>7</sup> Then one could try to relate criticism and the sense of efficacy to the same kinds of political objects and, by implication, to the mode of participation which could serve as an empirical base for the individual's attempt to influence, and his/her subsequent evaluation of its success.

Further inspection of the path models presented in Figure 2 suggest that *political factors predominate* among the causal antecedents of the four modes of political criticism. Party preference, political self-image, and ideological tendency appear to be relatively constant influences. On the other hand, an individual's social circumstances do not appear to have a generalized impact since they are detectable in only one of the models, linking social stratification to criticism of the incumbent administration. The impact of this socioeconomic factor is relevant, but limited to one mode of criticism. Among the cultural factors which could predispose Venezuelans to display critical attitudes about politics and the political process, *interpersonal distrust* appears in only one of the models, while the impact of community of residence is restricted to criticism of the institu-

tion of elections. Finally, if one keeps an eye on the signs of the paths and the specific coding categories of the exogenous variables of the different models, one observes that the identity of the critics remains relatively constant across the different modes. Apolitical persons, independents, persons of leftist ideological tendencies, and sympathizers with leftist parties are more critical regardless of the object being evaluated.

Given the modal right-of-center orientation of the Venezuelan electorate (Baloyra and Martz, 1979, pp. 110–11) and the fact that Venezuelan politics can be described as a party system (Levine, 1973, p. 8), it is hard to imagine how the system could change in order to accommodate leftist and apolitical critics. On the other hand, they do not seem to be critical of the same kinds of objects since an individual's overall political orientation (political self-image) appears to be relevant in connection with politicians and elections, while the ideological motive extends to the outcomes of the regime as well. Somewhat more disturbing is that those who are more critical of the institution of elections include the young and the urbanites. One has to speak of erosion here since Venezuela is a predominantly young and urban country and becoming more so. Therefore, this may become a much more serious problem.

Even though we have emphasized disaggregating criticism into separate modes, it would be useful to repeat the analysis using a measure that will combine all the different modes. This would facilitate comparison with other findings reported in the literature, and would help validate and summarize the foregoing discussion. The model presented in Figure 3 uses a measure of criticism based on a linear combination of the standardized scores of the four modes of criticism discussed thus far. Inter-scale correlations are sufficiently low to pre-

<sup>7</sup>One notable innovation popularized by Verba and Nie (1972) is the idea of participatory modes. Since the literature on "civic attitudes" attempts to relate these to participation, it is surprising that not much has been done to determine the dimensionality, that is the *modes* of these civic attitudes. Noteworthy exceptions are Balch (1974), Converse (1972), Coleman and Davis (1976), Muller (1970), and Seligson (1978).

Table 3. Correlations among Measures of Criticism

	Scale ( $X_6$ )	Regime	Elections	Politicians
Administration	.58	.32	.14	.26
Politicians	.70	.35	.41	—
Elections	.65	.29	—	—
Regime	.68	—	—	—

Source: Computed from data collected by Baloyra and Martz (1979, pp. 195–206).

Note: All values are Pearson's correlation coefficients, significant at the .01 level. Sample size is 661 for all computations.

clude the assumption of cumulativeness among the four, as suggested by the data presented in Table 3.

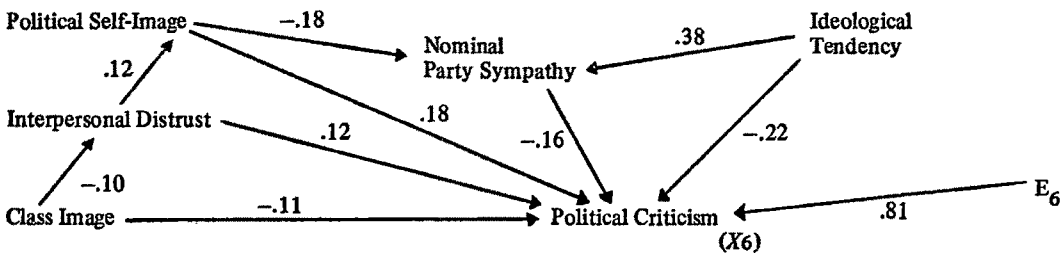
Ideological tendency appears to have the strongest impact on this aggregate measure of criticism ( $X_6$ ), followed by that of nominal party sympathy and of political self-image. When contrasted with the models presented before, this abbreviated model of political criticism affords no loss of information; once again we find voters of leftist ideological tendencies, sympathizers of leftist parties, and apolitical individuals to be more critical in an overall, aggregate sense. However, in spite of their limited impact which was confined to only one of the modes, both interpersonal distrust and social inequality are included in the model of Figure 3.

Two types of criticism appear implicit in the model. One is political and has little to do with social condition or interpersonal distrust. This is a type or mode of criticism following predominantly political considerations. This politicized criticism appears relatively independent of social or cultural influences and responds primarily to ideological and partisan

considerations. It contrasts with the other type or mode, which originates in levels of interpersonal distrust found in the society as well as in social inequalities. I am tempted to call it social protest but the label may be a bit premature. However, it does not appear to be an artifact of the style of analysis since it seems reasonable to assume that the less specific the object of criticism, the more salient factors like distrust and social condition are going to be. One practical implication of this finding is that measures of "diffuse support" for a regime may tend to exaggerate the importance of the social bases of political criticism.

### Political Cynicism

The point of departure for a contrast between political criticism and political cynicism ought to treat the former as a necessary but insufficient condition for the latter: all cynics are critical but not all critics are cynical. In addition, cynics, or better yet, level of cynicism among the electorate, are beyond the constraining influence of ideological or partisan considerations, as suggested by the model pre-



Source: Computed from data collected by Baloyra and Martz (1979, pp. 195-206).

Note: All paths leading to political criticism are fourth-order partial betas.

Magnitudes of the original zero-order (Pearson's) correlation coefficients between political criticism and:

- Class Image =  $-.12$
- Interpersonal Distrust =  $.17$
- Ideological Tendency =  $-.29$
- Political Self-Image =  $.25$
- Nominal Party Preference =  $-.29$

Not Represented:

- Distrust and Nominal Party Preference =  $-.09$
- Distrust and Ideological Tendency =  $-.01^*$
- Class Image and Party Preference =  $.00$
- Class Image and Political Self-Image =  $-.04^*$
- Class Image and Ideological Tendency =  $-.06^*$
- Ideological Tendency and Political Self-Image =  $-.09$

\*Not significant

N=666

Figure 3. Sources of Political Criticism in the Venezuelan Electorate

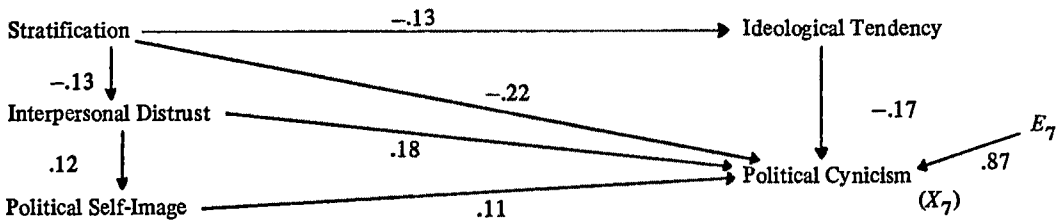
sented in Figure 4, which combines the standardized scores of the five measures of cynicism ( $X_7$ ) introduced earlier in the discussion. In this model, there is a bifurcation of causal paths somewhat similar to that discussed for criticism, but the partisan element is missing from the explanation. Stratification, interpersonal distrust, and ideological tendency appear more conspicuously with political self-image as the fourth exogenous variable.

This configuration identifies one basic source whose impact is independent of all others. Notice the lack of any difference in the magnitudes of the zero-order and partial betas between social stratification and cynicism. Obviously, the impact of stratification on cynicism is independent of any other source. A similar situation was observed for the relationship between class images and political criticism depicted in Figure 3. There the indirect effects of social inequality follow a more tortuous route toward criticism, although they are much less than the effects of stratification on cynicism. The contrast between the two models is instructive in that it gives some latitude to the assumption that criticism and cynicism do not result from lack of empathy, inadequate information, distrust, or pure misunderstanding. These kinds of influences are present and detectable but do not preempt the explanatory

contribution of other factors. For example, level of information correlates very weakly with cynicism (Pearson's  $r = .02$ ) and not at all with criticism ( $r = 0$ ).

Precisely what is the contribution of those other factors? The sign of the path from ideological tendency to cynicism suggests that the latter is more characteristic among the Left. Once the effects of stratification and interpersonal distrust are accounted for, the magnitude of the partial beta for ideological tendency goes up, declining slightly with the addition of political self-image to the model. Yet political self-image and stratification are not related (Pearson's  $r = -.04$ ), while the indirect path from the latter to cynicism changes sign. Apparently none of the strata is unique in its mixture of party sympathizers, independents and apolitical persons; the upper strata are less distrustful and less rightist, and also less cynical. But if neither ideological tendency nor level of interpersonal distrust can alter the magnitude of the direct path from stratification to cynicism, what is one to make of lower-class cynicism?

The answer seems to lie with the parties which may play for lower-class Venezuelans the same role and function that "civic" voluntarism plays for the middle class in the United States (Baloyra and Martz, 1979, pp. 64-74). It



Source: Computed from data collected by Baloyra and Martz (1979, pp. 195-206).

Note: All paths leading to political cynicism are third-order partial betas.

Magnitudes of the original zero-order (Pearson's) correlation coefficients between political cynicism and:

Stratification =  $-.22$   
 Interpersonal Distrust =  $.22$   
 Ideological Tendency =  $-.15$   
 Political Self-Image =  $.15$

Not Represented:

Stratification and Political Self-Image =  $-.04^*$   
 Ideological Tendency and Interpersonal Distrust =  $-.01^*$   
 Ideological Tendency and Political Self-Image =  $-.09$

\*Not significant

N = 661

Figure 4. Sources of Political Cynicism in the Venezuelan Electorate

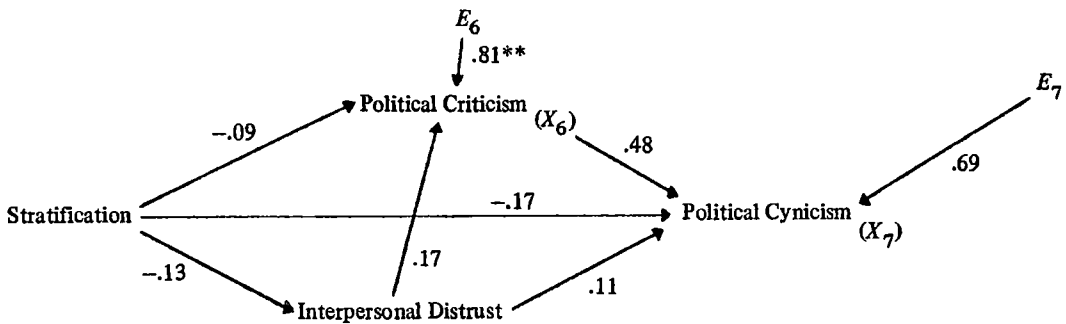
appears that participatory experiences through parties tend to deflate political criticism among lower-class Venezuelans. However, *lower-class cynicism seems beyond redemption by ideological or partisan manipulation in Venezuela.*

There is one additional element, missing in the model depicted in Figure 4, which should be made explicit in order to emphasize the connection between criticism and cynicism. Criticism, measured in aggregate terms, looms very large among the sources of political cynicism presented in the path model of Figure 5. Inclusion of criticism eliminates ideological tendency and political self-image from the explanation, while it cannot displace stratification and interpersonal distrust which carry over from the previous model. Indeed the impact of the former is reduced somewhat, while the impact of distrust is cut more drastically, but both factors must be included in this new version.

The social bases of political criticism and political cynicism may now be evaluated by

contrasting the models of Figures 3, 4, and 5. First of all, *the impact of social inequality on political criticism is relatively mild*, although independent of any other causal factor. However, the impact of social inequality on political cynicism is much more pronounced. In addition, the introduction of criticism into the explanation of cynicism, both being related very closely, fails to overshadow the impact of social inequality which must still be included in the model of Figure 5.

The implications of this seem relatively straightforward. Criticism is so generalized, so intertwined with party preference and ideological tendency, and even with the individual's degree of politicization (measured by self-image), that the relevance of social inequality is diminished. In other words, *Venezuelans need not be deprived to be critical but, regardless of their levels of criticism, chances are that they feel socially deprived if they are cynical.* The models presented in Figures 4 and 5 are conclusive in this regard. Whether considered in



Source: Computed from data collected by Baloyra and Martz (1979, pp. 195–206).

Note: All paths leading to political cynicism are second-order partial betas.

Magnitudes of the original zero-order (Pearson's) correlations between political cynicism and:

Stratification =  $-.23$   
 Interpersonal Distrust =  $.22$   
 Political Criticism =  $.51$

Magnitudes of third-order partial betas between political cynicism and other theoretically relevant variables:

Nominal Party Preference =  $-.02^*$   
 Political Self-Image =  $.01^*$   
 Ideological Tendency =  $-.04^*$

\*Not significant

\*\*From the solution presented in Figure 3

N = 666

Figure 5. Criticism, Distrust, and Cynicism

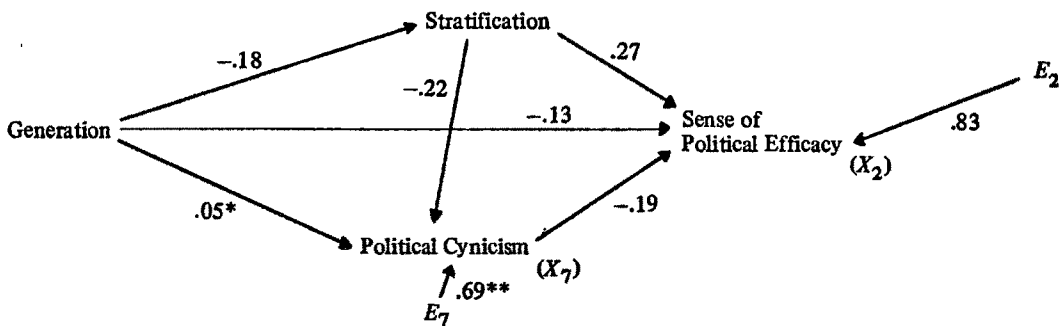
terms of criticism, or whether criticism is excluded from the explanation, the cynic is shown as a socially deprived person. Other factors drop in and out of the explanation, but the element of social inequality remains. In addition, even though practically everyone in Venezuela votes and even though the lower strata tend to be members of political parties more often than their upper-status counterparts, the upper strata feel more politically efficacious than the lower (Baloyra and Martz, 1979, pp. 66–68). These participatory activities may not bring much amelioration in a social sense, leading to more distrust, more apolitical self-images, and more cynicism. In either case, it is obvious that *political cynicism cannot be separated from low social condition in Venezuela.*

### Criticism, Cynicism, and Efficacy

It is necessary to emphasize that the distinction between criticism and cynicism has been

made using items purported to measure “trust in government.” This is important because even though our findings are in line with the conventional wisdom concerning the well-documented link between cynicism and low social condition, *cynicism has been measured as very extreme criticism* and not as the less corrosive forms identified as “cynicism” in the literature and treated as “criticism” in this analysis. This reminder intends to underline the fact that criticism appears to be a relatively “normal” part of Venezuelan politics, very much a result of party and ideological oppositions. In addition, there is reason to believe that criticism does not result in any serious decline in extant levels of the sense of political efficacy among Venezuelans.

The path diagram presented in Figure 6 shows that the sense of political efficacy can only be deflated by cynicism, not criticism, and this in conjunction with social condition and position in the life cycle. The model assumes that an individual’s sense of political efficacy is



Source: Computed from data collected by Baloyra and Martz (1979, pp. 195–206).

Note: All paths leading to sense of political efficacy are second-order partial betas.

Magnitudes of the original zero-order (Pearson's) correlation coefficients between sense of political efficacy and:

Stratification = .34  
 Political Cynicism = -.26  
 Generation = -.19

Magnitudes of third-order partial betas between sense of political efficacy and other theoretically relevant variables, with their original zero-order correlations in parentheses:

Interpersonal Distrust = -.04*	(-.11)
Ideological Tendency = -.03*	(-.05*)
Political Self-Image = -.05*	(-.09*)
Nominal Party Preference = -.02*	(-.03*)
Political Agility = -.02*	(.00)
Political Criticism = -.07	(-.17)

\*Not significant

\*\*From the solution presented in Figure 5

N = 666

Figure 6. Stratification, Political Cynicism, and Sense of Political Efficacy

determined by age, position in the social structure, and degree of cynicism. More specifically, the sense of efficacy is anchored on stratification or, better, on individual experiences related to social condition. Since aging brings a status deflation later in life, generation is shown to have direct and indirect impacts on the sense of efficacy, although there is no indirect impact through cynicism. Older Venezuelans feel less efficacious because of their generational experiences and declining social status, not because of their cynicism. Cynicism, on the other hand, appears to have more direct impact on the sense of efficacy. Finally, a higher social status reduces criticism while it is accompanied by a greater sense of efficacy.

It could be argued that Almond's and Verba's (1965, pp. 191–94) treatment of cynicism as dependent on a lack of sense of efficacy is equally plausible. However, criticism of administration policies was the only mode of political criticism related to efficacy in the first model (Figure 1), efficacy did not emerge as an important predictor of any of the modes of criticism (Figure 2), and the relationship between criticism and the sense of efficacy disappears when one controls for level of cynicism (Figure 6). Therefore, treatment of the sense of efficacy as a dependent variable seems justified by the findings,<sup>8</sup> and by their theoretical implications. Does this approach improve the theoretical usefulness of "sense of political efficacy?" Unfortunately, it does not.

In part, the problem with the concept of political efficacy is that we seem to know what it goes with—affluence, education, generation, civic norms and so forth (Balch, 1974, pp. 1–2)—but have not been able to solve the

trust-efficacy riddle satisfactorily. There is almost universal agreement that the sense of political efficacy ought to be high—like the Dow-Jones average or the parity of the dollar—to preserve democratic stability. In Venezuela, where levels of criticism are high, only one mode of criticism could be connected to fluctuations in levels of efficacy. That particular mode—criticism of the policies of the incumbent administration—has been measured in very specific terms. Paige was correct in warning that it may be very difficult to construct a measure of efficacy that is not contaminated by trust (1971, p. 814). However, this discussion has pointed out that what we need is to conceive of the sense of efficacy in its different modes. In addition, the sense of efficacy may not have as much impact on actual political behavior as it may appear.

### Cynicism, Efficacy, and Participation

Almond and Verba believed that the sense of political efficacy increases individual satisfaction with the role of participant (1965, p. 202). Balch (1974, p. 1, on Campbell et al., 1954) suggests that the concept of efficacy was first developed to help explain variations in electoral participation. Obviously, it would be impossible to test this hypothesis in Venezuela, given present levels of voting turnout. However, it has been shown that if a system of voluntary suffrage were introduced in Venezuela, those citizens who would be more likely to abstain from voting tend to match the profile of the cynic (Baloyra and Martz, 1976).

In trying to sort out the reasons for this type of non-participation in Venezuela one cannot come up with the sense of political efficacy among them, as suggested by the path model presented in Figure 7. Satisfaction with the role of participant in elections (voter)<sup>9</sup> is influenced by other factors. Notice that the data include second-order partial betas and the original zero-order correlations for the variables included in the model, as well as for those deemed relevant that did not turn out to be so. The model suggests that role dissatisfaction

<sup>8</sup>Zero-order correlations between efficacy ( $X_2$ ), criticism ( $X_6$ ), and cynicism ( $X_7$ ) are  $r_{26} = -.17$ ,  $r_{27} = -.26$ , and  $r_{67} = .51$ , respectively. From the model presented in Figure 6 and the discussion in the text it can be assumed that  $P_{76} = .48$  and since  $P_{26.7} = -.05$  the "weak" causal ordering appears to be

$$X_6 \xrightarrow{.48} X_7 \xleftarrow{-.18} X_2$$

although for the reasons advanced in the discussion it can be assumed

$$X_6 \xrightarrow{.48} X_7 \xleftarrow{-.18} X_2$$

Basically, the implication is that only criticism of government policies and cynicism can deflate the sense of political efficacy, while the context and issue- or actor-connected modes of efficacy that may reduce cynicism are yet to be specified in the literature.

<sup>9</sup>"Satisfaction with the role of participant in elections" was measured in the 1973 survey with the following item: Q. "Look and, if voting was not mandatory, would you vote or not?" Forty-eight percent of the respondents said "yes," they would continue to vote, while an identical percentage said "no," 3 percent answered "depends," and the rest (less than one percent) did not know or refused to answer the question.

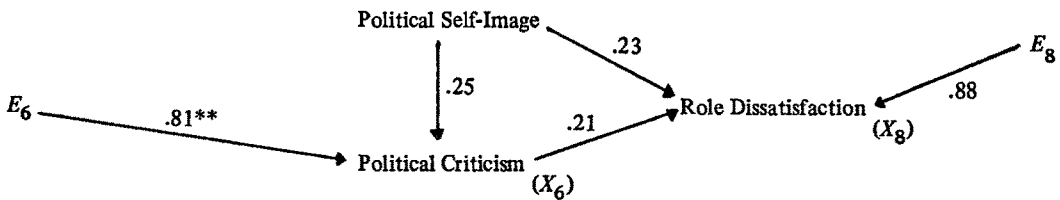


originates in criticism (measured in aggregate terms) and in the degree of politicization of the individual (measured in aggregate terms) and in the degree of politicization of the individual (measured by political self-image). Independents and apolitical persons, who also happen to be more critical, as well as critics in general, are the more likely candidates for withdrawal.

The model appears to have a number of important omissions. The more noticeable, as viewed from the perspective of the participation hypothesis and the literature associated with it, involves the absence of political efficacy from the explanation. In other words, people would no longer vote in Venezuela for reasons other than their declining or low sense of political efficacy. There is some association between the sense of efficacy and voting withdrawal (Pearson's correlation of  $-.12$  at bottom) but once self-image and criticism are accounted for, the impact of efficacy on role dissatisfaction is reduced to one half of its original magnitude. Likewise, individual social circumstances, measured by class images and

stratification, do not seem to have much relevance. It is not merely deprived people with a low sense of efficacy who would drop out of the system in this fashion, but *primarily people who do not want to play political roles in the first place.*

The fact that criticism and not cynicism is included in the model should not pass without comment. It has been shown that only one mode of criticism has some impact on sense of efficacy but, using the standardized aggregate index of political criticism, one finds this to have as much impact on role dissatisfaction as that of the individual's political self-image. Therefore, criticism has some corrosive effect after all since it contributes to the decrease in participation in elections. However, one should view this fact with some caution since it should be recalled (from Figure 2) that two of the scales of criticism included in the composite index allude to elections and politicians. Since the latter attract the highest levels of criticism found among the Venezuelan electorate, there may be much overlapping between criticism



Source: Computed from data collected by Baloyra and Martz (1979, pp. 195–206).

Note: All paths are first-order partial betas except between the exogenous variables which is a zero-order correlation.

Magnitudes of the original zero-order (Pearson's) correlations between role dissatisfaction and:

Political Self-Image = .28  
Political Criticism = .27

Magnitudes of second-order partial betas between role dissatisfaction and other theoretically relevant variables, with their original zero-order correlations in parentheses:

Interpersonal Distrust = $-.07$	(.13)
Ideological Tendency = $-.02^*$	( $-.06^*$ )
Nominal Party Preference = $-.05^*$	( $-.15$ )
Political Agility = $-.01^*$	(.00)
Political Cynicism = $.01^*$	(.22)
Sense of Political Efficacy = $-.06$	( $-.12$ )
Class Image = $-.01^*$	( $-.05^*$ )
Stratification = $-.03^*$	( $-.05^*$ )

\*Not significant

\*\*From the solution presented in Figure 3

N = 666

Figure 7. Sources of Dissatisfaction with the Role of Participant in Elections

and cynicism with respect to how politicians are evaluated. Consequently, even though 71.3 percent of the electorate are very supportive or highly supportive of elections, a number of them are among the 48 percent who would not vote if they did not have to, perhaps to punish the party politicians.

In summary, it is not who you are (nobody/somebody) nor how efficacious you feel (incapable/capable) but how politicized you are (apolitical/independent/partisan) and how critical you are that will determine whether you continue to vote or not. Obviously, what is required is not a great deal of cynicism, at least by Venezuelan standards, but intense levels of criticism in those modes having to do with parties and politicians. Thus, it is not the sense of efficacy itself but factors which contribute to fluctuations in the sense of efficacy which are more relevant determinants of the decision to participate. In my view, there is nothing "unconventional" about this.

### Recapitulation

Survey research has made important contributions to contemporary political science. Evidence accumulated during the last 35 years has allowed us to maintain a record of public reaction to politics in the United States and some of the other contemporary democratic regimes. This tradition has proved its usefulness for comparative politics as well, as evidenced by numerous studies dealing with similar topics in different cultural contexts. Ironically, attempts to interpret ongoing changes in some of the orientations among the publics of many democratic regimes have exposed some of the weaknesses of the assumptions and propositions which have lent theoretical coherence to the field. Nothing remotely resembling a "scientific revolution" is taking place, but the ongoing reevaluation offers an ideal opportunity for theoretical discussion and clarification.

A central concern of this analysis has been the use of standard measures of "civic attitudes" in making an analytical distinction between political criticism and political cynicism. The results of the analysis suggest that *political criticism is primarily a consequence of partisan and ideological oppositions*, although not beyond the influence of interpersonal distrust and social inequality. By contrast, *political cynicism appears as a form of criticism that cannot be dissociated from social condition*. The fact that this distinction can be made for Venezuela is especially significant, in light of the contextual and empirical difficulties discussed in this article. Consequently, I believe

that the distinction should be easier to make, using more robust measures in less critical contexts.

Some of the theoretical by-products of the analysis ought to lead to reflection about the role of criticism in contemporary democratic societies, as well as to some healthy skepticism about the theoretical relevance of the concept sense of political efficacy. Once it was established that it is more appropriate to treat the sense of political efficacy as dependent on criticism, rather than the other way around, not much relevance can be attached to the concept. Efficacy does not appear to have much impact on participation, while it is shown to fluctuate with individual evaluation of the performance of the government. However, in the United States, with a system of voluntary suffrage, this mechanism (increased criticism-reduced efficacy) may be at work in keeping people from participating in elections.

It goes without saying that all of the above relates to Venezuela and that its application to other cases may result in findings different from those discussed here. However, I would like to stress that the distinction between criticism and cynicism, which may or may not be useful, suggests that there may be a lot to gain from taking a closer look at some of the assumptions purporting to explain relationships which remained relatively invariant during much of the maturation period of the survey approach. It is symptomatic that many of the otherwise excellent attempts to deal with fluctuations in levels of trust and efficacy in the United States center on questions of validity and reliability, and not on the facts of change themselves. In other words, the measures need improvement but there is little question that civic attitudes *have* indeed changed during the last decade. I hope the ability to distinguish cynicism from criticism will prove itself helpful, but remain convinced that political cynicism may always be with us. As for political criticism, it is hard to conceive of a democratic society that would not have a place for it.

### Appendix.

#### Coding Categories of the Independent Variables Used in the Study\*

##### Interpersonal distrust

1. low (1.4)
2. moderate (5.2)
3. high (18.4)
4. very high (75.0)

## Nominal party sympathy

1. MEP, *Movimiento Electoral del Pueblo* (3.9)
2. MAS, *Movimiento al Socialismo* (6.4)
3. none (35.3)
4. AD, *Acción Democrática* (22.6)
5. COPEI, *Partido Social Cristiano* (27.2)
6. CCN, *Cruzada Cívica Nacionalista* (3.3)
7. other (1.4)

## Ideological tendency

1. Radical Left (15.4)
2. Left (7.5)
3. Center Left (13.0)
4. Center (10.8)
5. Center Right (11.1)
6. Right (42.2)

## Social stratification

1. agricultural poor (16.0)
2. manual poor (20.5)
3. manual middle class (24.8)
4. lower white collar (14.8)
5. upper white collar (9.4)
6. professional (14.5)

## Political self-image

1. militant, party sympathizer (48.7)
2. independent (19.2)
3. apolitical (32.1)

## Size of community of residence

1. rural, less than 5,000 inhabitants (24.5)
2. intermediate, between 5,000 and 24,999 (13.9)
3. urban, between 25,000 and 99,999 (15.6)
4. metropolitan, larger than 100,000 (46.0)

## Region of residence

1. Plains (7.4)
2. Andes (11.6)
3. West (14.3)
4. East (16.9)
5. Zulia (11.4)
6. Center (38.4)

## Political agility or level of political information

1. attuned (67.0)
2. disoriented (33.0)

## Gender role

1. male (49.7)
2. female (50.3)

## Religiosity

1. none, agnostic, atheist (3.9)
2. Protestant (2.0)
3. nominal Catholic (58.4)
4. practicing Catholic (35.7)

## Generation

1. 18 to 24 (27.5)
2. 25 to 29 (14.6)
3. 30 to 34 (11.1)
4. 35 to 39 (10.3)
5. 40 to 44 (9.0)
6. 45 to 49 (8.0)
7. 50 to 54 (5.2)
8. 55 to 59 (4.9)
9. 60 and over (9.5)

## Class images

1. poor (35.0)
2. middle class (65.0)

\*Adjusted relative frequencies in parentheses.

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# Social Amelioration through Mass Insurgency? A Reexamination of the Piven and Cloward Thesis

ROBERT B. ALBRITTON

*Virginia Polytechnic Institute and State University*

*Frances Fox Piven and Richard A. Cloward (1971, 1977) have argued that mass insurgency in the United States, occurring especially between 1964 and 1969, produced a series of responses by government, one of the most significant being massive expansion of welfare rolls. Using data on which they base their claim, this study examines the hypothesis that there is a positive association between social disorders and welfare caseload increases. The conclusion is that associations specified by Piven and Cloward are not supported by the data and a plausible rival hypothesis is offered to explain the massive increases in welfare caseloads.*

Social science research that focuses on explanation seldom bridges the gap between this objective and developing strategies for achieving group or social ends. The collaborative work of Frances Fox Piven and Richard A. Cloward is one of the rare examples of research that combines both social theory and social praxis. By analyzing historic movements of social protest, they suggest not only causal explanations of social policy shifts, but also implications for strategies by which the poor can achieve economic and political gains from governments and members of the elite ordinarily inured to their demands. As a significant contribution to the literature on the politics of mass society, their argument deserves more serious attention and more detailed analysis.

Piven and Cloward (1971, 1977) argue that mass insurgency is virtually the only strategy available to the poor for compelling social and economic progress consonant with their interests. Of all the influences that the poor have exerted upon the political structure to improve conditions of the lower classes, only mass disruption and defiance have achieved notable gains. Mob looting, rent riots, and relief insurgency brought expanded relief support and recognition of the cause of unemployed workers in the 1930s. Widespread labor unrest, including strikes, beyond the control of union officials, won concessions to labor power. Mass rioting in the United States between 1964 and 1969 produced a series of responses by government, one of the most significant being massive expansion of the welfare rolls. Piven and Cloward concede that there are inherent risks in pursuing strategies of mass defiance and that historical conditions may not be propitious for their success, but such caveats are diminished

by the fact that the limited political influence of the poor depends upon mass protest and disruption of the social and political order. Despite its limitations, enlargement of lower-class political influence is linked to a strategy of escalating "the momentum and impact of disruptive protest at each stage in its emergence and evolution" (1977, p. 37).

This is an innovative thesis in the literature about the politics of mass society. Piven's and Cloward's special contribution is to focus attention less on the causes of mass insurgency than on the potential for political influence by the poor in periods of widespread social and institutional dislocation; less on the causes of disruptions and rebellions than on their outcomes; less on explanations of why men and women rebel than on what happens when they do. Only a passion for organizational and electoral politics, they contend, limits the prospects offered by spontaneous disruption or rebellion. But sometimes the threat of social and political disorders enables the lower classes to wrest economic and political gains from governments and elites. The implications for praxis are clear: "A placid poor get nothing, but a turbulent poor sometimes get something" (1971, p. 338).

In a highly original treatment of the welfare system, *Regulating the Poor: The Functions of Public Welfare*, Piven and Cloward claim empirical support for their thesis. Echoing perceptions of many thoughtful Americans in characterizing the late 1960s as a period of "welfare explosion," they argue that an exponential increase in welfare spending and caseloads (particularly in the Aid to Families with Dependent Children program) was an outcome of historic causal processes, the most proximate and significant being the mass insurgency that

characterized the 1960s. According to their analysis, increases in AFDC represented a dramatic shift in structures of welfare institutions that had remained relatively stable for 25 years. Some impetus for this change was provided by community-organizing efforts in major American cities, instigated and largely funded by the Office of Economic Opportunity. However, the *effectiveness* of these efforts was a product of growing social and political disorder that caused governments at federal, state, and local levels to respond with increased welfare caseloads and benefits. "Relief-giving does not increase simply because economic deprivation spreads," they contend, "nor did it increase for this reason in the 1960s" (1971, p. 190). The primary cause was a mobilization of the poor to supplant traditional politics with threats of mass social and political disruptions, to which government responded with more welfare.

Piven and Cloward indicate the appropriate course for testing this "mass insurgency" thesis by linking specific welfare policy changes to the disorders of the 1960s. In accordance with their thesis, the rise in welfare activity should coincide with protests, demonstrations, riots, and other evidence of the breakdown of social legitimacy. Their claim is that mass disturbances and welfare increases can be linked empirically so as to confirm their analysis. This research is an effort to clarify the causal relationships asserted in *Regulating the Poor* and to provide empirical tests of the Piven and Cloward thesis—that during the 1960s mass turbulence led to liberalized social welfare policies as a political response to social and political disorder.

### Specification of the Model

Piven's and Cloward's work provides no explicit model clearly linking mass insurgency to social amelioration and change. Consequently, efforts to test their major propositions bear the burden of constructing the chain of causal processes in a manner consistent with their thesis. In their most unambiguous formulation of a causal model, Piven and Cloward state (1971, p. 338): "The terms in which this crisis [the relief explosion of the 1960s] must be explained are economic disruption, large-scale migration, mass volatility, and electoral responses—a sequence of disturbances leading to a precipitous expansion of the welfare rolls." Although the explicitness of this statement is qualified by a lengthy discussion of federal anti-poverty activities as the catalyst for mass

volatility and electoral response, it specifies the components of a developmental model in which a clearly defined causal sequence results in specified outcomes—massive increases in AFDC caseloads. With the qualification just noted, this statement appears to provide a basis for operationalizing and testing linkages representing their "mass insurgency" thesis and, thus, an implicit test of their thesis as a whole.

Economic disruptions specified by Piven and Cloward are those occurring during the post-World War II period when a combination of returning veterans, agricultural mechanization, and economic recession displaced thousands of unskilled workers, largely southern and black, into the ranks of the unemployed. The migration of southern blacks into northern industrial cities where they became a new "underclass" was accompanied by a corresponding social dislocation that undermined legitimacy structures and paved the way for spontaneous rebellions that followed (Smelser, 1963; Kornhauser, 1959; Skolnick, 1969). Thus, *effects* of "economic disruption" are expressed as mass migration and the ensuing social and political unrest.

The role of "electoral responses" in the model is more complex. Piven and Cloward argue that a strategy of mass insurgency was successful only because black voters possessed a strategic potential to disrupt the traditional Democratic party coalition of presidential politics. The potential for blacks in northern cities to determine the outcome of presidential elections, either by defections to the Republican party or by withholding their votes from Democratic candidates, destabilized electoral conditions and forced Democratic political leadership of both Congress and the presidency to resolve conflicting interests of northern black and southern white constituencies. The issue was resolved when members of the Democratic party elite chose to respond to the political threat of mass insurgency by forsaking their southern, white interests in favor of solidifying electoral support among black Americans (1977, p. 15ff.).

Specified in this way, "electoral responses" is not a variable, but a constant. The argument that mass insurgency of the 1960s produced gains for the lower classes in the form of expanded welfare coverage implies the tautological conclusion that conditions of the electoral system were such as to insure success of protest strategies. If electoral conditions had not been favorable, the protests would have failed to produce results. Thus, electoral environment constitutes a catalyst specified as constant in its effect throughout the 1960s,

which, at least for this period, is not a variable that requires inclusion in the model.

The crucial linkages of the model may be summarized as follows. Social dislocations represented by mass migrations of blacks to northern cities weakened structures that motivated these migrants and their progeny to express their grievances through legitimate social and political institutions. In the 1960s, failures of these institutions to meet the expectations of the poor resulted in mass insurgency that was concentrated primarily in northern urban areas. Confronted with the breakdown of social and political order, governments at all levels—federal, state, and local—attempted to placate, rather than suppress the growing turbulence of the cities. The result was an observed “explosion” of the welfare system in the form of caseloads and benefits.

### Effects of Mass Insurgency

In support of their argument, Piven and Cloward present data on changes in welfare (AFDC) caseloads for regions, states, and the 121 major urban counties for the periods 1950–60, 1960–69, and 1964–69, which ostensibly confirm the coincidence of the welfare explosion with major urban disorders. With this data, they demonstrate that while the largest urban counties accounted for less than one-quarter of the national increase in AFDC during the 1950s, these same counties, marked by urban violence, contributed approximately 70 percent of the increase during the 1960s. Most crucial to their thesis is the precipitous increase that occurred after 1964—58 percent

compared to 31 percent during the first four years of the decade. “Where” and “when” the largest and most rapid rates of welfare increases occurred constitutes the major supporting evidence that mass insurgency produced political responses leading to increases of the welfare rolls (1971, p. 187).

However, the data cited by Piven and Cloward do not provide strong support for the model. Their contention that the steepest increases in welfare caseloads (217 percent) occurred in the five most populous counties—New York, Philadelphia, Cook County (Chicago), Wayne County (Detroit), and Los Angeles—is based upon a misleading aggregation of the data. While New York and Los Angeles show higher rates of increase than the 116 remaining urban counties, the other three—Philadelphia (123 percent), Cook County (109 percent), and Wayne County (77 percent)—are all substantially below the 116-county average (135 percent). (See Piven and Cloward, 1971, Source Table 2.) In fact, only New York and Los Angeles appear among the top *ten* counties in proportion of increase in AFDC caseloads during the 1960–69 period (see Table 1).

The timing of the largest proportionate increases in welfare (AFDC) programs also leads to somewhat mixed conclusions. Nearly one-third of the urban counties (38) show greater increases during the 1960–64 period than during the period of mass insurgency, 1964–69.<sup>1</sup> Over one-fifth of these same coun-

<sup>1</sup>Feagin and Hahn (1973) note that the focus on riots occurring 1964–69 is largely a function of mass

Table 1. Top Ten Urban Areas in Proportionate Increases of AFDC Caseloads and Rank Order According to Increased Incidence of Robbery, 1964–69

Urban Areas Ranked by Proportionate Increase in AFDC Caseload	Percent AFDC Caseload Change	Rank of Increase in Robbery
1. Lubbock, Texas	300	117
2. Columbus, Ohio	225	70
3. Anaheim, California	195	67
4. Dallas, Texas	194	16
5. Atlanta, Georgia	184	65
6. Austin, Texas	180	60
7. Los Angeles, California	145	22
8.5. Savannah, Georgia	144	38
8.5. Yonkers, New York (Westchester County)	144	1.5*
10. New York City	137	1.5*

Source: Frances Fox Piven and Richard A. Cloward (1971), *Regulating the Poor*, New York: Random House, Source Table 2.

\*The Uniform Crime Reports do not distinguish between New York City and Westchester County in reporting crimes by urban areas.

ties (25) experienced higher growth rates of AFDC caseloads between 1950 and 1960 than during the 1960s (see Piven and Cloward, 1971, Source Table 2). In addition, Piven's and Cloward's attempt to link unemployment to the mass insurgency-welfare explosion process is undermined by data showing that the unemployed parent component of AFDC, which specifically provides relief to the unemployed, increased by substantially *larger* amounts (and rates) from 1960 to 1964, than during the 1964-69 period (see Piven and Cloward, 1971, Source Table 3).

These anomalies in Piven's and Cloward's data challenge their thesis that "the contemporary relief explosion was a response to the civil disorder caused by rapid economic change" (1971, p. 196). The data do not unequivocally confirm that the impetus toward liberalization of welfare was concentrated in the largest urban areas, nor that it had its beginning in the onset of mass disorders after 1964. There is clearly a need for further analysis and testing of these basic claims.

At least two of the claims advanced by Piven and Cloward seem amenable to more formal empirical tests. The first is that the areas of greatest social and political disorders were those that experienced the largest amounts of black immigration as a result of the post-World War II economic dislocations; second, that the most rapid rises in welfare (AFDC) caseloads coincided with those areas marked by civil disorders. These propositions may be tested using the 121 urban counties for which Piven and Cloward provide data. The measure of the dependent variable is the proportionate increase in AFDC caseloads during the period in which mass disorders occurred in urban areas of the United States (1964-69). Percentage changes in AFDC caseloads during this period are consistent with an emphasis on *dynamics* of welfare increases since the absolute magnitude of increases is generally a function of population size.

The intervening variable, *mass insurgency*, is operationalized as incidents of urban violence. Piven and Cloward suggest that "urban violence" refers not solely to mass urban disorders, but also to crimes of violence occurring especially, but not exclusively, in large urban areas.

Frequent invocations of "crime in the streets" to provoke political responses, whether of a repressive or ameliorative type, probably blurred distinctions between mass disorders and individual crimes of violence. Thus, an indicator of "urban violence," as Piven and Cloward suggest (1971, p. 226), is a rise in serious crime, especially robbery and burglary. A positive relationship between these crimes and increases in welfare caseloads in urban areas provides support for the Piven and Cloward thesis.

The tests that follow link changes in the incidence of robbery and burglary in urban areas to the percent change in welfare caseloads during the period 1964-69.<sup>2</sup> Table 2 indicates the strength of the relationship between increase in incidence of robbery and burglary and proportion of increase in AFDC caseloads over the 1964-69 period. Neither changes in the absolute incidence of these crimes nor changes in their rate of increase show any relation to welfare caseloads. In every case, the proportion of variance explained is virtually zero. (Although not shown in Table 2, coefficients of regression are not significantly different from zero.)

Anticipating that inclusion of southern urban areas might distort the relationship, I performed the same analysis for the 70 non-southern counties included in the sample. One observes only minor differences in the outcome (Table 2). Clearly, no significant relationship exists between increases in crime rates, e.g., robbery and burglary, and AFDC caseload increases during the period in question. In this respect, the null hypothesis cannot be rejected and the data do not lend support to the Piven and Cloward argument.

<sup>2</sup>Data on incidence of robbery and burglary are taken from the *Uniform Crime Reports* (1964, 1969). Data on these indicators are available for all but one (Montgomery, Alabama) of the 121 urban areas examined by Piven and Cloward. In the *UCR*, several of the urban counties are aggregated with adjoining large cities. Since these data would be identical for incidence of crime (e.g., New York City and Westchester County, or San Francisco and Oakland) but different for welfare caseload increases, inclusion of *both* areas would bias the analysis in favor of the null hypothesis. In these cases, the smaller areas have been dropped from the analysis, a procedure that reduces the sample size by an additional 12 units leaving 108 urban areas for analysis. The *UCR* do not distinguish between "robbery" and "armed robbery." The definition of the former includes "armed robbery where a weapon is used and strong-arm robbery . . . such as mugging, yoking, etc." In every case, it is considered a violent crime.

media attention. Most of the major riots occurred during this period, however, culminating in the riots of 1968. In any case, it is a useful period for examining the Piven-Cloward thesis and is consistent with their interpretation of events.



An examination of the relationship of increases in crime to welfare caseloads is hardly a definitive test of the Piven and Cloward thesis. A more convincing test of the relationship between mass disorders and welfare increases requires examination of the link between urban riots occurring in the 1960s and corresponding increases in AFDC caseloads. One way of testing the thesis that mass insurgencies produced social amelioration in the form of liberalized welfare policies is by attempting to answer the question: were the urban areas marked by violence the same as those characterized by the largest increases in welfare caseloads? The question can be plausibly answered by comparing proportionate increases in welfare among urban areas in which serious disorders occurred to urban areas in which they

did not. Fortunately, Piven and Cloward provide a list of urban areas in which "serious disorders" occurred, i.e., those marked by violence lasting more than one day, with at least some fires and rioting, and the use of state police to quell the disturbance (1971, pp. 239-40). In addition, they provide a list of comparable urban areas in which little or no rioting took place.

A comparison of means of the rate of welfare increases between 1964 and 1969 for the two types of areas is presented in Table 3. While urban areas in which riots occurred increased welfare caseloads at a larger rate than those not experiencing riots (78 to 67 percent), the difference is not significant. Again, the null hypothesis cannot be rejected and the evidence fails to support the hypothesis that mass

**Table 2. Proportion of Variance in AFDC Caseload Increase Rates Explained by Increases in Incidence of Robbery and Burglary in 108 Urban Counties and 70 Non-Southern Counties, 1964-69\***

	(r <sup>2</sup> )
<b>108 Urban Counties</b>	
Change in incidence of robbery with percent increase in AFDC caseloads	.0103
Change in incidence of burglary with percent increase in AFDC caseloads	.0159
Rate of increase in robberies with percent increase in AFDC caseloads	.0001
Rate of increase in burglaries with percent increase in AFDC caseloads	.0004
<b>70 Non-Southern Urban Counties</b>	
Change in incidence of robbery with percent increase in AFDC caseloads	.0188
Change in incidence of burglary with percent increase in AFDC caseloads	.0124
Rate of increase in robberies with percent increase in AFDC caseloads	.0045
Rate of increase in burglaries with percent increase in AFDC caseloads	.0062

Source: Frances Fox Piven and Richard A. Cloward (1971), *Regulating the Poor*, New York: Random House, Source Table 2; Federal Bureau of Investigation, *Uniform Crime Reports*, 1964 and 1969.

\*No regression coefficients are significantly different from zero at the .05 level. Incidence refers to the number of crimes per 100,000 population. Rate of increase is the proportionate change in AFDC caseloads or in incidence of crime, respectively.

**Table 3. Test for Differences of Mean Percent Increases in AFDC Caseloads between Urban Areas in Which Riots Occurred and Those Where Riots Did Not Occur between 1964-69**

	Number of Cases	Mean Increase	Standard* Error
Urban areas in which riots occurred	23	78.30%	9.379
Urban areas in which riots did not occur	26	67.38%	9.990

t-value = 0.80

df = 47

One-tail Probability = .215

Source: Frances Fox Piven and Richard A. Cloward (1971), *Regulating the Poor*, New York: Random House, Source Table 2 and pp. 239-40.

\*F-value for difference of variance,  $p > .50$ .

insurgency in the form of urban riots produced ameliorative political responses in the form of increased welfare caseloads.

### Black Fomentation of Mass Insurgency

The specified model suggested by Piven's and Cloward's analysis implies that the pattern of increases in black population as a result of mass migrations is associated with rises in all forms of social disorders, including rises in crime and mass violence. It is difficult to obtain data on migration to specific urban areas for the 1950–60 period and Piven and Cloward do not provide data necessary for testing the relationship between black migration and urban disorders. If one is willing to assume that

changes in nonwhite population are representative of migration patterns, even though some of this increase is due to births, the relationship can be tested with readily available census data. The relationship between increases in nonwhite population and social disorders represents a linkage inferred by Piven and Cloward—that blacks were responsible for fomenting specific disorders that led to increases in welfare (1971, pp. 222, 226).

A test of the hypothesis that changes in nonwhite population between 1950 and 1960 contributed to changes in incidence of crime, i.e., robbery and burglary, is presented in Table 4. Based upon the same urban areas used for Table 2, coefficients of determination ( $r^2$ ) indicate that there is virtually no association of

**Table 4. Proportion of Variance in Incidences of Robbery and Burglary Explained by Increases in Percent Nonwhite Population for 108 Urban Counties and 70 Non-Southern Counties, 1964–69\***

	( $r^2$ )
<b>108 Urban Counties</b>	
Percent increase in nonwhite population with change in incidence of robbery	.0249
Percent increase in nonwhite population with change in incidence of burglary	.0161
Percent increase in nonwhite population with rate of increase in robberies	.0144
Percent increase in nonwhite population with rate of increase in burglaries	.0110
<b>70 Non-Southern Urban Counties</b>	
Percent increase in nonwhite population with change in incidence of robbery	.0344
Percent increase in nonwhite population with change in incidence of burglary	.0083
Percent increase in nonwhite population with rate of increase in robberies	.0258
Percent increase in nonwhite population with rate of increase in burglaries	.0102

Source: U.S. Bureau of the Census, *U.S. Census of the Population*, 1950, 1960; Federal Bureau of Investigation, *Uniform Crime Reports*, 1964, 1969.

\*No regression coefficients are significantly different from zero at the .05 level. All correlation coefficients are negative. Incidence refers to the number of crimes per 100,000 population. Rate of increase is the proportionate change in incidence of crime.

**Table 5. Test for Differences of Mean Percent Increases in Nonwhite Population, 1950–60, between Urban Areas in Which Riots Occurred and Those Where Riots Did Not Occur between 1964–69**

	Number of Cases	Mean Increase	Standard <sup>1</sup> Error
Urban areas in which riots occurred	23	83.1344%	13.092
Urban areas in which riots did not occur	26	65.6982%	7.083

$t$ -value<sup>2</sup> = 1.21

$df$  = 47

One-tail Probability = .117

Source: Frances Fox Piven and Richard A. Cloward (1971), *Regulating the Poor*, New York: Random House, pp. 239–40; U.S. Bureau of the Census, *U.S. Census of Population*: 1950, 1960.

<sup>1</sup>F-value for difference of variances,  $p < .01$ .

<sup>2</sup>T-value based on pooled variance estimate.

nonwhite population increases with rises in crime during the 1964–69 era. Again, removing from the sample southern urban areas in which nonwhite populations were relatively stable or declining does not markedly change the result.

When one examines the differences between urban areas marked by riots or other mass disorders and those experiencing little or no violence, the results still offer little support for the Piven-Cloward thesis (Table 5). Nonwhite populations in urban areas experiencing riots during the 1964–69 period grew at a faster rate (83 percent) than in those experiencing few or no disorders (66 percent). Again, however, an analysis of the difference of means indicates that this difference is well within chance probabilities. The standard error of the two groups this time indicates that they may be drawn from different populations. Substantively, this means that another variable may be a factor, but neither the analysis nor the data permit more than speculation as to the cause of this difference. The null hypothesis that there is no relationship between changes in nonwhite populations of urban areas and urban disorders of the 1960s cannot be rejected.<sup>3</sup> In summary, neither of the relationships inferred by Piven and Cloward—that increases in nonwhite populations in northern cities led to urban disorders or that the rise of urban disorders led to increases in welfare caseloads—is supported by the analysis.

#### Plausible Alternative Explanations

Piven and Cloward do not argue that mass insurgency in the 1960s produced substantially higher benefits levels for persons already receiving welfare. They argue the opposite—that a declining rate of benefits increases in the 1960s was accompanied by an exponential increase in the welfare rolls. While average welfare payments during the 1950s increased by half, caseloads increased by only 17 percent, but in the 1960s a one-third increase in benefits was associated with more than a doubling of the welfare rolls (Piven and Cloward, 1971, p. 191).

<sup>3</sup>This finding agrees with results of a study by Seymour Spilerman (1970) in which the zero-order correlation of percent change in nonwhite population with number of disorders was  $r = .04$ , for a much larger sample that included even minor racial disorders. Spilerman found that absolute number of blacks was the only significant predictor of race riots—certainly the necessary, if not the sufficient, condition for racial disturbances. The data are, thus, more consistent with alternatives to the migration-social breakdown thesis. See Feagin and Hahn (1973).

In fact, the position of the poor may have been much worse than Piven and Cloward suggest. The total and average income gap of poor persons, even among those receiving government assistance, widened steadily between 1965 and 1972, relative to the rest of American society (Plotnick and Skidmore, 1975, p. 86). When increases in welfare grants, 1960–70, are adjusted for inflation, 22 states show declines in average monthly grants to AFDC recipients during this period. When “equity” considerations are taken into account, i.e., the proportion of average per capita income provided by AFDC grants, 44 states show declines over this period (Albritton, 1976, p. 374).

Finding no relationship between patterns of social disorder and welfare increases, can one propose rival hypotheses consistent with the dramatic rise in welfare caseloads and a corresponding decline in real and relative welfare benefits that occurred during the 1960s? One such hypothesis is that the rise in caseloads (AFDC) was a response to innovations and adjustments in federal welfare assistance structures by amendments to the Social Security Act adopted in 1965. These amendments (unnoted by Piven and Cloward) not only established a program of comprehensive medical coverage for the poor (Medicaid), but they also provided the first increase in the federal matching share of AFDC grants since 1958. One major provision made indigent patients in mental hospitals and tuberculosis institutions eligible for welfare assistance. Thus, it became in the interest of states to ensure that these persons were added to the rolls to pay for their maintenance and care. Effective July 1, 1965, states were permitted to provide *retroactive* assistance to AFDC families with children over 18 years of age who were still attending school or with minor children earning income. Another major incentive to welfare eligibility at the time was a provision that, for medical coverage (Medicaid), the federal government's reimbursement to states should be not less than 105 percent of the matching federal share of medical assistance at the end of 1965. Under these incentives, states had an interest in getting as many people onto welfare rolls as possible to maximize federal support of state programs in future years.

The increase in federal share of the reimbursement to states is, ironically, not an incentive to increasing welfare grants, but to expanding further the welfare rolls. There are very real fiscal incentives attached to increasing caseloads rather than grants levels and most states show that they understand this arrangement. The reimbursement formula provides a relatively

high rate of federal matching funds for welfare grants (as high as 5/6 of the cost) up to a maximum. For states with low levels of welfare grants, the federal government may be considered a fiscal resource by which a state can return five dollars to the state for every one invested in the welfare system. As grants levels exceed the maximum, this incentive declines sharply, but most states, even those with higher benefits, seem to understand this intriguing structure of the welfare system and allocate available funds for increasing caseloads rather than grants.

The pattern of increases in the welfare rolls during the later 1960s is consistent with a model that emphasizes effects of this legislative initiative at the federal level. It is neither coincidental nor inconsequential for the model that this legislation was a product of the 89th Congress. The Johnson landslide of 1964 produced a Congress determined to advance the interests of the poor. Amendments to the Social Security Act of 1965, signed into law days before the Watts riots, provided important changes in benefits, a mammoth rise in welfare spending, and a larger aggregate of resources to meet the needs of the poor. The result was a rapid expansion of the scope of welfare assistance in the form of increases in AFDC rolls, while cash welfare benefits declined.

Some tentative empirical evidence supports this interpretation of the data. When general growth trends are controlled, abrupt increases in AFDC spending by state and local governments coincide with implementation of the 1965 legislation, after which the course of spending for this category of welfare assistance recedes (Albritton, 1976, pp. 384–85). Similarly, when population size and federal welfare spending are controlled in equations estimating AFDC caseloads, 1960–70, using states as the unit of analysis, one observes a discontinuity in parameters as an abrupt increase that coincides with implementation of these amendments to the Social Security Act. Thus, the pattern of welfare activity during this period shows increases consistent with a trend established before 1965, interrupted by a change in level coinciding with the federal legislation (Albritton, 1975, pp. 61–64).

This perspective on the data, at variance with the Piven and Cloward thesis, suggests an explanation of why no significant relationships appear between AFDC caseload increases and the mass insurgency of the 1960s. Caseloads were largely a function of changes in the structure of welfare policy relationships between the federal government and the states, while the riots and other social disorders had

different origins. Absence of relationships between urban violence and the “welfare explosion” suggests independent causation of these two effects.<sup>4</sup> In any case, the riots of the 1960s, whether or not they represented calculated political strategies, appear to have had few, if any, direct consequences for expansion of the welfare system.

### Conclusions

The foregoing analysis represents an effort to examine the Piven and Cloward thesis that mass insurgency in the 1960s produced gains for the poor in the form of large-scale additions to AFDC caseloads. Analysis of the data which they claim provides empirical support for this thesis indicates no significant relationships between social disorders (such as riots or crimes) and increases in welfare caseloads, or between increases in nonwhite populations and social disorders in urban areas. In both cases, no association was found that could not be explained by chance. The conclusion is that the relationships specified by Piven and Cloward are not supported by the data in this form of analysis.

A failure to falsify the null hypothesis in this analysis, of course, does not prove that such relationships do not exist. Nevertheless, to the degree that the model as operationalized here accurately represents linkages argued by Piven and Cloward, results of the analysis cast considerable doubt on their conclusions. At the very least, they require a fundamental reconsideration of the way in which protest strategies can be said to accomplish social amelioration and change.

Events of the 1960s and the data advanced by Piven and Cloward are at least equally consistent with an alternative thesis—that the causes of the “welfare explosion” lie in welfare policy innovation and change preceding the period of pronounced mass insurgency that characterized the later decade. Amendments to the Social Security Act extended welfare benefits to millions of new recipients *and* offered incentives to states to admit them to the rolls. The data clearly are more consonant with an explanation holding that the riots and the

<sup>4</sup>The reader should consult Miller, Bolce, and Halligan (1977) for a sophisticated version of the “relative deprivation” thesis with findings that would lend credence to this speculation. This and other explanations are cogently examined in Feagin and Hahn (1973). The general conclusions of the latter are similar to those of this study—that riots produced little, if any, social amelioration.

"welfare explosion" are independent of one another and independently caused.

Did mass disruption and protest succeed as a political strategy in winning concessions for the poor? Perhaps the spectre of riots in some cities caused state and local officials to make some reforms before riots occurred in their areas. Very likely the protest movements of the 1960s, of all types, gave considerable impetus to efforts at ending racial discrimination against blacks, particularly the black middle class. But for the poor? No. The specific structures that contributed to the "welfare explosion" probably antedated the most significant period of urban disorders. For the poor, as well as for those on welfare, their "prize" was a gradual worsening of position relative to the rest of American society as they lag even further behind nonpoor Americans in a generally expanding economy.

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# Electoral Instability, Civil Disorder, and Relief Rises: A Reply to Albritton

FRANCES FOX PIVEN  
*Boston University*

RICHARD A. CLOWARD  
*Columbia University*

There are two serious flaws in Robert Albritton's challenge to the argument of our *Regulating the Poor* (1971). He tested the wrong theoretical relationships. Then, for the relationships he did test, he used the wrong data.

## Testing the Wrong Relationships

In *Regulating the Poor*, we argued that the welfare explosion of the 1960s had its roots in the problems created for the national political system, particularly for the national Democratic party, by economic and demographic changes. Agricultural modernization in the South, followed by mass migration, eventually generated a rising tide of mass volatility, both at the polls and in the streets, which in turn destabilized the regional coalition of the national Democratic party. In the 1960s, the party's response was to cope with these instabilities with a series of measures, ranging from civil rights legislation to urban service programs; one consequence of the urban service programs was to help set off the welfare explosion at the local level. In our analytical scheme, the relationship between mass volatility and the welfare explosion was *indirect*; it was mediated by complex national electoral conditions.

Given the main elements of this analysis, the proper relationships for testing are apparent. One set of relationships is between unstable voting patterns and mass protest, on the one hand, and the national Democratic party's "New Frontier" and "Great Society" legislative and administrative initiatives, on the other. Another set of relationships is between these policy responses and the rising welfare rolls. In short, did mass volatility help explain a series of national policy initiatives that in turn contributed significantly to the subsequent rise in the welfare rolls?

Albritton short-circuited these indirect relationships, and proceeded as if we had said black crime and rioting alone led directly to welfare rises, locality by locality. If we had thought the matter so simple, we would not have troubled to make a detailed analysis of the complex

interaction among economic and demographic change, mass volatility, and national legislative responses, and then between national legislative responses and the subsequent local welfare rises.

Only by tossing aside our entire analysis of national electoral politics could Albritton treat "mass volatility" as equivalent to crime and rioting among blacks. In fact, we included voting instabilities in that term, and we gave voting instabilities great weight. Agricultural modernization drove southern rural blacks northward to the urban strongholds of the national Democratic party, with the consequence that a civil rights bloc formed in the northern wing of the party. The South responded with outrage and voting defections. The resulting regional fissure was first visible in the Dixiecrat revolt of 1948, and it worsened in the elections of 1952, 1956 and 1960 (it also worsened because industrial modernization in the South was thrusting up a new middle class whose political sympathies tilted toward the Republican party). The volatility in black voting patterns which then appeared in the election of 1956, and to a lesser extent in 1960, compounded these problems. Perhaps the national Democratic party could survive defections among southern whites, or among blacks, but it could not survive defections among both.

Civil disorder, welling up in the context of electoral instability, aggravated the national Democratic party's problems all the more. Nor was civil disorder limited to crime and rioting, or just to one contending racial group. Blacks mounted organized protests, exemplified in the southern civil rights movement and the northern welfare rights movement. Southern whites also rose up in protest, ranging from the emergence in the mid-1950s of a movement of "massive resistance" led by the political leaders of the South, to the southern white mob and police violence that erupted at the same time and persisted for ten years.

These various expressions of mass volatility were treated as interactive in our analysis; discontent first took form in the voting booths, and then in the streets, but each manifestation

tended to reinforce the other. When leaders in the northern wing of the party gave legitimacy to civil rights aspirations in order to ensure the allegiance of an enlarging black constituency, they helped spur the emergence of black protest. And when leaders in the southern wing denounced federal action on civil rights in order to mobilize southern voters against the national Democratic party (the strategems were those of unpledged electors, presidential Republicanism, third-party threats, etc.), they gave legitimacy to white protest. From 1948 onward, each successive episode of voting instability exacerbated civil disorder, and each successive episode of civil disorder exacerbated voting instabilities. To say that the national Democratic party, beginning in the late 1940s, was literally being pulled apart along regional lines by these oppositional movements is not an overstatement.

With the approach of the presidential election of 1960, regional divisions had to be repaired, even while civil disorder was accelerating. John Kennedy's campaign, which emphasized civil rights and poverty, signaled the direction of the party's strategy. As black and white insurgency spread, the Democratic administrations of the 1960s promulgated the measures to restore both electoral stability and civil order, and by the end of the decade, these measures had more or less succeeded. Civil rights legislation imposed political modernization upon the South, with the consequence that white mob violence subsided and the civil rights movement was channeled from mass protest to electoral politics; and the enfranchisement of millions of blacks whose allegiance could be counted upon enabled the Democratic party to shore up its southern regional base. In the urban ghettos, quiescence was gradually restored by new federal urban programs which simultaneously provided services to the black poor and hired the black militants. These new urban programs also helped fuel the welfare explosion, for they both enlarged the volume of applicants for welfare, and pushed up acceptance levels by weakening the capacity of the welfare system to fend off new applicants. In turn, more liberal dispensation of relief contributed to the ebbing of civil disorder in the ghettos.

Of all the new service programs, we considered the federal anti-poverty effort the most significant, and we devoted an entire chapter to documenting the various ways in which anti-poverty activities spurred welfare expansion. For one thing, the new "community action" agencies provided expert information and assistance to those seeking welfare benefits, thus

helping to enlarge the volume of applications (up from 500,000 in 1960 to more than 1,000,000 annually, beginning in 1968). Litigation was undertaken by the legal services program which successfully overturned many of the laws and rules disqualifying poor families (residence laws, employable-mother rules, man-in-the-house rules, etc.), so that whole categories of poor people became eligible for assistance (especially in the South, where these exclusionary rules were more vigorously enforced). In addition, anti-poverty agencies provided resources facilitating the emergence of "welfare rights" organizations whose "direct action" tactics generated pressure on welfare officials to make more permissive eligibility decisions. Under the impact of litigation and protest, acceptance rates shot up (from 55 percent in 1960 to 70 percent in 1968). The result was the welfare explosion. Nor were we alone in reaching the conclusion that it was the new service programs, and the anti-poverty program in particular, that did so much to generate the welfare explosion. Among others, the Maryland State Department of Public Welfare, aghast at the fact that its rolls first doubled and then trebled, said in a report issued in the late 1960s that

when people are everywhere encouraged to make use of public welfare—by publicity, by action workers on their own block, by teachers of their children in school, by their doctors and by any social agency they happen to have contact with, a surge of response must be expected. It is the contention of this report that the increase of AFDC caseload reflects this response to the anti-poverty effort . . . and that the poor families of this state . . . encouraged by the national effort to do something about poverty have responded to the use of public services . . . in numbers heretofore unequalled (cited in *Regulating the Poor* [1971], p. 331).

The sole justification given by Albritton for neglecting our emphasis on national electoral analysis is that "the electoral environment [was] constant in its effects throughout the 1960s," while the welfare rolls only went up steeply in the second half of the decade; in other words, a constant cannot explain a variation. To be sure, both the Kennedy and Johnson administrations were "favorable" to the poor, but that hardly means that the national electoral environment was constant. In the first two years of Kennedy's term, the intensifying threat of southern electoral defections, together with the great power of the southern congressional delegation, prevented Kennedy from making good on either his civil rights or anti-poverty campaign rhetoric. It was

only as civil rights demonstrations reached alarming proportions in 1963 (provoked by the events in Birmingham) that Kennedy, and then Johnson, set in motion the initiatives leading to the civil rights acts of 1964 and 1965, and to a series of service programs for the ghettos. The legislation establishing the community mental health centers, the anti-poverty program, aid to school districts with poor children, and the medicaid program were all passed between 1963 and 1965, by which time civil disorder had broken out in the North as well.

The implementation of these programs was not "constant," either; it was shaped and reshaped by the continuing political problems of the national administration. Even as these programs were enacted, rioting and demonstrations worsened both in the North and South, and among both blacks and whites. By mid-decade, the issues of caste, discrimination, and poverty had come to the very fore of national and local politics. In the vortex of these forces, anti-poverty staff became progressively more militant on behalf of poor minority constituents in their dealings with local agencies of government, whether the police, urban renewal authorities, or welfare officials. Thus anti-poverty attorneys had at first shied away from lawsuits that would antagonize local officials; by 1966, the policies and practices of housing authorities, health agencies, and welfare departments were being forcefully and often successfully challenged in the courts. At the same time, anti-poverty staff began to organize the minority poor for direct action against local agencies. Moreover, the federal guidelines being written in this period, and the federal review teams which oversaw their implementation, gave tacit encouragement to these activities, even to the extent of tolerating the mobilization of the poor for mass protests and demonstrations. Mayors, county officials, and governors complained bitterly and vocally that the federal government was financing an assault on them, and they were not wrong. Nor, in the end, could they withstand the assault, and so they too made concessions, whether in the form of the greatly expanded hiring of blacks or the more liberal giving of relief.

There is another, and simpler, sense in which the electoral environment cannot be treated as constant throughout the decade. The policies promulgated in response to the rising tide of mass volatility did not have an immediate impact on any local agency, including the welfare agencies. This was not the Great Depression: here we had no Roosevelt calling upon Congress immediately after his inauguration to enact emergency relief legislation which,

within 18 months, would bring some 20 million Americans onto the relief rolls. Instead we had successive administrations which secured the enactment of one program after another, some of which slowly came to have an impact on the relief rolls. Thus it took time for a legislative strategy to evolve, it took time for that legislation to be implemented at the local level, and it took still more time for the reverberations of the new service programs to be felt in rising welfare application rates, acceptance rates, and then in caseload statistics. The anti-poverty program was framed by Kennedy in 1963, enacted under Johnson in 1964, implemented in 1965 and 1966, and only then did its effects begin to be felt. It was 1967 before class actions initiated by anti-poverty attorneys challenging exclusionary welfare regulations reached the lower federal courts, and it was later still before the Supreme Court ruled on them and the Department of Health, Education and Welfare rewrote its guidelines in conformity with the new court rulings. Even then, many a state and local welfare department remained out of conformity until anti-poverty lawyers and welfare rights organizations brought pressure on HEW to bring recalcitrant departments before "compliance hearings." The impact of the "New Frontier" and "Great Society" programs was thus cumulative, not precipitous.

If these were the main arguments of *Regulating the Poor*, how could anyone who set out to develop empirical tests simply correlate crime and riots with welfare caseload changes? We surmise that Albritton was led astray by our emphasis on the historical role of political disorder in producing relief liberalization. We were prompted to write *Regulating the Poor* because we thought that historians of relief had failed to see that it was not just widespread unemployment, but political unrest among the unemployed, that forced the initiation of relief programs or their periodic expansion, and we reviewed evidence going back four centuries in Europe to develop this thesis. We thought the United States in the 1950s provided still more evidence. Millions were being forced off the land and many remained unemployed in the cities during the "rolling recessions" of the period, but the relief rolls rose by a mere 17 percent, a net increase of only 110,000 families, during the entire decade. It was only with the eruption of widespread civil disorder in the 1960s that the relief rolls exploded, leading us to argue (1971, p. 196) that "the contemporary relief explosion was a response to civil disorder caused by rapid economic change." It was sentences such as this one that Albritton seized



upon in justifying his simplistic correlational approach. But in doing so, he ignored a major qualification. We did *not* argue that insurgency among the unemployed always, or even usually, produces relief liberalization, as the closing sentence of *Regulating the Poor* emphasizes: "The moral seems clear: a placid poor get nothing, but a turbulent poor *sometimes get something*" (1971, p. 338).

In other words, if mass insurgency is a precondition of relief expansion, it is not a sufficient one. There is the further question of the conditions determining how the ruling stratum will respond. If a regime commands the allegiance of other groups not disaffected by economic changes, then it may be free to ignore or repress the disruptors. But when civil disorder reverberates upon a regime whose grip has weakened, perhaps as a consequence of the same economic instabilities that provoke the poor to insurgency, it is more likely that concessions will be made, such as giving relief. Our analysis of the Democratic administrations of the postwar period was intended to demonstrate that a welfare explosion occurred in the 1960s precisely because widespread civil disorder coincided with the growing vulnerability of those regimes; to be sure, rioting was one source of that vulnerability, but it was not decisive.

### Correlating the Wrong Data

If the main arguments of *Regulating the Poor* dealt with national politics, there is still local politics. And on the local level, our general thesis would lead us to expect to find some correlation between rioting and increases in the Aid to Families with Dependent Children (AFDC) rolls. The difficulty is that the relationship between disorder and welfare expansion in particular localities was mediated by the complexities of the national political dynamics we have described, and which Albritton's correlations ignore.

At a minimum, in accounting for variations in correlations locality by locality, one would want to know something about the role that the locality played in a national administration's political fortunes, which has to do in turn with both local voting patterns and the relationship between the national party and the local party. Thus the fact that Chicago's Mayor Daley continued to produce stable majorities, including stable black majorities, led the national administration to tailor its ghetto programs in Chicago to the requirements of the Daley organization. Daley had, after all, been in

sufficient command of the local political exigencies created by migration and unemployment to permit the AFDC rolls in Chicago to rise by an exceptional 80 percent in the 1950s. Correlatively, he remained in sufficient command in the 1960s to moderate the impact of the federal programs and to limit the expansion of the welfare rolls to a mere 107 percent at a time when the rolls in New York City and Los Angeles were quadrupling. One would also have to take into account variations in the structure of state and local AFDC programs, for the pattern of welfare increases depended in part upon the specific mechanisms relied upon to exclude the poor from aid. Thus southern AFDC systems were notorious for the web of laws and rules which simply barred whole categories of the poor outright, while in the North a formally more liberal system relied more upon bureaucratic mechanisms, such as discouraging waiting periods or humiliating investigations. It follows that the increases in the rolls in the southern states were much more dramatically affected by anti-poverty litigation, which knocked down exclusionary statutes all at once in the late 1960s, while the rolls in the northern states were pushed upward somewhat more gradually as a result of the myriad pressures generated by the federal programs on the welfare bureaucracies.

If developments in the national polity could be disentangled from developments in the urban polity, an analysis of the relationship between rioting and welfare increases from city to city would indeed be interesting. Great care has to be taken in marshalling the appropriate data, a point which brings us to the second large flaw in Albritton's critique. He employed the wrong data.

To test the relationship between civil disorder and the welfare explosion obviously requires that riots be correlated with *post*-riot welfare increases; instead, Albritton mainly used a *pre*-riot measure of changes in the AFDC rolls. Roughly three-quarters of the riots throughout the decade occurred just in 1967 and in 1968. In these two years alone, more than 300 persons were killed, some 10,000 injured, and 100,000 arrested. But Albritton's measure of caseload changes, defined as the average of the annual percentage increases, includes the years 1964, 1965 and 1966!<sup>1</sup> Now

<sup>1</sup> See his Table 3, but note that the time period referred to is deceptive. Albritton's data on caseload changes were taken from our tables, all of which end with February 1969. Throughout his critique, both

we certainly thought riots had an impact but, just as certainly, not retroactively. If Albritton had looked up the welfare increases after 1968, he would have found (as we reported in the epilogue to *Regulating the Poor*) that *the greatest period of rioting (1967-68) was followed by the greatest period of welfare growth (1969-70)*. There were 1.5 million families on the AFDC rolls at the end of 1968; by the end of 1970, another million had been added, for an increase of 67 percent in just two years (compared with 107 percent in all of the preceding eight years). Little wonder, then, that while his correlation moved in the right direction, it failed to reach statistical significance.

There is still another large error that depresses Albritton's correlation, and it arises from his superficial understanding of the welfare system. When he computed caseload changes, he was deceived by a welfare system sleight-of-hand in the reporting of caseload statistics; as a result he included some false data which greatly reduce the possibility of a correlation between rioting and welfare rises. Here we must pause to explain.

In 1961, Congress enacted an amendment to the Social Security Act making the two-parent family with an unemployed head eligible for AFDC benefits for the first time. About 67,000 families were admitted to the new AFDC-UP (unemployed parent) program in the 1961-64 period, and another 12,000 cases were added in the 1965-68 period. Albritton included both AFDC and AFDC-UP cases in his correlational analysis, and that was a serious mistake. For the fact is that the AFDC-UP cases did not consist of *new* families accepted for assistance, but of cases already enrolled in the General Assistance (GA) category which has traditionally contained a number of families with unemployed fathers. Since GA costs are borne entirely by states and localities, it was natural enough that the states would promote a wholesale transfer of these particular GA cases to the new AFDC-UP program in order to benefit from federal reimbursement. Thus 10 states opened AFDC-UP programs between April and December 1961. In April, their aggregate GA caseload stood at 212,000; by December, it had dropped to 160,300, a loss of 51,700, while the aggregate AFDC-UP caseload rose to 48,044. Our subsequent interviews with welfare directors confirmed that these cases had simply been transferred from the one category to the other.

AFDC-UP cases were not, in other words, a genuine part of the great expansion of the AFDC caseloads which characterized the 1960s. For technical reasons, we included AFDC-UP cases in our tables in *Regulating the Poor*, but we repeatedly warned the reader that by doing so we had greatly "understated the difference in caseload growth between the earlier and latter parts of the decade" (1971, p. 334). Albritton should have paid attention to this and similar caveats for a reason that is simple to see. In the 1960-64 period, AFDC-UP cases represented 29 percent of the combined AFDC and AFDC-UP rise, thus greatly inflating the 1964 caseload which served as the denominator for calculating subsequent percentage increases in those places which implemented AFDC-UP.

Now with this point made, consider two additional facts. AFDC-UP programs were mainly opened in the northern states (only 4 of 17 southern states did so).<sup>2</sup> Consequently, it was *in the North* that Albritton greatly underestimated the magnitude of the post-1964 caseload rises. Moreover, it was *in the North* that severe rioting was more pervasive (southern cities experienced almost no conflagrations like those which erupted in Watts, Detroit, Newark and many other northern cities). Now, linking this chain of points together, we may see the second general way in which Albritton's correlational procedures are defective. By failing to exclude AFDC-UP cases, the post-1964 caseload percentage increases were *deflated* in the North where rioting was more widespread, relative to the South where rioting was less widespread. His correlation, in short, was depressed by the strong positive relationship between regional variations in both serious rioting and AFDC-UP implementation.

We hope Albritton will perform his correlations again, using appropriate post-rioting caseload measures, and with AFDC-UP cases excluded. Even with the depressing effects of both errors at work, he obtained a correlation which moved in a direction consistent with our analysis. With the errors corrected, the correlations could turn out to be statistically significant.<sup>3</sup>

<sup>2</sup>As to why almost all southern states refused to implement the AFDC-UP program, see *Regulating the Poor*, pp. 132-33.

<sup>3</sup>Albritton does not cite an earlier study which bears on this matter. Writing in *Social Problems* (1974), Michael Betz concludes that there was a strong relationship between rioting and enlarging state and local general assistance expenditures.

text and tables, he consistently refers to 1969 caseloads, as if for the full year.

What, then, of crime and welfare? (As Albritton says, the impact of crime on welfare changes is "hardly a definitive test" of *Regulating the Poor*, but he investigated the matter anyway.) In Tables 1 and 2, the errors of data management are as serious as in the riot correlation tables. We spoke of changes in *black* crime rates; he averaged black and white rates. The measure of *post*-crime caseload changes is inadequate. If shifts in crime rates were to be calculated through 1969, then caseload changes for at least 1969 and 1970 should have been included (and, it will be recalled, these were the two years of the greatest proportional caseload changes in the entire decade). To make matters worse, AFDC-UP cases are included, an error which is even more troublesome than in the earlier discussion of the riot correlations. Consider Table 1. It contains the ten urban counties which led the nation in proportional caseload increases after 1964. Five are in the states of Georgia and Texas, neither of which implemented AFDC-UP, and the other five are northern counties with AFDC-UP. In other words, half of the counties in the correlation, all of them northern, have greatly deflated post-1964 percent caseload changes.

Table 1 is absurdly misleading for an additional reason. We made much of the fact that many of the southern urban counties successfully resisted admitting virtually any families at all to the AFDC rolls, despite the great economic suffering produced by the postwar agricultural upheaval and despite the turmoil of the early 1960s. Consequently, when more cases were finally accepted after 1964, spectacular percentage increases resulted even though the numerical increases were miniscule. The five southern counties on this list are of just this kind, but that is not evident because the table only shows percent changes, not actual caseload changes. Thus Lubbock had a grand total of 100 AFDC cases in 1964; over the next four years it added 300, producing a 300 percent change which placed it at the very top of the list for the entire country. By contrast, New York, with 81,000 cases in 1964, added 111,000 for an increase of only 137 percent, placing it at the very bottom of the list. Similarly, with an addition of only 900 cases, Austin achieved an increase of 180 percent; but with 56,000 cases added, Los Angeles could only show growth of 145 percent. Taken together, the five southern counties on this list contributed a mere 11,000 cases, or less than 3 percent of the total urban county increase after 1964, while the remaining five northern counties contributed 175,000 cases, or 46 percent.

It was bad enough that we had to include these particular southern counties in our list of 121, but to place five of them in a table with an N of ten is ridiculous. To conclude from such a table that "the data do not lend support to the Piven and Cloward thesis" is to use statistics mindlessly.

We note in passing that these errors of data management also undermine Albritton's challenge to our assertion that the welfare explosion took place in the late 1960s. He says that 85 percent of the AFDC-UP increase occurred before 1965, and we have already said why he is wrong to attribute significance to that. He also notes that 38 of 121 urban counties (defined as counties with a central city of at least 100,000 in 1960) had larger percentage increases in the first period; again, with AFDC-UP cases removed, the picture changes radically. Moreover, when caseload changes for the full decade are tabulated (not just for the first eight years), it is clear that virtually all urban counties had much larger percentage increases after 1965. It was truly an explosion, which is just how we characterized it.

#### A Spurious Explanation of the Welfare Explosion

Albritton's attempt to show that the welfare explosion was not a post-1964 phenomenon is curious, since he himself attributes the great rise in the welfare rolls to the social security amendments enacted in 1965; had he succeeded, he would have discredited his own explanation. He argues that the 1965 amendments "extended benefits to millions of new recipients and incentives to states to admit them to the rolls." Once more, he reveals a superficial knowledge of the welfare system, for his illustrations do not bear significantly on the AFDC rolls. He says, for example, that states "were permitted to provide retroactive assistance to AFDC families with children over 18 years of age who were still attending school or with minor children earning income." These changes mainly affected welfare budgets, not caseloads. Previously, earnings by minors had simply been deducted from the family's monthly check, as was that portion credited to the child who became 18 years old. Only under exceptional circumstances (where the 18-year-old continuing in school had been the youngest or only child, or where the minor's earnings had tipped the family over the income eligibility line) would these factors have affected eligibility and thus caseload statistics. In any case, the earnings of minors were not often reported, and

few 18-year-olds in welfare families continued in school.

It is the Medicaid legislation on which Albritton mainly hangs his explanation. He says it created a large incentive for welfare directors to promote caseload growth because of a provision establishing a floor under the new federal medicaid reimbursements equal to 105 percent of the pre-existing medical assistance program. The notion that this provision generated a scramble on the part of welfare officials to enlist people onto the welfare rolls so as to raise the medical assistance costs on which Medicaid floors would be calculated is astonishing, both on logical and empirical grounds. It overlooks the enormous state and local costs such a response would have entailed in return for a slightly raised floor under federal Medicaid payments, since state and local governments had to bear as much as half of both the welfare and Medicaid costs they were presumably hastening to enlarge, no matter what the federal floor. And even if such a large disincentive did not cancel out a small incentive, the rolls would have been affected only in the period immediately after the amendments were passed. In fact, the AFDC rolls rose each year, at faster and faster rates, until the early 1970s.

Moreover, it would have been political suicide for welfare directors to set out to enlarge the rolls, and Albritton does not identify a single administrative mechanism by which caseload growth was deliberately spurred. In principle, intake workers might have been directed to make more permissive eligibility decisions, thus raising acceptance rates; or "out-reach" programs might have been developed to recruit the eligible but unaided, thus enlarging the volume of applications. Albritton does not intimate that either course was pursued, nor do we know of any instances, and so we are left with a not only illogical but a mystical connection between the Medicaid program and caseload growth. Just to be sure, we questioned Mitchell I. Ginsberg, who was New York City's commissioner of welfare in these years, about whether Medicaid provided incentives to welfare directors to bring new families onto the AFDC rolls. He just snorted. In the years after 1965, most welfare directors were progressively more preoccupied with how to stem welfare growth, for the fiscal repercussions of the growth were everywhere producing political headaches for local welfare officials. Moreover, the huge Medicaid costs imposed on states and localities greatly added to these fiscal and political travails (which are now no small factor in the Proposition 13 movement).

It is true, however, that Medicaid generated an incentive for officials in *other* institutions to promote welfare growth. Here Albritton notes that hospitals and health agencies referred indigent patients to welfare for Medicaid cards, and many of these patients were then found to be eligible for welfare benefits as well. But the particular people to whom Albritton refers are "indigent patients in mental hospitals and tuberculosis institutions." Now we wrote *Regulating the Poor* about the explosion in the AFDC rolls, not about the rises in the categorical assistance programs for the aged and disabled (which, in any case, were dwarfed by the AFDC increases in this period). Before undertaking a critique, it would have been helpful if Albritton had got straight which categorical assistance programs he was talking about, and how Medicaid differentially affected them.

Finally, Albritton not only oversimplified our argument, he also trivialized it. In *Regulating the Poor*, we attempted to understand why public relief-giving institutions emerged in Western countries with the decay of feudalism and the growth of market economics. Further, we attempted to understand why these new institutions periodically expanded and contracted, why the proportion of the poor absorbed by them periodically rose and fell. The problem was to probe beneath the expressed motives of particular actors in order to identify more general socioeconomic and political regularities, whether in explanation of the relief explosion in the England of the 1830s or in the United States of the 1930s. At the most general level, the crucial combination of forces appeared to be these: mass unemployment, mass volatility, and vulnerable regimes. In attributing welfare expansion in the 1960s to the Medicaid legislation and other social security amendments, Albritton had an obligation to say how this explanation exemplified, at a more general level, the working-out of large-scale forces that were either similar to or different from those we had identified as being at the heart of relief explosions for nearly four centuries. That Albritton failed to grasp this is evident in his single sentence accounting for the enactment of the Medicaid legislation: "It is neither coincidental nor inconsequential . . . that this legislation was a product of the 89th Congress . . . a Congress determined to advance the interests of the poor" (p. 1010). We attempted to account for the sources of this rather unusual congressional determination in large-scale forces that were emerging in the society to which national electoral leaders were vulnerable. Albritton has

no explanation, and does not seem to sense the need for one. With this statement, he dropped the matter. In a word, Albritton brought neither historical nor theoretical perspective to his critique. He not only trivialized *Regulating the Poor*, he also trivialized the question of why the poor sometimes win something from their rulers.

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# A Reply to Piven and Cloward

ROBERT B. ALBRITTON

*Virginia Polytechnic Institute and State University*

Piven and Cloward correctly believe that I was "led astray by [their] emphasis on the historic role of political disorder in producing relief liberalization" (p. 1014). I mistakenly read their work as an effort to propose a theory explaining historic expansions of welfare relief. Since the "War on Poverty" effort cannot be generalized to other historic periods, I regarded their discussion of this effort as a specific, but unique, mechanism (other historic periods produce their own mechanisms, usually different in structure) for translating social and political forces into public policy. I thought they were positing an interesting and significant theory of welfare policy, a theory focusing on "civil disorder, welling up in the context of electoral instability" (where the "context of electoral instability" would be a constant) to explain expansions of welfare caseloads. If Piven and Cloward see no direct causal impacts of civil disorders, i.e., riots, on welfare caseloads during the 1960s, I can accept this only as a major qualification of their work.

Perhaps I was "led astray" because so much of their analysis argues both implicitly and explicitly the contrary position that mass insurgency during the 1960s produced a rise in welfare caseloads of such proportions as to warrant characterization as a "welfare explosion." Despite their denials, Piven and Cloward claim that civil disorders, i.e., riots, *directly* contributed to expansion of the numbers of families receiving assistance from the Aid to Families with Dependent Children program and that the riots of the 1960s were partly a result of "the rate of increase of the black population" (1971, p. 239). It was these claims and only these claims that I examined in "Social Amelioration through Mass Insurgency. . . ."

## The Relationship between Riots and Welfare

In *Regulating the Poor*, Piven and Cloward state:

In the chapters to follow, we shall argue, as we did in our discussion of the relief explosion of

the Great Depression, that the contemporary relief explosion was a response to the civil disorder caused by rapid economic change (1971, p. 196).

This is only one of their more explicit, unqualified assertions that a direct relationship exists between civil disorders and the level of welfare dependency. One can only wonder at the logic which suggests that statements such as the following: "economic convulsions which also produce mass turbulence—whether riots in the streets or upheavals in electoral alignments—are likely to lead to the temporary liberalization of relief provisions" (1971, p. 197) imply "interactive relationships" between civil disorders and electoral instabilities or an "indirect" impact of these factors on welfare. By linking riots and electoral instabilities with the word "or" rather than "and," their theory appears to hold that these are independent causal factors, not "necessary" and "sufficient" ones as they argue in their response. If they argue that electoral instabilities provide the "context" of the 1960s (although the degree of electoral support for the Democratic party in 1964 is not a *prima facie* example of electoral instability), it is not clear why this "context" should not be treated as a constant for purposes of comparative analysis, in which case the crucial issue is the impact of the presence or absence of civil disorder (the variable) on expansion of the welfare system during this period.

Now, Piven and Cloward also claim that only an indirect relationship exists between mass insurgency/unstable voting and welfare caseloads; that separate sets of relationships must be examined—first, the relationship between civil disorder and/or electoral instabilities) and legislative or administrative initiatives, and second, whether these legislative or administrative initiatives added to the welfare caseloads. This conceptual scheme is more consistent with my emphasis on impacts of specific legislation (which includes major amendments to the Social Security Act dealing directly with the welfare system, but completely missed by Piven and Cloward) enacted substantially *before* the period of the most intense riots, including the War on Poverty legislation. The time sequence

militaltes against the Piven and Cloward view. As riots and other social disorders increased during the 1966–69 period, ameliorative legislation was clearly on the wane. The data may be even more consistent with an inverse relationship—as riots increased, ameliorative social legislation declined; or, conversely, as ameliorative social legislation declined, riots increased. My own view is that as civil rights protests increased during the early 1960s, ameliorative social legislation increased, culminating in the significant legislation adopted and implemented during the 1964–66 period. As major rioting increased during the late 1960s, however, ameliorative social legislation declined. Civil rights protests and riots were perceived very differently, something Piven and Cloward do not seem to comprehend.

Although Piven and Cloward explicitly deny any attribution on their part of a direct relationship between riots and welfare, they continue to affirm such direct relationships even in their response. How else can one read such statements as:

In turn, the more liberal dispensation of relief contributed to the ebbing of civil disorder in the ghettos (p. 1013).

*the greatest period of rioting (1967–68) was followed by the greatest period of welfare growth (1969–70) (p. 1016).*

Even with the depressing effects of both errors at work, he obtained a correlation (sic) which moved in a direction consistent with our analysis (p. 1016).

or, in footnote 3:

Michael Betz concludes that there was a strong relationship between rioting and changes in state and local money payments for General Assistance. Of course, rising money payments mean rising rolls.

The Betz article (1974) contains so many methodological and interpretive problems that it was too difficult to treat along with the ideas of Piven and Cloward in my original analysis. In fact, I read the article as doing considerable damage to Piven's and Cloward's claims (although this is not Betz's intention). The data on welfare spending is not state and local payments for General Assistance as Piven and Cloward say, but all combined expenditures for welfare by state and local governments. Furthermore, Betz's data show that, in a comparison between riot and non-riot cities, expenditures for welfare increased by larger amounts in cities experiencing minor or no civil disorders

than in cities which experienced major riots, 1968–69. (In fact, the major riot cities experienced a net decline in welfare spending during this period. See Betz's Table 1, p. 351.) In another test, applying a one-year lag of effects, the change in welfare spending in the year following the riots differed significantly from changes in other years at a level of significance below the .05 level in only one of the three tests (1974, p. 352). One reason I omitted the Betz article from my analysis was that he claimed to support the Piven and Cloward thesis, but I could never see how the analysis led to that conclusion.

What interests me most about the Betz article is that his theoretical relationships are identical to those suggested in "Social Amelioration through Mass Insurgency. . ." to test the Piven and Cloward thesis. (Apparently, Betz read them the way I did.) Betz uses a comparative form of analysis, with cities as the unit of analysis (I have used urban counties to be consistent with Piven's and Cloward's data), to test *direct* effects of riots or other major civil disorders on expansions of the welfare system. Piven and Cloward cite the Betz article favorably, not because it uses any different theoretical relationships, but because they believe that his conclusions support a position very important to their thesis—that riots cause welfare increases.

### The Validity of the Analysis

Space limitations preclude any detailed consideration of all the distortions and misstatements in the Piven and Cloward response. (For example, nowhere do I argue against the concept of a welfare explosion occurring during the 1960s. Nor do I find the "correlations" in Tables 3 and 5 to which they refer. All I can find are mean percentages and statistical significance tests.) In considering the suitability of the data for the analysis, however, one should note that the data that Piven and Cloward find so flawed are the data from Source Table 2 in their *Regulating the Poor* which they asked readers to accept as supporting their analysis.

In any case, I will rely on their own arguments to justify the data. They indicate, for example, that "an unknown part of the AFDC-UP rolls is composed of families who were transferred from AFDC rolls, or who could have been admitted to the AFDC rolls in the absence of the AFDC-UP category" (1971, n. 184), a factor mitigating, if not eliminating, any error caused by inclusion of AFDC-UP cases. Their suggestion that an analysis be performed excluding the AFDC-UP factor is

clearly facetious, since the data on urban counties does not distinguish between these types of recipients (1971, n. 184). When all factors concerning the data are considered, only "a small part of the Northern increase is due to AFDC-UP" (1971, n. 184).

It is not entirely clear why Piven and Cloward object to including AFDC-UP cases in the data. These are, after all, welfare families from whatever source. While qualifying former General Assistance recipients for AFDC-UP would pose some problems for comparing pre-1965 with post-1965 caseloads, there is virtually no threat to a cross-sectional design comparing changes in caseloads solely from the post-1965 period.

The major problem with Piven's and Cloward's critique of the data is their inability to comprehend the logic of comparative analysis, especially the cross-sectional design. When percentage increases in welfare caseloads for riot and non-riot urban counties over a span of years, 1965 to 1969, are compared, the riots, especially those occurring during 1967-68, are treated as an intervening variable. The percentage increases in welfare caseloads during this period are simply not "pre-riot" measures. Perhaps the reader can make more sense than I can of the statement that my measure of caseload changes "defined as the average of the annual percentage increases, includes the years 1964, 1965, and 1966" (p. 1015). I can only invite the reader to consult Piven's and Cloward's Source Table 2 (1971), subtract 1964 caseload from 1969 caseload, and divide by 1964 caseload. The result will be a percentage figure reported in their column headed "Percent Change 1964-69," the data used for the most crucial tests in "Social Amelioration through Mass Insurgency. . . ." Piven's and Cloward's characterization of the data is simply wrong.

The correlations of crime incidence/rates (not rates alone) increases with increases in welfare caseloads (Table 2) and black population increases (Table 4) in "Social Amelioration through Mass Insurgency. . . ," while not decisive, are damaging to Piven's and Cloward's assumption that the breakdown of social institutions among the black population as a result of migration led to "an alarming increase in serious crimes, such as armed robbery and burglary" (1971, p. 226). These tables show that increases in black populations in urban counties had no association with increases in incidence or rates of armed robbery and burglary. Similarly, although the evidence is weaker, even if blacks were responsible for crime increases, this form of civil disorder had no

impact on welfare caseloads.

Piven and Cloward now argue that the "greatest period of rioting (1967-68) was followed by the greatest period of welfare growth (1969-70)" (p. 1016). This is a peculiar argument not only because they say they do not accept the notion of a direct relationship between riots and welfare, but also because they effectively discounted it by positing an alternative hypothesis in *Regulating the Poor*: "The stimulus for this new upsurge was, we believe, the Nixon Administration's anti-inflation strategy which sharply increased unemployment" (1971, p. 341). In this statement, they suggest a plausible rival explanation of the data that must be controlled before any causal relationship can be asserted between rioting and welfare growth, 1969-70. I suggest a similarly plausible rival hypothesis that explains some part of the welfare increase subsequent to 1964, since I find no relationship between riots and welfare.

Amendments to the Social Security Act, adopted in 1965 and omitted from Piven's and Cloward's analysis, increased federal assistance to states that raised levels of eligibility, thereby qualifying a new stratum of low-income families for assistance. Also under these amendments, families which ordinarily would leave the rolls because the youngest child reached the age of 18 could remain recipients for several more years, sometimes until the child finished college. For the first time in history, states were able to require the federal government to pay medical care costs for persons in state or local mental health or tuberculosis institutions by qualifying them for Medicaid under MAFDC where families were involved. Further, as Piven and Cloward suggest, Medicaid provided incentives for a variety of other institutions to make sure that all eligible patients were appropriately enrolled under public welfare programs. Collectively, these amendments provided substantially extended coverage and incentives to add eligibles to the welfare rolls. Welfare rights movements and community action agencies certainly played a significant role in informing eligible persons of benefits to which they were entitled. However, contrary to Mr. Ginzberg and his associates in New York, I found many welfare caseworkers in Chicago during this period eager to extend benefits to eligible persons whenever possible, not from any sense of threat from welfare applicants, but because they believed that the law intended that eligible persons should be provided assistance, civil service offered them some protection from political retaliation, and because almost none of the AFDC expenditures are borne by the city of



Chicago. (They are borne almost solely by the State of Illinois.)

### Conclusions

If I was "led astray" by Piven's and Cloward's emphasis on the "historic role of political disorder in producing relief liberalization" (p. 1014), they were responsible for leading me (and other scholars who read them the same way, including Michael Betz) thus astray. Social scientists should be always more interested in such relationships that have profound implications for social theory than in excellent, but otherwise essentially journalistic accounts of the War on Poverty. In "Social Amelioration through Mass Insurgency. . ." I examined two sets of relationships that were explicitly posited in *Regulating the Poor*, using essentially the data Piven and Cloward provided. These were that "the difference between [urban areas] that experienced riots and those that did not was the rate of increase of the black population" (1971, p. 239), and that "riots in the streets . . . are likely to lead to the temporary liberalization of relief provisions" (1971, p. 197). The analysis, based on a cross-sectional design sug-

gested by Piven and Cloward in the section "Local Responses to Disorder" (1971, p. 240 ff.) does not support these propositions.

I wonder if Piven and Cloward are prepared to explore fully the implications of the notion that there is no *direct* link between mass insurgency and welfare. Sometimes they seem to mean that civil disorder may or may not produce legislative or administrative responses, that these responses may or may not have some impact on ameliorating systems of social injustice. The evidence that it does is certainly thin. A "turbulent poor sometimes get something?" Like welfare? Well, maybe so, maybe not. In any case, mass insurgency is a highly dubious strategy for middle-class academics to urge upon the poor.

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# Ethnicity and Socioeconomic Status as Sources of Participation: The Case for Ethnic Political Culture

DALE C. NELSON  
Fordham University

*Many studies undertaken in recent decades have documented the effects of socioeconomic status (SES) on political participation. As consensus has grown on the role of socioeconomic status, other factors, like ethnicity, have been relegated to secondary importance. However, variations in levels of participation can be traced, in part, to differences in ethnic political culture. Furthermore, the findings presented in this article indicate that holding socioeconomic status constant does not eliminate the independent impact of ethnicity on political attitudes that affect participation. Ethnicity is shown to have a greater effect than socioeconomic status on levels of participant political culture. How these findings might influence our understanding of social and political inequality is discussed, and students of participation are urged to give more serious attention to the ethnic factor.*

For several decades a strong consensus has been building on the underlying sources of political participation. Increasingly, socioeconomic status is being viewed as the single most important social background influence on levels of participation behavior. Verba and Nie (1972) aptly symbolize this consensus when they refer to the relationship between socioeconomic status and participation as the "Standard SES Model." As consensus has grown on the role of socioeconomic status, attention has been diverted from the influence of other social background factors like personality and ethnicity. Verba and Nie, for example, in their already classic work on participation, make only one passing reference to ethnicity and no reference to personality per se. When compared to earlier works on participation by such writers as Lane (1959), Milbrath (1965), and Berelson, Lazarsfeld and McPhee (1954), the omission is striking and indicative of a trend that plays down the importance of non-socioeconomic factors.

There are two major reasons why ethnicity has been relegated to secondary status as an explanation of American political participation. First, many social scientists have been so

steeped in the normative dimensions of the "melting pot" myth that they are unwilling (or unable) to conceive of ethnicity as an enduring and important element of social division. Certainly race has long been recognized as a lasting basis of socio-political conflict, but the belief that religious and nationality aspects of ethnicity could also withstand the onslaught of the "inevitable" assimilation process is recent.

The second reason (related to the first) centers on the empirical relationship between ethnicity and social class.<sup>1</sup> It is not difficult to distinguish conceptually between social class and ethnic status. In the United States, at any rate, class status is based primarily on the achievement criteria of income, educational level, and occupational prestige, whereas ethnic status is derived from such ascriptive criteria as race, religion, and nationality.<sup>2</sup> But however clear the conceptual distinctions between ethnic and class status, they are nonetheless highly related empirically. Those who share a common ethnic background also tend to share a common

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<sup>1</sup>Researchers commonly employ measures of socioeconomic status as surrogates for the broader, richer concept of "objective social class." The terms "socioeconomic status" and "social class" are used interchangeably throughout this paper.

<sup>2</sup>The concepts of achievement and ascription were, of course, first developed by Talcott Parsons to compare traditional and modern societies. See his work, *The Social System* (1951), for a full description of "pattern variables," including the "achievement-ascription" dimension of societies. Although Parsons uses this pattern variable to compare societies, it is also useful for conceptual distinctions between different forms of social stratification.

class position. And given the high empirical correlation between class and ethnicity, it is difficult to determine whether each has an independent influence on participation. Simply put, the question is as follows: does one individual participate more than (or in different ways from) another because of ethnicity, or because of social class position, or both?

Despite these three possible answers to the question, only the latter two arguments are given much credence in the literature on participation. Many contend that the relationship between ethnicity and political participation is spurious, and that social class is the "true" causal factor. Implicitly, the argument is based on the notion that because socioeconomic status is related to participation in non-ethnic populations, and because ethnic groups can be distinguished one from another along socioeconomic lines, differences in the level of participation between ethnic groups are merely the result of their socioeconomic differences. Milbrath (1965, p. 138) makes perhaps one of the more extreme statements of this position when he claims: "Physiological racial characteristics do not account for participation differences between races; rather, it is the relative social position of racial groupings that create these differences." That there might be non-class differences between races that account, in part, for differences in participation is not acknowledged by Milbrath.

There is also a growing but less prominent body of literature arguing for the joint impact of ethnicity and social class. Those taking this view (implicitly or explicitly) rely on the notion of "ethclass" first developed by Milton M. Gordon (1964). That ethnicity may have an impact independent of its relationship to social class is a position held by relatively few students of participation. One consequence of these views is the paucity of empirical studies of participation which focus on ethnicity. Theories relating ethnicity to political participation are equally rare, and those that have been put forward are not very advanced.

The goal of this article is to develop and test a political culture theory to explain ethnic differences in levels of political participation. I shall focus on non-electoral behavior, where the intention of the participant is clearly to influence the distribution of resources by local political authorities. I will label such participation "communal," and will examine four forms: contacting local public officials about community problems, joining community problem-solving organizations, signing petitions for neighborhood improvement, and attending community political protest demonstrations.

To examine the political attitudes and participation patterns of American blacks, Cubans, Dominicans, Irish, Jews, and Puerto Ricans, I will use data based on survey research conducted in northern Manhattan during 1973. Because the conclusions of this study are based on survey data from only one location (i.e., New York City), a note of caution is in order about how far the findings are generally applicable. Even the most casual visitor to New York is aware of its profoundly ethnic character. Thus, a case could be made that the salience of ethnicity to politics is greater in New York than in most or all other major American cities.

New York, however, is not unique in the fact that its citizens are divided along ethnic lines. Nor are the findings of this study about the effects of ethnicity on political attitudes unique. Using national-level data, Greeley (1974) found strong evidence to support the thesis that ethnicity is a major determinant of political attitudes and participation in America. Lenski (1961) and Laumann (1973) report findings based on survey data from Detroit that are consistent with the thesis of this study, as do Antunes and Gaitz (1975) in their analysis of white, black and hispanic participation in Houston. In short, studies in other contexts cast doubt on the notion that the findings of the present study are applicable only to New York City.

The first section of this article explores the meaning of culture and how cultural differences can affect political participation. A case is made for linking together culture and ethnicity in an explanatory model of participation. Later sections employ multivariate analysis to test the validity of the proposed ethnic cultural model. I will explore the relationship between socioeconomic status and political culture, since the "Standard SES Model" can be viewed as the main alternative thesis to my own ethnic cultural argument. In addition, two other alternative explanations will be examined. The first I will label the *assimilationist* thesis, which maintains that some ethnic groups participate in politics more than others simply because they are more assimilated into mainstream American society. The second, which I will call the *ethnic political consciousness* thesis, contends that ethnic group differences in participation "wither away" when individuals from the least politically active groups gain a strong identification with the political goals and interests of their ethnic groups. The concluding section of the article examines the findings in relation to both the "Standard SES Model" and the issue of ethnic inequality in America.

### Culture, Problem Solving, and Political Participation

While the concept of *culture* is central to anthropology, the use of the term has not by any means been restricted to that discipline; rather, it has been used by economists, sociologists, psychologists, social psychologists, and political scientists as well. The concept of culture, then, ranks with *class*, *status*, *social system*, *power*, and *personality* as one of the basic concepts of the social sciences.

There are many definitions of the complex term, "culture," some of them contradictory. Some definitions are broad and seem to include all forms of social behavior and psychological states, as well as the material products of society like art, literature, food, and music. Other definitions are more restricted.

However, most definitions agree that culture is a shared set of values, norms, beliefs, and attitudes. Furthermore, many definitions stress the relationship between culture and *problem-solving* behavior. Some have even defined culture exclusively in such terms. Ford, for instance, states that culture "consists of learned problem-solutions" (1963, p. 106). Young equates "folkways" with culture and defines folkways as "continuous methods of handling problems and social situations" (1963, p. 106). Oscar Lewis (1966, p. 18), in a discussion of the function of culture for the individual, points out that culture "provides human beings with a design for living, with a ready-made set of solutions for human problems so that individuals in each generation do not have to begin all over again from scratch."

Gluckhohn and Strodtbeck, using the conception of culture as a problem-solving mechanism, have attempted to specify the ways in which culture affects social behavior. They argue that the ethos (culture) of a people is "indispensable to the interpretation of concrete behavior" (1961, p. 1). To them, the best indicators of ethos are value orientations, which are essential for interpreting social behavior because they "give order and direction to . . . human acts and thoughts as these relate to the solution of 'common human' problems" (p. 4). Gluckhohn and Strodtbeck, then, bring two common elements of definitions of culture together. They maintain that the most basic value orientations of cultural groupings are those that guide individual problem-solving behavior.

If culture provides guidelines for problem-solving behavior in general, then it follows that *political* culture acts as a guide to problem solving in the political realm. As Verba puts it,

political culture affects "when and in what ways individuals become involved in political life" (Pye and Verba, 1965, p. 517). Such involvement in politics is often expressed through various forms of political participation. Although some forms (like voting) do not always have a clear-cut problem-solving base, many kinds of participation are essentially motivated by a desire to solve some problem or set of problems. This is especially the case when individuals (alone or with others) contact political authorities or agencies. The problem being addressed is the absence or relative scarcity of some resource (e.g., money, job security, respect, influence, etc.) that people believe can be gained through political participation. Contacting public officials, signing petitions, joining certain types of political organizations, and attending protest demonstrations are all forms of participation that are based on problem-solving motivations.

Since political culture consists of those value orientations which guide people to solutions in the political realm, forms of political behavior that are rather straightforward expressions of problem-solving action should be highly related to political culture. Some people will participate more than others, in part because they exhibit a political culture more supportive of participation—i.e., what might be labeled "participant political culture."<sup>3</sup>

Given the salience of participant political culture as a source of participation behavior, one important task is to establish the determinants of such culture—that is, what explains variations between people in levels of participant culture? Much previous research has identified socioeconomic status as a key factor.<sup>4</sup> While I make no attempt to take issue with the findings of this research, my thesis here is that the impact of ethnicity on participant culture is at least as important as that of socioeconomic status, and may be more important. It can be openly questioned whether the dual processes of acculturation and assimilation have so eroded the cultural bases of ethnic groups that

<sup>3</sup>Gabriel A. Almond and Sidney Verba (1965) coined the term "participant political culture" to apply to the political cultures of nations. There is no reason, however, why the concept cannot also be applied to individuals and groups.

<sup>4</sup>While Verba and Nie (1972) is probably the best current statement of the SES thesis, a vast literature exists linking socioeconomic status to levels of political participation. For a good summary of the major works on this subject, see also Milbrath and Goel (1977), especially Ch. 4.

distinctive ethnic political styles are no longer identifiable. Fundamental value orientations die hard, and unless assimilation runs its full course, distinctive ethnic value orientations will remain to act as guides to distinctive ethnic social and political behavior.

### Measurement of Political Culture

How, then, is political culture to be measured? Triandis (1972, p. 80) points out that value structures are best reflected in attitudes. Thus, to measure political culture, it is necessary to identify basic political attitudes. Of the attitudes Verba identifies as basic to political culture, four stand out as pertinent to the problem-solving framework of the present study: *involvement*, *efficacy*, *trust*, and *civic awareness* (Pye and Verba, 1965, pp. 537-42). A fifth attitude, *community political knowledge*, is also relevant to an examination of political culture and participation in a local setting.

*Political involvement* is an attitude which involves a generalized interest in politics, a tendency to discuss politics with others, and an understanding of political issues. In a problem-solving framework, it reflects the general value an individual places on the potential effectiveness of political solutions in general. Therefore, when individuals exhibit a high level of political involvement, we can assume that they will be predisposed to political action, since the attitude itself reflects the positive value they place on political action.

If political involvement reflects the individual's value orientation to the political system, the *sense of political efficacy* reflects one's views of one's own role in that system. When individuals exhibit a high sense of political efficacy, they place a high value on their worth in the political arena. Political efficacy, then, can be defined as a belief in one's ability to influence political authorities and other political actors. A high sense of political efficacy will tend to encourage participation, because others are perceived to be responsive to the individual's demands. Conversely, we expect a low sense of efficacy to inhibit political participation.

Like political involvement, *political trust* reflects a value orientation toward the political system. Like the sense of efficacy, trust is also an evaluative dimension of political attitudes and measures the degree to which public officials are esteemed. In a problem-solving context, political trust can be defined as the value people place on political authorities as

potential problem solvers. Political trust is a complex attitude that includes an evaluation of the moral character of public officials, their competency, and their responsiveness to people. Individuals who exhibit high levels of trust will tend to be *non-participants*, since they assume it is safe to leave the solution of problems in the hands of trusted public officials. They feel little need to influence decision makers, who are seen as acting on behalf of the general welfare of the community. *Political cynicism*, or low political trust, is therefore presumed to be a source of participation, since it reflects the belief that officials cannot be trusted to act in the best interests of the community without pressure to do so.

*Civic awareness* usually reflects the value one places on the general welfare of the community. Specifically, an individual who knows about community problems, and is also aware of which individuals, groups, or organizations are attempting to solve these problems, is exhibiting civic awareness. It can be hypothesized that those exhibiting high levels of civic awareness will be more prone to seek political solutions to community problems than individuals who are unaware of the nature and severity of such problems or the actions being taken by others to solve them.

*Community political knowledge* means the individual's knowledge of people and places in the community that help people deal with local government. It is related to both political involvement and civic awareness, in that it reflects a high value placed on seeking political solutions and, to some extent, an awareness of what is going on in the community. Those with high levels of community political knowledge will tend to take political action because they are aware of concrete places and people who are likely to respond to their initiatives. Conversely, those who are otherwise politically inclined may not seek political solutions to their problems when they lack knowledge about where to go for help.

### Operational Definitions

I have standardized and combined three survey questions to form a "political involvement index." The questions were as follows: (1) "Some people don't pay much attention to politics. How about you? Would you say that you are very much interested, somewhat interested, not very interested, or not interested at all?" (2) "And what about national political questions? Do you talk about national political questions with people you know well—often,

sometimes, rarely, or never?"<sup>5</sup> (3) "How about local issues in New York City? Would you say that you understand them very well, pretty well, not too well, or not at all?"

The "political efficacy index" was formed from two interrelated questions: "Suppose a law were being considered by the New York City government that you considered unjust or harmful. What do you think you could do? If such a case arose, how likely is it that you would actually do something about it—very likely, pretty likely, not too likely, not likely at all?" Open-ended responses to the question "What do you think you could do?" were dichotomized into "could do something" and "could do nothing." I then standardized and combined the two variables to form the overall index.

The "political cynicism index" was comprised of the following questions: (1) "How much do you think you can trust the government in New York City to do what is right—just about always, most of the time, some of the time, or almost never?" (2) "Would you say that the government in New York City is run for the benefit of a few big interests or that it is run for the benefit of all people?" and (3) "How about New York government officials—

do you feel they generally do their jobs well or don't they—just your impression?" Negative responses were assigned high response code numbers for each question. I standardized and combined the variables to form the overall index.

The "civic awareness index" was comprised of three questions: (1) "How effective would you say local organizations are in dealing with neighborhood problems?" (2) "Have you heard or read anything about the Neighborhood Action Program for this part of the city? Can you remember anything they have been doing?" and (3) "Is there a local (neighborhood) newspaper that carries news about your area? What is the name of the paper?" Because of the large number of "don't know" responses to the first question, I divided responses into "knows about community organizations" and "does not know about community organizations." I coded the other two questions for their cognitive content; that is, if respondents could name an activity of the Neighborhood Action Program they were coded as "yes," if they could name a local (neighborhood) newspaper they were coded as "correct." The overall index was a simple additive one composed of the three dichotomized variables.

"Community political knowledge" was a single variable based on the following question: "Are there any places, or groups, or particular people in this neighborhood who help people deal with city offices and services? What are their names?" The resultant variable was dichotomous—those who could name a person, place, or organization helping people deal with local government were coded "yes," those who could not were coded "no."

The five political attitudes described above were all used as indicators of participant political culture. Further, because the concept of culture generally refers to a set or cluster of values, norms, beliefs, and attitudes, we can anticipate that the five attitudes selected for analysis will be highly related empirically. Table 1, a Pearson *r* (zero-order) correlation matrix,

<sup>5</sup>The survey instrument did contain a question on discussion of *local* politics with friends. It was found to be highly correlated with the other two items of the "political involvement index," but not as highly as discussion of *national* political questions. Since the intent was to measure a general political involvement orientation, discussion of national political questions was selected for the index.

Technically, discussion of politics is a *behavioral* rather than an attitudinal measure. However, it was included as an indicator of political involvement because it is not clearly a form of instrumental behavior (like contacting public officials, for example). That is, the mere discussion of politics with friends and/or acquaintances need not lead to overt problem-solving action. Discussion of politics is better viewed as an expression of conscious interest in politics.

Table 1. Pearson *r* Zero-Order Correlations between Political Attitudes (N=379)<sup>1</sup>

Attitude	1	2	3	4	5
1. Involvement	—	.48	.38	.32	.22
2. Efficacy		—	.26	.19	.17
3. Awareness			—	.40	.23
4. Knowledge				—	.15
5. Cynicism					—

Source: Ethnic Block Survey, New York City Neighborhood Project, Bureau of Applied Social Research, Columbia University, 1973.

explores the assumption that the five political attitudes are intercorrelated.

The findings in Table 1 provide convincing evidence that the attitudes chosen to measure participant culture are strongly related. Each attitude exhibits a positive correlation with the other four attitudes. On the basis of the correlation matrix, I standardized and combined the five attitudes to form an overall attitude index.<sup>6</sup>

Identifying and defining relevant political culture concepts has laid the necessary groundwork for testing the thesis of this study. The section that follows begins with an examination of political problem-solving behavior identified above as communal participation. I used factor analysis to explore the dimensionality of participation variables, and to demonstrate the distinctiveness of communal items from other forms of participation. Next I examined the relationship between communal participation and participant culture, to test the proposition that political attitudes selected for analysis do in fact tend to predispose individuals to become communally active.

#### Communal Participation and Participant Culture

A number of survey questions were used to determine whether the respondent had in the

<sup>6</sup>A principal factor analysis (PA2), without rotation, was also performed using these five attitudes. Only one factor resulted. Significant loadings (i.e., greater than .50) existed for all but political cynicism, which had a loading of only .32. Its lower loading reflects the fact that cynicism does not correlate very well with efficacy and knowledge. However, because its correlation is high with political involvement and civic awareness, and because of the theoretical import of cynicism to political analysis in general, it was decided to include cynicism in the overall index of political attitudes.

past few years contacted a New York City official or agency or performed other political problem-solving acts. The seven acts identified by the questionnaire were as follows: (1) contacting (i.e., calling, writing, or visiting) a city official or agency about neighborhood problems; (2) attending a community protest meeting, joining a protest parade, or picketing about neighborhood problems; (3) signing a petition about neighborhood problems; (4) joining a community organization to deal with neighborhood problems; (5) voting in a city-wide election; (6) individually contacting an unemployment, social security, or welfare office for help with a problem related to financial subsistence; and (7) contacting a city official or agency to seek personal (particularized) services. Table 2 reports the proportion of respondents performing each act.

With the aid of factor analysis I attempted to determine whether the four *communal* participation acts (contacting officials, joining organizations, signing petitions, and attending protest demonstrations) constitute a form of political behavior distinct from voting, subsistence political activity, and personal service seeking. Table 3 presents the factor loadings (without rotation) from a principal factor analysis (PA2) of the seven kinds of political activity. I included only U.S. citizens in the analysis, since voting and subsistence political activity require such citizenship.

The data in Table 3 are quite revealing as to the nature of political participation at the local level. The table demonstrates that there are four distinct dimensions of political activity, three of which are interrelated. Subsistence activity is shown to constitute one dimension by itself. It is uncorrelated with the communal items and has a moderately negative correlation with personal service seeking (component II). Voting, while more highly correlated with communal activity, has too low a factor loading to be considered a communal act. Seeking

Table 2. Percent of Respondents Performing Political Problem-Solving Acts (N=379)

Problem-Solving Act	Percent	No.
Voting	52.8	(200)
Signing Petitions	39.6	(150)
Seeking Subsistence Aid	34.6	(131)
Seeking Personal Services	20.1	( 76)
Contacting Officials About Community Problems	15.0	( 57)
Joining Community Organizations	13.2	( 50)
Attending Community Protests	7.7	( 29)

Source: Ethnic Block Survey, New York City Neighborhood Project, Bureau of Applied Social Research, Columbia University, 1973.

**Table 3. Principal Factoring with Iteration (PA2):  
Unrotated Factor Matrix for Seven Political Acts among Citizens Only (N=275)**

Political Act	Component I	Component II	Communalities
Joining Organizations	.71	-.25	.56
Contacting Officials	.60	.04	.36
Signing Petitions	.51	.07	.26
Attending Protests	.48	.04	.23
Voting in City Elections	.28	.04	.08
Personal Service Seeking	.20	.56	.36
Subsistence Activity	-.09	-.19	.05
Percent of Total Variance	77.4	22.6	—
Eigenvalue	1.47	0.43	—

Source: Ethnic Block Survey, New York City Neighborhood Project, Bureau of Applied Social Research, Columbia University, 1973.

personal services (particularized contacting) has a weak positive loading (.20) on component I, but component II explains a much higher level of its variance (i.e., 31.6 percent); it is therefore best viewed as a separate dimension of participation.

Subsistence political activity is essentially unrelated to all three dimensions of participation. It does not exhibit the "voluntarism" characteristic of the other three items, and is therefore best conceived as "non-participatory" political activity. That service seeking, voting, and communal participation are intercorrelated suggests that they are all measuring some form of participation. But the results of the factor analysis lead to the conclusion that it is better to construct three distinct measures of participation (communal, electoral, and service seeking) than to create a single participation index. Taking into account the size of factor loadings and correlation analysis, I combined the four communal items into a simple additive index that shall be called "communal participation."

Having established the distinctiveness of communal participation, next I explored its relationship with participant culture. Table 4 presents the Pearson  $r$  zero-order correlations between participant culture and communal participation items, reporting individual communal acts and political attitudes to give a more comprehensive picture of the relationship.

The importance of political attitudes to communal participation can be easily ascertained from Table 4. The relationship between the participant culture and communal participation indexes is particularly strong ( $r = .49$ ). As a predictor variable, participant culture alone explains about 24 percent of the variance in participation. It is clear that any factor which significantly affects participant culture will also strongly influence communal participation, albeit indirectly. Others have demonstrated the central role that socioeconomic status plays in the formation of participant culture. In the data analysis to follow, multivariate techniques will be used to demonstrate that ethnicity also

**Table 4. Pearson  $r$  Zero-Order Correlations between Political Attitudes and Communal Participation (N=379)**

Attitude	Participation Index	Contact Officials	Join Organizations	Sign Petitions	Attend Protests
Involvement	.36	.30	.26	.26	.18
Awareness	.37	.28	.27	.28	.16
Efficacy	.33	.23	.25	.25	.17
Knowledge	.30	.34	.19	.19	.14
Cynicism	.24	.14	.16	.17	.22
Attitude Index	.49	.40	.35	.35	.27

Source: Ethnic Block Survey, New York City Neighborhood Project, Bureau of Applied Social Research, Columbia University, 1973.



has a powerful impact on participant culture, and that its role in the formation of attitudes is at least as strong as that of socioeconomic status.

### The Effects of Ethnicity on Participant Culture

Ethnic groups have been defined alternatively as cultural groups, psychological groups, social groups, social class groups, status groups or as some combination of these. But at the individual level of analysis the important distinction to be made is between *psychological* and *nominal* identity. Psychological ethnic identification is subjective, in the sense that individuals decide for themselves to which ethnic group they "belong," or for that matter whether they feel *any* attachment to their ethnic heritage. In the United States, at any rate, nominal identity is usually based on the more objective factors of race, religion, and nationality. For the purposes of this study, individuals will be classified as to their nominal ethnic identity. Since my goal is to examine political value orientations, and not the effects of ethnic consciousness on political behavior, the objectively based nominal identification of ethnicity appears to be more appropriate. References to ethnicity, then, shall mean nominal ethnic identity.

The survey question used to determine nominal ethnic identity is as follows: "Here is a list of several groups. [The respondent was handed a sheet with ethnic group names, including "other."] Which *one* group best

describes the background of most of your ancestors?" The question clearly elicits an "objective" response, since the respondent is not asked how closely she/he identifies with an ethnic group.

How much impact does ethnicity have on participant culture? Put another way: to what extent does the knowledge of an individual's nominal ethnic identity enable one to predict his/her political attitudes? Table 5 begins answering this question by way of multiple classification analysis; its purpose is to determine how much variation in participant culture is "explained" by ethnic identity alone. Six ethnic groups will be analyzed: Irish, American blacks, Jews, Puerto Ricans, Cubans, and Dominicans.

The data reported in Table 5 provide strong initial support for the proposition that ethnicity is a major predictor of participant culture. With an  $\eta^2$  (or  $R^2$ ) of .601 with the overall attitude index, it can be said that ethnic identity alone explains 36 percent of the variation in participant attitudes. And although the amount of variance explained by ethnicity does vary among individual items of the attitude index—from a low  $\eta^2$  (or  $R^2$ ) of .098 to a high of .312—for all five items the correlation is substantial.

There is the possibility, however, that the relationship discovered between ethnicity and participant culture is spurious. Social class differences between ethnic groups are large, and are reflected in the rather high correlation between ethnicity and socioeconomic status ( $R = .48$ ). Therefore, participant culture differences could be solely or primarily a function

Table 5. Multiple Classification Analysis (MCA): Political Attitudes with Ethnic Identity\* (N=379)

Ethnic Identity	Attitude Index	Civic Awareness	Political Involvement	Community Political Knowledge	Political Cynicism	Sense of Political Efficacy
Irish (n=58)	.72	.77	.49	.36	.22	.52
Black (n=63)	.58	.56	.24	.49	.40	.21
Jewish (n=69)	.38	.30	.42	.20	.24	.07
Puerto Rican (n=50)	-.10	-.31	.04	-.18	.22	-.10
Cuban (n=78)	-.58	-.57	-.34	-.38	-.47	-.15
Dominican (n=61)	-.89	-.67	-.79	-.44	-.46	-.52
**Eta=	.601	.559	.451	.362	.362	.313
**Eta <sup>2</sup> =	.362	.312	.204	.131	.131	.098

Source: Ethnic Block Survey, New York City Neighborhood Project, Bureau of Applied Social Research, Columbia University, 1973.

\*All attitudinal variables are standardized; thus, numbers reported in the table are plus/minus standard deviation units from the mean of zero.

\*\*Eta and Eta<sup>2</sup> are equivalent to  $R$  and  $R^2$  derived from multiple regression analysis.

of socioeconomic differences between ethnic groups. On the average, members of white ethnic groups like the Irish and Jews tend to be much older than blacks and hispanics residing in central city areas, so ethnic differences could also reflect the impact of age on participant culture. Further, men were inadvertently oversampled in some ethnic groups, and owing to traditional sex role norms, men may tend to exhibit higher levels of participant culture than women. Finally, ethnic groups also vary in terms of their average length of residence in the community. It is conceivable that the longer one resides in a community, the more knowledgeable one becomes about community political processes; hence, ethnic differences in participant culture could simply be a reflection of demographic factors like length of residence. More important, longer residency may reflect higher levels of assimilation and acculturation. Length of residence, then, may be a surrogate measure of cultural adaptation and may account for much of the difference in participant culture exhibited by ethnic groups in this study.

Table 6 is an attempt to test for possible spuriousness. Socioeconomic status,<sup>7</sup> age, sex,

<sup>7</sup>The "index of socioeconomic status" is composed of three variables: educational achievement (number of school years completed), score on the National Opinion Research Center's occupational prestige scale,

and length of residence<sup>8</sup> are introduced into multiple regression analysis along with ethnicity, to determine whether ethnic differences are "real" (i.e., the result of ethnic factors) or simply by-products of other correlates of participant culture.

Table 6 provides interesting information, which requires some interpretation. Both ethnicity and socioeconomic status prove to be the strongest predictors of participant culture. Length of residence has a marginally important impact ( $\beta = .13$ ), while sex and particularly age explain very little about political attitudes.<sup>9</sup>

and family income before taxes. Variables were first standardized and then combined to form the overall index.

<sup>8</sup>The variable "length of residence" was computed by dividing the number of years the respondent had lived in New York City by his or her age. Such a procedure was necessary because of the obvious correlation between age and length of residence. In effect, "length of residence" refers to the "percentage of one's life spent living in New York City."

<sup>9</sup>When sex and age were introduced last into the analysis via stepwise regression, they explained less than 1 percent additional variance in political attitudes. The relationship between age and political attitudes was also examined for possible curvilinearity, but the  $\eta$  was found to be statistically insignificant. Since the betas for sex and age suggest that these variables add little to the explanation of political

Table 6. Multiple Regression: Political Attitudes with Ethnic Identity and Selected Control Variables (N=379)

Independent Variable	Pearson $r$	Beta
Socioeconomic Status	.49	.31**
Sex (Male +)	.19	.09*
Age	.01	-.01
Length of New York City Residence	.50	.13*
Ethnic Identity <sup>a</sup>	.60 <sup>b</sup>	(.41) <sup>c**</sup>
Irish (n=58)	.13	.15**
Blacks (n=63)	.08	.18**
Puerto Ricans (n=50)	-.16	-.07
Cubans (n=78)	-.39	-.18**
Dominicans (n=61)	-.46	-.23**

Multiple  $R = .69$

Multiple  $R^2 = .47$

Source: Ethnic Block Survey, New York City Neighborhood Project, Bureau of Applied Social Research, Columbia University, 1973.

<sup>a</sup>Nominal ethnic identity is represented by dummy variables; Jews are the excluded (i.e., comparison) group.

<sup>b</sup>Coefficient is a Pearson  $R$ .

<sup>c</sup>Beta coefficient obtained from Multiple Classification Analysis (MCA).

\* $p < .05$

\*\* $p < .01$

A comparison of unstandardized regression coefficients with their standard errors for all independent variables reveals that a strong probability that the betas are zero exists only for the variables age and Puerto Ricans. The fact that the multiple  $R^2$  for the entire regression is .47 means that the independent variables explain almost half of the variance in participant culture.

A problem arises, however, in attempting to interpret the effects of ethnicity, socioeconomic status, and length of residence. The beta for each is substantially lower than its zero-order correlation.<sup>10</sup> Reductions in correlation reflect the fact that these three independent variables are highly intercorrelated: i.e., socioeconomic status with ethnicity,  $R = .48$ ; socioeconomic status with length of residence,  $r = .40$ ; and ethnicity with length of residence,  $R = .69$ . I therefore performed a commonality analysis to sort out the unique and common variances explained.<sup>11</sup> Table 7 reports the results.

attitudes, they can safely be dropped from the analysis that follows.

<sup>10</sup>Using multiple classification analysis I calculated a summary *partial* beta of .42 for ethnicity, controlling for socioeconomic status and length of residence. Given that the zero-order relationship between participant culture and ethnicity is .60, the effects of ethnicity on participant culture appear to be somewhat contaminated with socioeconomic status and length of residence.

<sup>11</sup>Commonality analysis, sometimes called elements analysis, is a method of decomposing the variance of a dependent variable into common and unique variances to allow the analyst to interpret the

The commonality analysis reveals much about the joint and separate impacts of ethnicity, socioeconomic status, and length of residence. Length of residence is shown to be a very weak predictor of participant culture. It uniquely explains only 0.7 percent of the variance in political attitudes, compared to 11.4 percent for ethnicity and 7.9 percent for socioeconomic status. Its effects are particularly related to ethnicity (i.e., length of residence and ethnicity share 10.4 percent of the explained variance); and the fact that a large portion of variance explained by ethnicity (i.e., 11.4 percent) is *not* shared with length of residence suggests a spurious direct relationship between length of residence and political atti-

relative influences of  $n$  independent variables. In the three variable case, the following formula applies:

$$R^2y.123 = U(1)+U(2)+U(3)+C(12)+C(13)+C(23)+C(123).$$

For identifying each unique and common variance, the following formula applies:

$$U(1) = -R^2y.23 + R^2y.123$$

$$U(2) = -R^2y.13 + R^2y.123$$

$$U(3) = -R^2y.12 + R^2y.123$$

$$C(12) = -R^2y.3 + R^2y.13 + R^2y.23 - R^2y.123$$

$$C(13) = -R^2y.2 + R^2y.12 + R^2y.23 - R^2y.123$$

$$C(23) = -R^2y.1 + R^2y.12 + R^2y.13 - R^2y.123$$

$$C(123) = R^2y.1 + R^2y.2 - R^2y.3 - R^2y.13 - R^2y.23 + R^2y.123.$$

See Kerlinger and Pedhazur (1973, pp. 297-305) for a more complete discussion of commonality analysis.

Table 7. Commonality Analysis: Unique and Common Variances in Political Attitudes Explained by Ethnic Identity, Length of Residence and Socioeconomic Status (N=379)

Variance Explained	1 Ethnic Identity	2 Length of Residence	3 Socioeconomic Status
$R^2$ Unique to 1	.1141		
$R^2$ Unique to 2		.0070	
$R^2$ Unique to 3			.0792
$R^2$ Common to 1 and 2	.1040	.1040	
$R^2$ Common to 1 and 3	.0179		.0179
$R^2$ Common to 2 and 3		.0149	.0149
$R^2$ Common to 1, 2, and 3	.1255	.1255	.1255
Sum:	.3615	.2514	.2375
Multiple $R^2 = .4626$			

Source: Ethnic Block Survey, New York City Neighborhood Project, Bureau of Applied Social Research, Columbia University, 1973.

tudes. Ethnicity and socioeconomic status, then, emerge as the key explanatory variables. Together they explain about 45.5 percent of the variance in participant culture. (See Table 8.) It is clear that eliminating age, sex, and length of residence from the explanatory model does little to diminish its predictive capability.

Results of the commonality analysis suggest the need for a closer examination of the separate and joint impacts of ethnicity and socioeconomic status. Which one has the greater impact on participant culture? How much variance in participant culture is uniquely explained by each? How much explained variance do ethnicity and socioeconomic status share in common? In order to answer these questions, I used commonality analysis again. Table 8 reports the results for the participant culture index and each of the five attitudinal items making up the index.

The results reported in Table 8 are startlingly clear regarding the joint and separate impacts of ethnicity and socioeconomic status. On the average, for all six items in the table, about 72 percent of the variance in participant culture explained by these two variables can be attributed to the *unique* contributions of each. Interaction effects (multiplicative terms for each ethnic group and socioeconomic status) were examined and found to be statistically insignificant. Thus, although ethnicity and socioeconomic status do share some of the variance explained in participant culture, by far the greatest amount of variance is attributable to their *independent* effects.

Table 8 clearly points to the conclusion that ethnicity has the greater relative impact on participant culture. For the overall participant culture index, ethnicity exhibits more than twice the explanatory capacity of socioeconomic status; for cynicism and community political knowledge, approximately three times;

and for civic awareness, ethnicity exhibits an amazing ten times the explanatory power of socioeconomic status. Only for involvement and efficacy are the impacts of the two factors roughly equal (i.e., for involvement, ethnicity explains 9.8 percent of the variance uniquely, while socioeconomic status explains about 6.5 percent; for efficacy, 6.1 percent and 6.3 percent, respectively).

There are two remaining alternative hypotheses that might explain the high predictive capability of ethnicity. First, several of the ethnic groups examined (Cubans and Dominicans, for example) are largely *unassimilated*. Although it was earlier demonstrated that length of residence in New York City has little impact on communal participation or participant culture, other dimensions of assimilation could be affecting our findings. An argument could be made that as intermarriage rates increase, as friendship circles become multi-ethnic, as attachments to cultural institutions (e.g., religious institutions) break down, and as hispanic groups become more proficient in the use of English, levels of participant culture should be similar for the various ethnic groups in the sample.

A second alternative hypothesis centers on the role of *ethnic political consciousness*. Many students of ethnic politics maintain that high levels of ethnic political involvement are largely a function of two interrelated aspects of consciousness raising: (1) a belief that members of the mainstream society discriminate against one's ethnic group, and (2) a positive identification with ethnic group goals and interests (see, for example, Antunes and Gaitz, 1975; Glazer and Moynihan, 1975, pp. 1-26; Leggett, 1968; Wolfinger, 1965; Verba and Nie, 1972). It might be expected, then, that as ethnic political consciousness increases, a political culture more supportive of participation will develop. Thus,

Table 8. Commonality Analysis: Variances in Participant Culture Explained by Ethnic Identity and Socioeconomic Status (N=379)

Participant Culture	R <sup>2</sup> Unique to Ethnicity	R <sup>2</sup> Unique to Socioeconomic Status	R <sup>2</sup> Common to Both	Total R <sup>2</sup>
Overall Index	.218	.094	.143	.455
Awareness	.232	.023	.080	.335
Involvement	.098	.065	.106	.269
Knowledge	.090	.031	.041	.162
Cynicism	.089	.027	.042	.158
Efficacy	.061	.063	.037	.161

Source: Ethnic Block Survey, New York City Neighborhood Project, Bureau of Applied Social Research, Columbia University, 1973.

once ethnic political consciousness is statistically controlled, political culture differences between nominally defined ethnic groups could be small or nonexistent.

I employed stepwise regression analysis to test the alternative theses that variations in levels of assimilation and/or ethnic political consciousness account for differences in participant political culture between ethnic groups. Variables measuring assimilation (i.e., intermarriage rates, inter-ethnic friendship patterns, and religiosity) and ethnic political consciousness (i.e., preferences for ethnic friends and neighbors, beliefs about the need to take political action with ethnic cohorts, and the belief that New York City government officials discriminate against the respondent's ethnic group) were all entered in step one of the regression equation along with socioeconomic status.<sup>12</sup> Nominal ethnic identity was entered in step two. If lack of assimilation or low levels of ethnic consciousness account for participant culture differences between ethnic groups, we can assume that nominal ethnic identity will explain little or no additional variance in participant culture once these variables are controlled, i.e., its additional  $R^2$  will approximate zero.

<sup>12</sup>Along with socioeconomic status, three other indexes were constructed for control purposes. The "ethnic social cohesion index" is composed of four items: (1) *intramarriage*, i.e., whether respondent is married to a member of the same nominal ethnic group, (2) *ethnic friendship patterns*, i.e., the percentage of one's best friends who share one's ethnic identity, (3) *primary ethnic social intimacy*, i.e., the degree to which one discusses problems *only* with relatives and close friends, and (4) *religiosity*, i.e., the rate of attending religious services at the religious organization modal to the respondent's ethnic group. The second index is labeled "ethnic social consciousness," and is composed of responses to two statements: (1) "I prefer to make friends mostly with other (respondent's ethnic group)," and (2) "I prefer to live in neighborhoods where there are many other (respondent's ethnic group)." The third index is labeled "ethnic political consciousness," and is composed of responses to three items: (1) "(members of respondent's ethnic group) should take political action to help other (members of respondent's ethnic group)," (2) "Sometimes it is necessary for (members of respondent's ethnic group) to use militant tactics to fight for the things (members of respondent's ethnic group) need," and (3) whether or not the respondent, in an open-ended statement, identified his or her own ethnic group as one receiving worse treatment from New York City government than other ethnic groups. Variables were standardized and combined to form three separate indexes.

The assimilationist and political consciousness theses were not confirmed by stepwise regression analysis: ethnic identity continued to explain about 18 percent of the variance in participant culture when assimilation, consciousness, and socioeconomic status were controlled, compared to 22 percent when only socioeconomic status was controlled. In addition, the effects of English proficiency (as measured by language of interview) were examined among the three hispanic groups only ( $N = 189$ ). Although the zero-order correlation ( $r$ ) between participant culture and English proficiency was .316, when social background variables (nominal ethnic identity, socioeconomic status, sex, age, and length of residence), variables measuring assimilation, and political consciousness variables were controlled, the resultant beta for English proficiency was only .089. Knowing an individual's level of English proficiency, then, adds little to our understanding of variations in participant political culture that is not already explained by other variables in the analysis.

Thus, the thesis that ethnic cultural differences persist and are reflected in political attitudes that predispose individuals to participate in politics continues to receive strong support from the data.

### Conclusion: Ethnic Political Culture, Participation, and Inequality

The class-caste controversy has a long and venerable tradition in sociology. At issue in this controversy is the relative influence of social class versus ethnicity as sources of socio-political behavior. For many students of political participation the class-caste controversy appears to have been resolved in favor of social class, or at least in favor of the joint (ethclass) effects of ethnicity and social class. Apparently researchers assume that the homogenizing processes of assimilation and ethnic socioeconomic mobility reduce the effects of ethnic culture to a marginal role in explaining participation behavior.

The findings of this study raise serious doubts about the idea that ethnic cultural influences on participation are withering away. Large differences in levels of participant culture were found to exist between ethnic groups. Controlling for socioeconomic status, age, sex, length of residence, assimilation rates, and levels of ethnic consciousness did not eliminate the cultural effects discovered in correlation analysis. Our analysis also demonstrated that both ethnicity and socioeconomic status make

*independent* and powerful contributions to the explanation of participant culture. Ethnicity, however, proved to be a *better* predictor of participant attitudes than socioeconomic status.

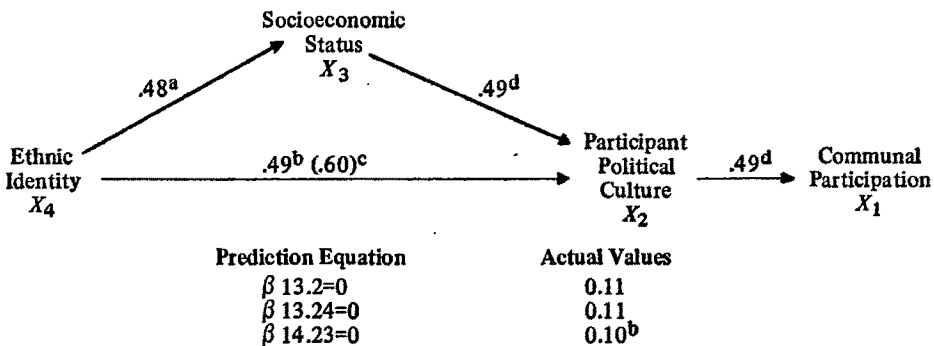
The data reported in Figure 1 provide strong evidence for the importance of ethnic political culture. Socioeconomic status and nominal ethnic identity are shown to have minimal direct effects on communal participation once participant political culture is controlled. However, both ethnicity and socioeconomic status have powerful effects on participant culture, as demonstrated by the size of the path coefficients in the diagram. Further, after controlling for ethnic identity, the resultant partial beta for the relationship between socioeconomic status and participant political culture was .35. Since the corresponding partial beta for the relationship between ethnic identity and participant culture (i.e., controlling for socioeconomic status) was .49, we can conclude that ethnic identity has a greater direct effect on participant culture than socioeconomic status.

The findings reported in Figure 1 also lead to a reconsideration of the adequacy of Verba's and Nie's "Standard SES Model" of participation. It is no doubt accurate as far as it goes, but other factors ignored by the standard model (like ethnicity) may add a great deal to our understanding of participation and political inequality. The Verba and Nie standard model

clearly symbolizes the consensus that has developed on the primacy of social class as the fundamental source of variation in levels of political participation. By placing their findings in the context of social inequality in America, they make a convincing argument that upper-status individuals have disproportionate access to political decision makers, and that such access can lead to inequality in the distribution of benefits.

While Verba and Nie do examine other potential sources of political inequality (like sex, age, party affiliation, and organizational membership), their analysis fails to give serious attention to inequality that is not directly linked to social class. True, they do analyze the role of race in political participation, and race is certainly an important dimension of ethnicity, but the authors' treatment of race is restricted to its relationship to socioeconomic status and race consciousness. Obviously, whites and blacks are differentiated along class lines, which explains many differences between the races in political participation. Race consciousness can also serve as a resource for increasing rates of black participation. But race is only one dimension of ethnicity, and socioeconomic status and social discrimination are only two factors leading to political inequality between ethnic groups.

This study, with its focus on ethnic political



Source: Ethnic Block Survey, New York City Neighborhood Project, Bureau of Applied Social Research, Columbia University, 1973.

<sup>a</sup>The one-way arrow leading from ethnic identity to socioeconomic status reflects the assumption that one's ethnic identity affects one's life chances, including economic opportunities. It is in this sense that ethnic identity can be viewed as a "cause" (albeit not the only one) of socioeconomic status. Clearly, socioeconomic status cannot be viewed in any sense as a cause of nominal ethnic identity.

<sup>b</sup>Partial beta obtained from multiple classification analysis (MCA).

<sup>c</sup>Coefficient in parentheses is a Pearson *R*.

<sup>d</sup>Coefficient is a Pearson *r* (i.e.,  $p_{12}=r_{12}$ ;  $p_{23}=r_{23}$ ).

Figure 1. Path Model of Communal Participation

culture, goes beyond Verba and Nie in addressing the relationship between participation and social inequality. Since people are predisposed to participate in politics because of certain political attitudes, these attitudes can be viewed as *political resources* that are unequally distributed. Political cultures strongly supportive of participation can also be viewed as *group resources*. Ethnic groups whose members exhibit such strong participant cultures stand to benefit politically because their members are more politically active. On the other hand, ethnic groups with weaker participant orientations may well be disadvantaged vis-à-vis benefits distributed through the political system. Discrimination and low socioeconomic status have been demonstrated by others to limit the opportunities of some ethnic groups to receive benefits from the political system. Weak participant political cultures may well be a third basis for political and therefore social inequality.

Several authors have identified ethnicity as an important source of political participation. Among the best-known works on the subject are those of Greeley (1974) and Wilson and Banfield (1965, 1971). Using Verba's and Nie's data, Greeley maintains that ethnicity is "a meaningful predictor of political participation in American society" and that "its impact does not go away when social class is held constant" (p. 170). He pessimistically concludes that "given the poverty of research done on American ethnic groups, it is impossible to generate any hypotheses about their political participation" (p. 173). While my findings support Greeley's contention that ethnicity is a meaningful predictor of participation, even when socioeconomic status is held constant, I do take issue with Greeley concerning the possibility of explaining ethnic differences in participation. My theory, admittedly a partial one, proposes a political culture explanation that links ethnicity to communal participation.

The works of Wilson and Banfield are particularly relevant here, because they also argue a cultural thesis in relation to ethnic participation. However, their "political ethos" theory is significantly different from the ethnic culture argument of this article. The authors present the rather questionable thesis that working-class white ethnic culture, nurtured historically by the urban political machine, is a "private-regarding" political ethos (i.e., it stems from a private, personalistic motivation rather than a "public regarding" or community-minded motivation). The political culture thesis argued here deals not with private versus public *motivations* for political behavior; rather, it contends that ethnic groups vary in participant

culture supportive of participation. Furthermore, Wilson and Banfield posit an explicitly "ethnaclass" argument that links their "private-regarding" ethos to working-class populations. They therefore fail to consider the separate impacts made by socioeconomic status and ethnicity on political participation.

The discovery of cultural differences between ethnic groups does not necessarily lead to a pessimistic conclusion about the future of ethnic political equality in America. Cultures do change. Low or high levels of participant culture are not frozen into a group's existence. But cultural change is slow if such change is the result only of individual adaptation. Dramatic events, increased organizational activity, and mobilization can be especially important for enhancing levels of participant culture. Black Americans, for example, may have acted as a prototype for other disadvantaged groups, and there is evidence that the experience of blacks has had a kind of "demonstration effect" on American Indians, Chicanos, and Puerto Ricans during the 1960s and 1970s. In addition, high levels of ethnic political consciousness and increased social interaction with members of the broader community can both lead to increased political involvement. Thus there are a number of factors that can affect participant political culture, and such changes need not come about slowly.

However, if ethnic divisions are going to be enduring features of the socio-political landscape, social scientists and policy makers need to understand more fully the nature and implications of such divisions. To date, public policy has responded to ethnic inequality primarily in terms of improving opportunities for better education, higher income, and more satisfying jobs, while concurrently seeking to ameliorate social discrimination through legal redress of grievances. In the future, it may be necessary for decision makers to address inequality in terms of political culture resources, particularly those that inhibit political participation.

At the level of theory building there is a strong need to clarify the role played by ethnicity in political participation. In particular, theorists should focus more closely on the identification of factors which create and maintain socio-political inequality among ethnic groups. It is already clear that there are social and economic sources of ethnic political inequality. It may become increasingly clear that a low level of participant culture is yet another source of political inequality among ethnic groups, and one which is in need of serious attention if inequality based on ethnicity is to be reduced or eliminated.

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# The Development of Party Identification among Adults: Exploration of a Functional Model

W. PHILLIPS SHIVELY  
*University of Minnesota*

*This article proposes a model for the systematic development of adults' party identification, based on voters' need for a way to handle difficult electoral decisions. Several variables are noted which should heighten this need, thus making it more likely that voters will develop party identification. The model is partially tested, in an exploratory way, by analysis of panel data from the United States and Britain, and by cohort analysis of United States elections from 1952 to 1976. I develop the following implications of the model: (1) the "life-cycle" process by which party identification increases with age may be largely a function of difficulty in meeting information costs; (2) the process by which party identification, once it exists, becomes stronger appears to differ from the process by which voters move from independence to identification; (3) class-consciousness, in the presence of class parties, may obviate the need for direct identification with parties; (4) the American electorate appears increasingly to be one in which political change may occur regularly, rather than through the fitful process of realignment.*

Party identification has been much studied since the key work of *The Voter Decides* and *The American Voter* (Campbell et al., 1954, 1960) but its causes have not been much studied. Researchers have described the nature of party identification, its development and its effects, but the fact that most Americans are attached to a party has seldom been taken as an anomaly to be explained. On the contrary, this has generally been treated as a natural situation, though when party identification began to decline in the late 1960s, the *decline* was treated as an anomaly. This acceptance of party identification as a natural thing is puzzling, since it flies in the face of the individualistic liberalism on which most American assumptions about politics are based.<sup>1</sup>

Perhaps one reason why party identification has been treated as a natural condition is that its sources have been thought to reside largely in childhood, a time when people are not yet much involved in politics. This has precluded most political explanations, and has suggested instead parental transmission as the major process by which people become partisans.

Research has concentrated on rates of intergenerational transmission, on the age at which party identification first appears, on which parent is the chief socializing agent, and so on. (For an excellent review of this literature, see Sears, 1975.) This body of research has done a great deal to clarify the circumstances surrounding children's acquisition of party identification, but it has not allowed us to address the basic question: Why do people identify with parties?

Though research has concentrated on children's acquisition of party identification, it is not necessarily true that the roots of adult party identification lie in children's party identification. Evidence on this question—pro or con—is simply lacking. We know that adults frequently identify with the party their parents identified with, but it is not clear whether this is because of direct transmission, or because the children share their parents' social situation and many of their basic values; either of these might eventually lead them to choose the party their parents had earlier chosen. Goldberg (1969) provides some evidence for the latter of the two processes.

Similarly, we know that children report party identification, and that their reports are fairly stable; and we know that adults report party identification, and that their reports are fairly stable. But no one has as yet been able satisfactorily to link childhood party identification with adult party identification—admittedly a massive problem of design and analysis. The best evidence on this question is that of Jennings and Niemi (1978), who followed a panel of adolescents aged 17–18 in 1965

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<sup>1</sup>Two exceptions to this general rule are Beck (1975) and Goldberg (1969), who treat political realignments and social position, respectively, as sources of party identification.

through to 1973, when they were 25 and 26. Along with this panel, they followed a panel of the respondents' parents. Thus, they were able to ascertain how thoroughly the party identification of people in what might be called their "late childhood" carried over into the early years of their adulthood. They found (1978, p. 349) that the relationship between adolescent party identification and young adult party identification, over the eight-year span, was  $T_b = .42$ . By way of comparison, the stability of the parents' party identification over the same time span was  $T_b = .67$ . Though some of the adolescents may have in fact been "adult" at 17 or 18 years,  $T_b$  of .42 is large enough to show a relationship between childhood party identification and adult party identification. However, it is also small enough, compared with  $T_b$  of .67 for the sample of adults, to suggest that much of the formation of adult party identification may occur in the adult years. Thus there is simply not a clear verdict on how much of adult party identification can be traced to childhood.

Further, recent research has found more systematic changes in party identification among adults than had previously been expected (Brody, 1977, 1978). This should encourage us to look to adult situations for explanations of adult party identification.

#### Adult Partisan Development

The most generally recognized model of adults' development of party identification is the so-called "life cycle" model which was first put forward in *The American Voter* (Campbell et al., 1960, pp. 161-65), and buttressed in Converse's elegant "Of Time and Partisan Stability" (1969). This model holds simply that through their adult years, people become less independent as they age and, if they already identify with a party, identify with increasing strength. This is generally explained, as in *The American Voter*, as a reinforcement process. There has been a running debate over whether the model actually describes what happens across the life cycle (Glenn, 1972; Glenn and Hefner, 1972; Abramson, 1976, 1979; Converse, 1976; Shively, 1979), but what is important for our purposes is that the model largely does not address the question which was set out in the opening paragraph, "Why should people identify with a party?" Though much of the original relationship in the American data is due to movements away from independence (Campbell et al., 1960, p. 162), and much of the later literature—especially that which is based on Gallup data—has addressed solely move-

ments between partisanship and independence, the model does not offer an explanation for such movement. Reinforcement may explain why people who already identify with a party should learn to identify with it more strongly, but it cannot explain why independents should choose to identify with a party in the first place (Claggett, 1979). How can what does not yet exist be reinforced?

How then may we address the question of why people identify with parties? A functional model of party identification, based on individuals' needs which are satisfied by party identification, would seem an appropriate vehicle. That party identification may fill needs for individuals has long been recognized. Among those who have recently studied voting, Downs (1975), Campbell et al. (1960), and Goldberg (1969) have particularly emphasized this. While Goldberg is concerned with the virtues of choosing one party rather than another, both Downs and the authors of *The American Voter* described a function which might account for the presence or absence of party identification, per se. This is the party's function of providing political cues to voters who feel themselves in need of guidance because they must make political decisions under confusing circumstances. (In the interest of brevity, I shall refer to this as the "decisional function.")

Though the existence of the decisional function is noted by these and later authors, it has been treated as an effect of party identification, rather than as a need which might draw people into party identification. (Discussion of the function in *The American Voter*, for instance, occurs in a chapter entitled "The Impact of Party Identification.") Treated as a need, the function may provide an explanation for adults' acquisition of party identification. Treating it in this way also leads us to specify how the function operates under varying circumstances; the function may thus provide the basis for a somewhat broadened model of the development of party identification among adults.

#### A Functional Model

Five variables seem likely to affect the production of party identification through the decisional function: (1) the strength of the feeling that one must participate in politics (I shall deal only with voting, but the same analysis could apply also to other forms of political participation); (2) the cost of information pertinent to the voting choice; (3) resources available to the voter to pay those costs; (4) concern for the quality of the decision; (5) the availability of other, more

efficient, decisional shortcuts. Let me expand on these:

(1) In order for the process to operate at all, individuals must feel some need to make voting decisions. Presumably in the eighteenth century most people would not have felt such a need. The spread of the franchise and of the expectation that it should generally be exercised must have spread this need through most of the populations with which we are concerned in studies of voting behavior. However, it is not necessarily true that this need is felt equally strongly by everyone (it obviously is not, since some people do not vote), or that it is equally widespread in all countries, or that it is equally widespread in any given country throughout that country's history. *The more strongly the need to participate is felt, ceteris paribus, the more the decisional function will tend to produce some guide, such as party identification.*

(2,3) To the extent that information pertinent to the choice which must be made is "expensive," voters will seek out efficient shortcuts to a decision, such as party identification. (While none of the variables presented here is entirely original to this paper, the notion that information costs will lead people to identify with parties comes quite directly from Downs, 1957.) "Expense" may be literal (the cost of a newspaper or television set) or non-monetary (the strain of dealing with conflicting bits of information, the difficulty of organizing and remembering political facts, and so on). Again, these costs are not constant either for individuals or for nations. The explosion of "news coverage" in the last few decades has surely made information cheaper. And at least one individual characteristic, the level of formal education, must increase the resources available to some people for gathering and organizing information, as compared with others. (2) *Ceteris paribus, the more "expensive" political information is, the more the decisional function will tend to produce a guide such as party identification; and (3) the lower the resources available to individuals to pay those costs, the more the decisional function will tend to produce such a guide.*

(4) The level of quality desired in the decisions must affect the individual's choice between party identification and some other way of deciding how to vote. To the extent that a person does not care a great deal whether the vote is cast wisely or not, low-information shortcuts such as party identification will be relatively attractive. At very low levels of concern, shortcuts with even less informational content than party identification might be

chosen; for instance, the voter might simply flip a coin. The degree of concern for the quality of decisions must vary to some extent among individuals and across space and time. Of available guides, party identification should generally involve a relatively low level of information; a "responsible party system" would presumably provide the major exception to this. *Ceteris paribus, the tendency of the decisional function to produce party identification will be related to the individual's concern for the quality of his or her decision. A moderately low level of concern—but not the very lowest—will be associated with the development of party identification.*

(5) Finally, let us consider the fifth variable affecting the production of partisanship by the decisional function. Conditions (1) through (4) have been concerned with whether individuals will feel the need to equip themselves to make voting decisions, and with whether they will choose to meet that need by becoming informed or by seeking a shortcut through the mass of political information. I have referred only to one kind of shortcut, party identification, but the arguments I have advanced so far could apply equally well to other shortcuts.

There are many modes of decision which could provide shortcuts. One which voters have often used is to pick some significant person—a spouse, for example, or union head, or local political boss—as a leader and vote as that person suggests. Another shortcut, which is possible if class-consciousness is high, is to vote according to the perceived interests of the class. In this case, class rather than the party which represents that class, serves as the organizing shortcut. (It seems likely, for instance, that the weak direct ties to political parties in postwar Europe have resulted from the prevalence of strong ties to a class or religion, which have obviated the need for direct ties to any party, per se.) *Ceteris paribus, if there is available to the voter some other shortcut to decisions which is closer to his or her preferred combination of information costs and decision-quality than identification with any of the parties, then the voter will not identify with a party. Instead, the voter will use the alternative shortcut.*

When a voter uses a shortcut other than party identification, he or she may still be associated with a party. While some shortcuts, such as selling the vote each time to the highest bidder, do not provide the individual with a lasting association with any particular party, most shortcuts do. Voting according to the interests of one's class, for instance, requires one to vote for the party which represents the class. For organizational reasons there is likely

to be considerable continuity in the relationship between political parties and classes; therefore, class voting will result in a prolonged association of voters with certain political parties.

Such association differs from party identification, however, in that there is no direct tie to the party. The party itself is not a guide to voting choice, but is rather an expression of that choice. Expressed partisanship, then, will be synonymous with the vote and parties as such will not serve as guides to organize behavior. Under the proper organizational circumstances, support for such parties may be quite volatile, if various parties compete to represent the same class (Shively, 1972, pp. 1220–25).

The five statements above comprise a model of the following sort: Statements No. 1 through No. 4 identify four conditions which, in interaction with each other, will lead individuals to seek out some sort of decisional guide. The four conditions operate in *interaction* with each other because, to the extent that one of them is weak or absent, the others will not operate as strongly to make people seek a decisional guide. For those quite unmotivated to vote, for instance, the magnitude of information costs associated with voting would not matter much. Or, if “perfect information” were available, no matter how motivated a person were to vote, there would be no need for him or her to develop an identification with a party.

The first three conditions (motivation, magnitude of information costs, and capacity to bear information costs) will be monotonically related to the propensity to identify with parties, all other things equal. The fourth condition, concern for quality of the decision, will bear a U-shaped relationship to the propensity to identify with parties. If one has little or no concern for the quality of the vote, there will be no need to develop any decisional guide. At a higher, but still low level of concern, one would presumably be likely to develop party identification as a decisional guide. At a still higher level of concern, one might be expected to develop some other sort of guide which yielded decisions of higher quality, or perhaps to eschew guides altogether.

The fifth condition (the availability of alternative decisional guides) stands rather apart from the first four, since it is not related to the need for some sort of decisional guide. Rather, if some guide is needed, it will affect the probability that party identification is the guide which develops.

### A Test of the Model

I shall present here only a partial test of the model, a test of statements 1 and 3: “The more the need to participate is felt by a voter, the more strongly the decisional function will operate to produce some guide such as party identification.” “The lower the resources available to the voter to pay information costs in voting, the more strongly the decisional function will operate to produce a guide such as party identification.” The major test will consist of analysis on the 1956–60 and 1972–76 Center for Political Studies panels; a complementary test will follow a cohort from 1952 to 1976, analyzing net changes in partisanship by education.<sup>2</sup> A test of statement 5 of the model appears below (pp. 1049–50).

For the 1956–60 and 1972–76 panels, I have calculated the conditional probability, for each category of respondent examined, that a nonpartisan in 1956 (or 1972) would have identified with a party in 1960 (or 1976). Similarly, I have calculated the probability that one who identified with a party in 1956 (or 1972) would have shifted to independence in 1960 (or 1976). The model predicts that for individuals with a low capacity to handle information costs and for highly motivated individuals—and especially for individuals who have both of these characteristics simultaneously—the probability of a shift into party identification should be high relative to a shift into independence. It is difficult to devise a sum-

<sup>2</sup>The data used here were made available by the Inter-University Consortium for Political and Social Research. The data were originally collected by the Survey Research Center Political Behavior Program, now the Center for Political Studies at the University of Michigan. Neither the original collectors of the data nor the Consortium bear any responsibility for the analyses or interpretations presented here. CPS uses the following questions to indicate if a respondent has acquired a party identification: “Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what?” If a party identification is indicated, a followup question asks: “Would you call yourself a strong \_\_\_\_\_ or a not very strong \_\_\_\_\_?” Independents are also asked if they “lean” to a particular party. For the present analysis I have defined “partisans” as all those who responded with a party to the first question. There is, of course, controversy about this, but I prefer to interpret the question as literally as possible, since this seems to me to be the most likely way that respondents will have interpreted it. For an argument that leaners may often simply be stating their intended vote when asked how they “lean,” and thus should be treated as genuine (if decisive) independents, see Shively, 1977, pp. 16–20.

mary measure for these two transition probabilities, but one measure is suggested by the fact that such a pair of probabilities is associated with a unique level of party identification which, if the probabilities remained constant over a series of transitions, would be reached and would be maintained at equilibrium (Coleman, 1964, pp. 103–12). For instance, from 1956 to 1960, 62 percent of independents who had had less than eight years of education shifted into party identification, while only 11 percent of party identifiers at that level of education shifted into independence. These two transition probabilities tend towards an equilibrium level of .85 as the proportion identifying with a party. The single number, .85, may thus summarize the joint tendency of the two transition probabilities. It is this number, the equilibrium point for the two transition probabilities, that I shall use as the dependent variable in the tests with panel data below.

Note, incidentally, that the two probabilities given in this example represent a very considerable systematic movement of adults between independence and party identification, considering that the shift occurred between just two elections. More than half of the independents moved to identify with parties, with only a small movement in the other direction. As we shall see below, systematic movement of this magnitude appears to be the rule for those groups of the population identified by the

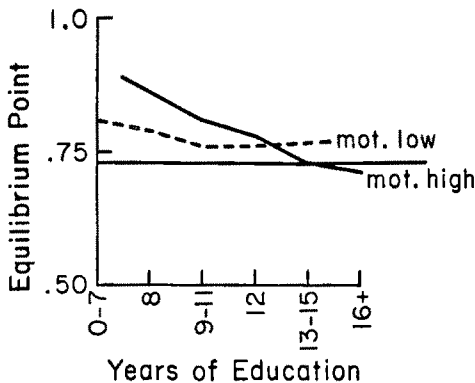
decisional function model as most in need of a decisional guide.

The two tests of the model are presented in Figure 1. Very rough measures of information costs and motivation are used to predict to the equilibrium point of party identification. Education is used as an index of individuals' capacity to bear information costs, and the "citizen duty scale" is used as an index of motivation to vote. (For a description of this scale see Rusk et al., 1968, Vol. 1, pp. 461–62). The prediction from the model is that equilibrium points should be positively related to the citizen duty scale and negatively related to education, with an interaction effect (higher expected equilibrium points) where low education coincides with higher scores on the citizen duty scale.

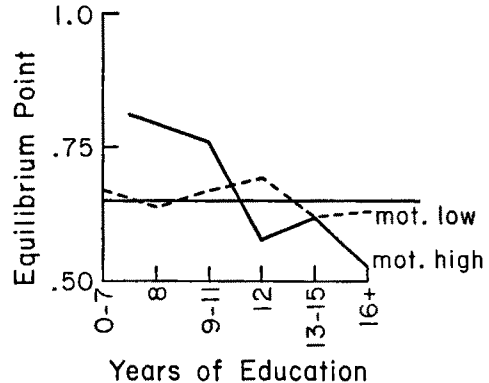
The results of the test are displayed in Figure 1. The prediction of the model with regard to education is borne out cleanly, as is the prediction of an interaction between education and motivation.<sup>3</sup> The weights of transition

<sup>3</sup>It might be objected that the effect is accomplished only by the small numbers of cases at the lower end of education. However, the electorate still includes substantial numbers of people with little education. Of the 1956–60 panel, 27 percent had had eight years of education or fewer; the comparable figure for the 1972–76 panel is 17 percent.

a) 1956–60:



b) 1972–76:



Source: 1956–60 and 1972–76 panel studies of the national electorate conducted by the Center for Political Studies, University of Michigan.

Note: Motivation is "high" if the score on the citizen duty scale is 4; for any lower score, motivation is "low." Due to small numbers of cases, 0–7 and 8 are merged for high motivation in both panels, and 13–15 and 16+ are merged for low motivation in the 1956–60 panel. For N's and conditional probabilities, see Appendix A.

Figure 1. Equilibrium Points for Moves between Identification and Independence, by Education and Motivation

into party identification relative to transition out of party identification, as summarized by the equilibrium point to which they tend, decline steadily with education among the highly motivated in both panels. Among the less motivated there is little if any relationship to education.

The prediction of the model with regard to motivation is *not* borne out, except through the interaction with education. At low levels of education, the more highly motivated tend towards markedly higher equilibrium points, but at high levels of education, the relationship is reversed. Overall, there does not appear to be any direct effect of motivation on the tendency to party identification.

For each panel, I have inserted a horizontal line indicating the actual proportion of the panel identifying with a party at the first of the two elections (.73 in 1956, .65 in 1972). For the 1956–60 panel, all but two categories of voters tended towards equilibria greater than the current level for the panel as a whole. This would be consistent with a general increase in party identification with age, as described in the “life-cycle model.” Note, however, that the extent of this “life-cycle” effect appears to be in part dependent on the terms of the decisional model; I will return to this point below. For the 1972–76 panel, though the general form of the relationships is about the same, everything has dropped lower; presumably this is a result of negative period effects. Now several categories fall below the current level of party identification.

While the differences shown in the figure do not look dramatic, this is partly due to the conservative nature of the summarizing measure. For instance, for highly motivated voters in the 1956–60 panel, the equilibrium point for those with eight years or less of education is .89, while the equilibrium point for college graduates is .71. This difference does not appear to be large, but it summarizes the fact that over the four years of the panel, of those with eight years of education, 62 percent of independents moved to party identification, while only 8 percent of party identifiers moved to independence—a difference of 54 percentage points. At the same time, of the college graduates, 29 percent of independents moved to party identification, while 12 percent of identifiers moved to independence—a difference of 17 percentage points. Thus in this case equilibrium points of .89 and .71 correspond to percentage differences of 54 and 17. Particularly considering that we are looking at a period of only four years, the contrast in the movements of these groups of voters is striking. A

full list of proportional movements is given in an appendix.

The fact that the test produces results of a similar form in the two panels adds to our confidence that the results are not accidental. More importantly, it suggests that the process set out in the functional model continues to operate in the 1970s much as it did in the 1950s, even though in the later period something has clearly been working generally to discourage moves into partisanship.

A complementary test adds further evidence of this. In Figure 2 the percent partisan for different educational levels of a large cohort is traced as that cohort aged from 1952 to 1976. The cohort consists of all white respondents aged 21–48 in 1952, aged 25–52 in 1956, and so on up to those aged 45–72 in 1976. I have included only whites because the special historical experience of blacks across this period might have led to changes in party identification which could have been confounded with those caused by the decisional function.

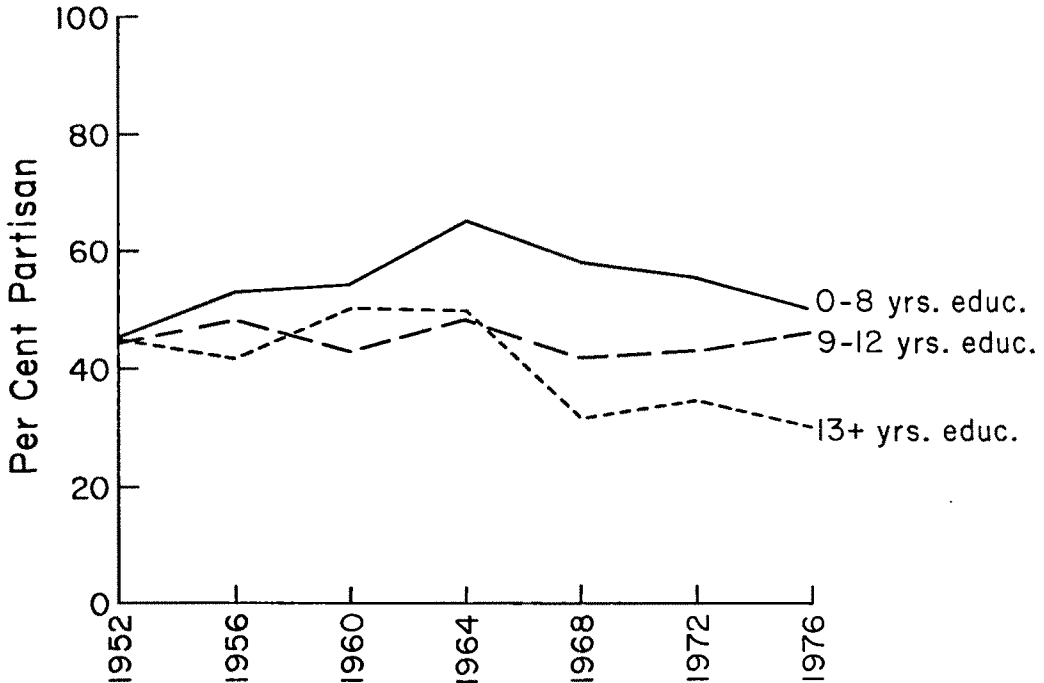
Though any individual point on the graph in Figure 2 is of only moderate reliability (*N*'s on which the percentage are based range from 162 to 600), there is a pattern of continuous differentiation by education, in the expected direction. In 1952, the three educational groups did not differ. As time passed and the respondents aged, the educational groups pulled apart fairly steadily, with the least educated groups becoming more partisan relative to the others.

The changed circumstances for party identification in the 1970s are evident in the graph. From 1952 to 1964 the groups generally increased in partisanship, while from 1964 to 1976 they generally decreased. But differentiation by education, which is presumably owing to the decisional function, continued during *both* the periods of increase and decrease.

This cohort analysis is only partially independent of the panel analyses; some of the 1956 and 1960 respondents were members of the 1956–1960 panel, and some of the 1972 and 1976 respondents were part of the 1972–1976 panel. However, it is a largely independent test, which suggests again that the decisional function is robust across quite different historical circumstances.

### Implications of the Model

If this partial test of the model at least establishes it as a plausible candidate for an explanation of why adults come to identify with parties, it is worth considering what



Source: 1952, 1956, 1960, 1964, 1968, 1972 and 1976 presidential election studies conducted by the Center for Political Studies, University of Michigan.

Figure 2. Development of Partisanship, 1952-1976, among a Cohort of Whites Aged 21-48 in 1952

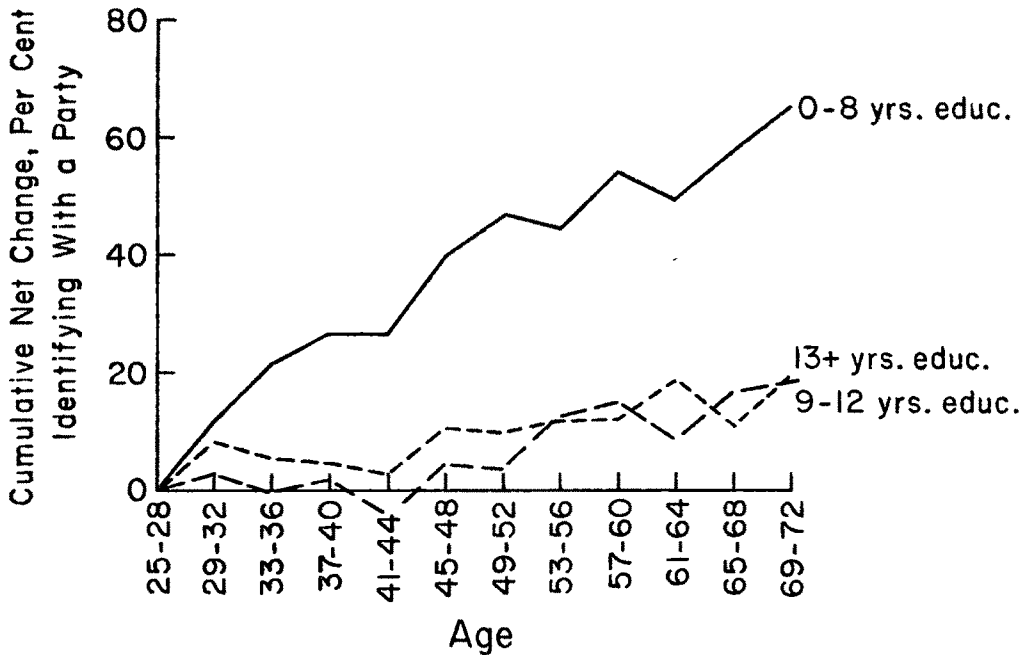
insights the model can provide into electoral politics. Let me present here some suggestions about the "life-cycle model," about the strengthening of party identification, about class-voting as an alternative to party identification, and about historical variation in electorates and the potential for realigning elections.

**The Life-Cycle Model.** The idea that as adults age they are increasingly likely to identify with parties, and to identify with them strongly, has been important in theories of voting behavior. Both Converse (1969) and Przeworski (1975), for example, have incorporated such a process as an important assumption in models of electoral development. While the strengthening of party identification when it is already present may be in part a function of selective reinforcement (a point to which I shall return), as I have argued, no explanation has been offered as to why adults should progressively identify with parties in greater numbers as they age. Our analysis of the decisional function suggests a set of conditions which could cause movements of this sort, and it further suggests

that such movements should not be uniform but should come differentially from among the most vulnerable portions of the population. This is clearly implied in Figure 2, but we can look at the process more systematically.

Cohort analysis of the electorate from 1952 to 1976 may allow us to examine the process, at least with regard to the prediction that life-cycle gains in the proportion of the population identifying with parties should come disproportionately from among the least educated.

Figure 3 presents results of a cohort analysis of the American electorate from 1952 to 1976, separated according to their education. This analysis proceeded as follows: each education level was divided at each election into four-year cohorts by age (25-28, 29-32, etc.). Change in the percent identifying with parties was measured for each cohort over each pair of elections from 1952 to 1976. (Thus, I measured change from age 25-28 to 29-32 from 1952 to 1956 by subtracting the percent of 25 to 28-year-old voters identifying with parties in 1952 from the percent of 29 to 32-year-old voters identifying with parties in 1956.) For each education level at each age, then, there are



*Source:* 1952, 1956, 1960, 1964, 1968, 1972 and 1976 presidential election studies conducted by the Center for Political Studies, University of Michigan.

*Note:* The life cycle starts at age 25 rather than at age 21, because of the problem in sampling voters in the 21-24 age group noted by Converse (1976, pp. 47-50). Blacks are excluded because of problems noted by Abramson (1979).

Figure 3. Development of Party Identification across the Life Cycle, by Education

six estimates of their change over four years of aging, one estimate for each contiguous pair of elections from 1952 to 1976.

One cannot simply average these six estimates to calculate the effect of aging, however, because in addition to whatever effects were associated with their aging, voters over the latter half of the period were also pulled in the direction of independence by historical forces. Actual change would be a combination of these historic forces ("period effects," in the terminology of cohort analysis) and of change associated with aging. This is, in other words, the frequently encountered identification problem in cohort analysis (Mason et al., 1973).

In order to separate effects associated with aging from period effects, I developed criterion groups for each education level, in order to measure pure period effects. These groups were constructed so as not to age over a given pair of elections, nor to change significantly through generational replacement. For each pair of elections, I compared the percent identifying with a party at the first election among voters who had entered the electorate in 1944, 1948,

1952 or 1956, with the percent identifying at the second election among voters who had entered the electorate in 1948, 1952, 1956 or 1960. These two groups were of approximately the same age; and, assuming that the 1944-1960 period was one of more or less constant impulse towards partisanship, they should not have differed much generationally among themselves. Thus, any change from the first group to the second group must be due almost solely to period effects occurring over the pair of elections. For the period 1964 to 1968, for those with 0-8 years of education, for example, the estimate of period effects is -.8 percent. For the cohort of 0-8 years education who were 37-40 years old in 1964, total change from 1964 to 1968 was -1.25 percent. Subtracting the estimate of period effects from this leaves an estimate of effects purely associated with aging of -.45 percent. I corrected each cohort change for estimated period effects in this way, and then averaged the six estimates for each age/education cohort. In the averaging, I weighted estimates by the number of cases on which they were based.



(For a fuller presentation of the correction for period effects and for justification of the required assumptions, see Shively, 1979.)

Thus this large base of data was finally distilled down to a single estimate of change associated with four years of aging, for each of eleven age groups at each level of education. The results are presented in Figure 3. Changes at different ages are arrayed cumulatively, so as to imitate a life-cycle. The results indicate that across 44 years of adult aging, the percent of persons with eight or fewer years of education identifying with parties could be expected to increase by about 65 percent. Those with more education might be expected to increase by a bit less than 20 percent. By implication, this suggests that that part of the life-cycle which involves acquisition of party identification (as distinct from its strengthening) is largely caused by the workings of the decisional function.<sup>4</sup>

**The Strengthening of Party Identification.** Party identification has generally been conceived as a simple dimension involving both the identification itself and the strength of one's feelings about the party with which one identifies. However, identification and its strength are potentially separate phenomena and should be treated separately, since it appears that they may develop by different processes (Van Wing-en and Valentine, 1978; Claggett, 1979). I have argued above, for example, that reinforcement might be involved in the strengthening of party identification over time, but that it could not be a factor in the original acquisition of party identification. The model based on the decisional function states only who should be most attracted to party identification; it does not necessarily state who, once identifying, will develop strong partisan feelings. Of course, acquisition and strengthening will be related, as those who had been most in need of partisanship would presumably be relatively more pleased by it, and should tend to develop "strong partisanship" out of that pleasure. But political parties are not neutral vessels for individuals' psychological needs. Parties make policies, and present themselves symbolically,

in ways which may or may not be pleasing to their followers. One person might have been led into identifying with a party because of a felt need for guidance, and yet never find that party so pleasing that he or she developed enthusiastic loyalty to it. Another, even one with less initial need for the party, might be greatly pleased by its policies and by its presentation of itself, and become an enthusiastic partisan. In other words, the depth of one's original need for a party guide should affect the pleasure taken in that party and the strengthening of one's identification with it, but many other things will do so as well. The decisional function should lead to the strengthening of party identification, but other factors will also be involved.

It may be interesting to look at the strengthening of party identification among different educational groups in this light. To the extent that a party presents itself as a party of the lower social classes, its class nature should reinforce the operation of the decisional function. The least educated should strengthen in partisanship both because of the class benefits (tangible and symbolic) that they receive from a party, and by virtue of the fact that they find party identification, *per se*, especially satisfying. An upper-class party, however, should show a mix of two contradictory processes. From its class nature we should expect a positive relationship between education and strengthening, while from the decisional function we should expect a negative relationship. Depending on the relative force of the two processes, the resulting curve might be positive, U-shaped, or (relatively weakly) negative.

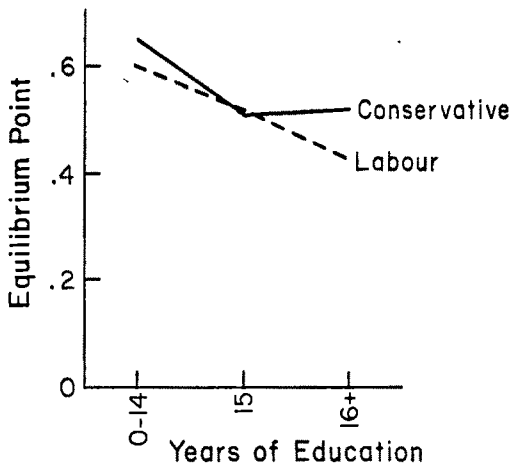
Figure 4 presents the relationship between education and the equilibrium points for probabilities of moves between "not very strong" or "fairly strong" partisanship on the one hand, and "very strong" partisanship on the other, based on the 1964-1966 and 1966-1970 British Butler-Stokes panels.<sup>5</sup> Equilibrium points are calculated here as they were above for the probabilities of moves from identification into independence and from independence into identification; only those respondents are examined who identified with the same party at both stages of the panel.

The results are as one would expect, given the strong class biases of the two parties. From

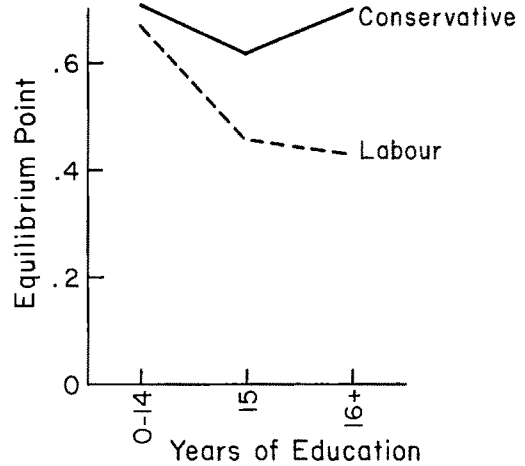
<sup>4</sup>For any who may be uncomfortable with the correction for period effects, a similar analysis done on the "steady-state" period of 1952-1964, with no correction for period effects, gives similar results. The evidence of those elections indicates that persons of 0-8 years of education would show a net gain of 49 percent over 44 adult years. Similar figures for those of 9-12 years and more than 12 years of education, respectively, are 15 and 8 percent.

<sup>5</sup>The data were made available by the Inter-University Consortium for Political and Social Research. They were originally collected by David Butler and Donald Stokes.

a) 1964-66:



b) 1966-70:



Source: 1964-66-70 panel study of the British electorate conducted by David Butler and Donald Stokes.

Note: For N's and conditional probabilities, see Appendix B.

Figure 4. Equilibrium Points for Shifts between Weak and Strong Identification, Britain

1964 to 1966, the curve for the Conservatives (for whom class interest and the decisional function pull in opposite directions) is U-shaped, dipping to the right, while from 1966 to 1970 it is U-shaped and fairly symmetric. For the Labour party class interest and the decisional function pull in the same direction, and the curve for Labour is negative across both panels.

One interesting question regards the class appeal of American parties. It is not clear what we should expect here, and an examination of strengthening in the context of the decisional function may help us assess the overall class appeal of the two parties.

Figure 5 presents analyses of strengthening for American parties parallel to the analysis for British parties presented in Figure 4. Instead of looking at equilibrium points for the probabilities of moving into and out of identification, as in Figure 1, we here examine the equilibrium points for the probabilities of shifting between "strong" and "not very strong" identification. From this figure it appears that *both* American parties are basically upper-class parties.<sup>6</sup> The

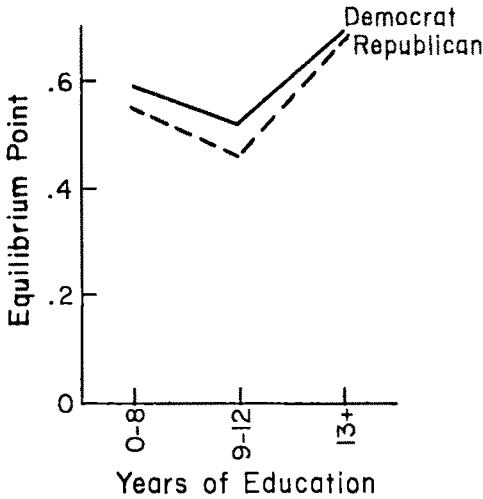
curves for both parties across both panels are either positive or U-shaped. It is also apparent that the upper-class bias of the Republicans is stronger than that of the Democrats, since across both panels the Republicans tip more to the highly educated than do the Democrats (the difference is slight for the 1956-1960 panel, considerable for the 1972-1976 panel). The dominant impression, however, is of a system of two upper-class parties.

Incidentally, this result may help to explain a minor puzzle in studies of American party identification, the fact that Republicans have been found in almost every study to identify more strongly with their party than Democrats, and to be more loyal to it in their voting.<sup>7</sup> An examination of Figure 4 suggests that this may be owing to the fact that Republican supporters are concentrated in the most highly educated group, the group most likely to strengthen in

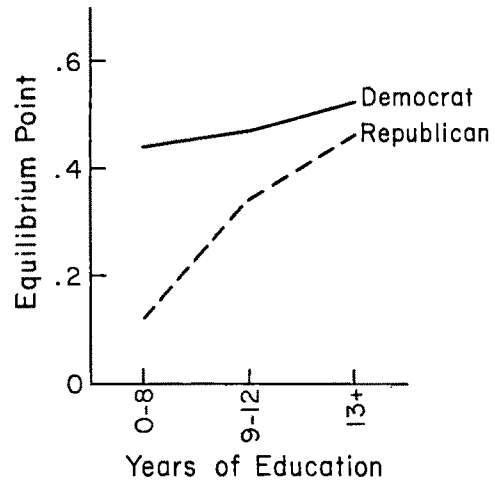
<sup>6</sup>To be precise, both American parties appear to be happier lodgings for the well educated than for the less educated. This could be given interpretations other than the class interest one I prefer.

<sup>7</sup>Over the first six CPS presidential election studies, on the average 47.1 percent of Republicans have been strong identifiers, compared with 45.5 percent for the Democrats, even though the latter were much the more popular party. Over this period, on the average 85.5 percent of the strong Democrats have voted Democratic, while 96.3 percent of strong Republican identifiers voted Republican (figures calculated from Asher, 1976).

a) 1956-60:



b) 1972-76:



Source: 1956-60 and 1972-76 panel studies of the national electorate conducted by the Center for Political Studies, University of Michigan.

Note: For N's and conditional probabilities, see Appendix B.

Figure 5. Equilibrium Points for Shifts between Weak and Strong Identification, U.S.A.

their attachment to either party. Of consistent partisans from 1956 to 1960, 42 percent of Republicans had more than 12 years of education, compared with 24 percent of Democrats. The comparable figures for 1972-76 are: Republicans, 54 percent; Democrats, 34 percent.

As a result, even though both panels cover periods of Republican decline, Republican partisans did not weaken in their commitment to the party as much as they might have been expected to do if the Republican party were of the same social composition as the Democratic party. Of consistent Republican partisans, there was a small net gain in percent strong from 1956 to 1960 of +.4 percent. If Republicans had had the same educational distribution as Democrats, however, they would have sustained a small net loss of -.2 percent. From 1972 to 1976, there was a sharp net loss in percent strong of -8.3 percent among consistent Republicans. If Republicans had had the same educational distribution as the Democrats, the loss would have been -11.8 percent. These are small differences, but they appear large enough to account for the small systematic advantage the Republicans have enjoyed in this regard.

**Class-Voting as an Alternative to Partisanship.** The fifth statement of the model based on the decisional function has a status rather different

from the first four. Statements No. 1 through No. 4 set out the conditions under which individuals will need a guide to making electoral decisions. Statement No. 5 points out that where other, more efficient, guides are available, individuals will not become partisans. Such guides might include voting as one's spouse or a local political leader does, flipping a coin or whatever. One particularly interesting guide is class-consciousness. If a voter is a conscious member of a class, and if that class is represented by a particular political party, then there may be no need for the voter to develop any identification with a party per se; such voters can simply base their electoral choice on their class.

This is a relevant consideration, because it may help to explain the puzzle of European "party identification." After a flurry of enthusiasm for party identification following its successful use by Americans in the 1950s, Europeans have generally concluded that it does not help much in understanding voting behavior. It is rather unstable across time among European populations; and responses to the identification question more often seem to reflect a current voting intention than a long-standing commitment to the party. There is a suspicion that party identification in its American sense does not exist among Europeans

(Thomassen, 1975; Budge et al., 1976).

The possibility that class-voting obviates the need for a decisional guide among Europeans can provide a solution to this puzzle. In every European country there are clear class parties available to class-conscious voters; religious and other socially based parties are similarly available to other groups. Certainly, European elections and party systems have been primarily interpreted in these terms. Under these circumstances, it may not be surprising, in view of the decisional model presented here, that identification with parties per se has failed to develop among Europeans.

However, Europe may be moving into an era in which the conditions which make party identification superfluous will decline. Some party systems (notably those of Germany, France and Italy) appear to be condensing into systems with a small number of broadly based parties, which cannot make the same clear social or class appeals that parties did in the past. New issues have arisen, such as the issues surrounding European integration, which do not involve simple class or social appeals. And the social and class structure of European populations may become less simple as Europeans become more mobile. For all these reasons, the decisional function model would lead us to predict that substantial numbers of Europeans should now begin to develop party identification (Shively, 1972). In a study addressing this question, Baker (1978) provides a test of whether party identification functions more among younger generations of Germans than among older generations. He concludes that it does.

Thus, the model of the decisional function leads to an interesting explanation of the difference between American and European partisanship, with a prediction of convergence between the two. It is difficult to design tests for this interpretation, but one partial test may at least add plausibility to the suggested interpretation of European partisanship.

We have already looked at the strengthening of British partisanship. It is possible to separate out a portion of the British electorate which is relatively more "class-conscious," and compare the strengthening of partisanship among class-conscious voters to strengthening among those who are less class-conscious. The model of the decisional function predicts that for each party the relationship between education and the strengthening of party identification should be more negative among the less class-conscious than among the class-conscious. This is so because, for the class-conscious, the decisional function should be irrelevant to changes in the

strength of their expressed partisanship; for the less class-conscious, however, a negative relationship between education and the strengthening of party identification, the result of that function, should be added to whatever other relationship is present.

The results of such a test are presented in Figure 6. The class-conscious have been selected by pulling together those voters who simultaneously (1) placed themselves in one of the social classes, and (2) chose conflict when asked, "Do you think there is bound to be some conflict between different social classes, or do you think they can get along without conflict?" All other voters were classified as "less class-conscious." By this rough measure, one-fourth of consistent Labour and Conservative partisans are class-conscious. Because of diminishing N's under this further control, I have merged the 1964-1966 and 1966-1970 panel results, and I have dichotomized education into "left school at 14 or younger," "left school at 15 or older." Even at this rate, some N's are a bit small. (See Appendix B for a report of the N's.)

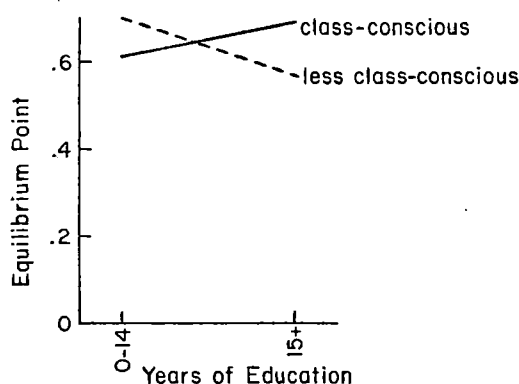
We see in Figure 6 that the prediction from the model of the decisional function is borne out. For both parties, the relationship is more negative among the less class-conscious than among the more class-conscious. In the case of the Labour party, the prediction was certainly counter-intuitive, that the strengthening of Labour support should be more dependent on class for the less class-conscious voters!

**Historical Variation in Electorates, and the Potential for Realignment.** Since conditions associated with the decisional function have changed considerably over the last few decades in the United States, what does the model of the function imply for changing patterns of partisanship in the country? A large systematic change has occurred in levels of education, presumably implying a greater capacity to handle information. In 1900 about 70 percent of new voters were of grade-school education or less; in 1930 the comparable figure was 45 percent, and in 1970 7 percent.<sup>8</sup> Along with this change, one would guess that information costs have declined. Thus there should be a reduced need for guides such as party identification among the electorate.

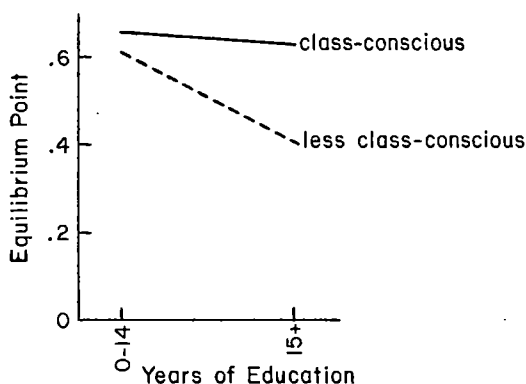
These changes cannot have produced the large drop in partisan propensities among American voters from 1966 on, which show up

<sup>8</sup>1970 United States Census. Figures for 1900 and 1930 are inferred from data in the 1940 census.

a) Conservatives:



b) Labour:



Source: 1964-66-70 panel study of the British electorate conducted by David Butler and Donald Stokes.

Note: For N's and conditional probabilities, see Appendix B.

Figure 6. Equilibrium Points for Shifts between Weak and Strong Identification, by Class-Consciousness

in Figure 2 as a fairly general decline among all groups. That drop is too sudden to have been produced by gradual demographic processes.

However, the model of the decisional function does have implications for what we might expect as a resolution of the drop in partisanship. Currently, the electorate looks ripe for realignment, according to most scenarios. Voters' sense that the electoral system is relevant and important to them has dropped, and their attachment to the present parties has dropped. In order for realignment to occur, presumably the parties should shift their positions in a way which would make them more appropriate to the political needs of the electorate; consequently, the electorate should respond by being brought back into enthusiastic participation, and then cement the new alignment by becoming "identified" with the redefined parties.

Now, according to the model presented here, different electorates will vary in how well they play out their part in this process, according to how much their members need party identification as a guide in deciding how to vote. For example, if we take the transition probabilities of the 1956-1960 panel as typical, the difference between the educational makeup of new voters in 1900 and new voters in 1970 implies that among the former under conditions of high motivation, if half the new voters were to enter as partisans, we could expect a net increase of 16 percent in the percent partisan after their first election. The comparable figure

for new voters in 1970 is 9 percent. This is a hypothetical situation, but it illustrates the fact that parties have now apparently lost a good deal of their capacity to cement themselves onto the electorate through party identification.

From the model of the decisional function we would predict that an electorate like that of the 1970s, while it could call forth a redefinition of the parties and could respond to that redefinition by increased enthusiasm for elections, would *not* respond as much as past electorates have done by locking the redefined system into a pattern of party identifications. This should be a healthy change. It does not mean that political change would not happen; it means only that that change would occur more fluidly than it could do through realignments.

### Conclusion

I have presented here a functional model of partisanship which yields an explanation for the neglected question, "Why do people identify with parties?" It certainly does not provide a full explanation of partisanship. Childhood acquisition of identification is not included in its purview, and there is much variation among adults in both panels examined, which is not accounted for by the model. However, even given the crude measures I have used, this model has uncovered substantial systematic adult change in the panels. The process implied in the model appears to be historically robust

over the changed circumstances of the 1950s and 1970s. And it yields interesting implications for the political life-cycle, for the relationship between class and party, and for differences between European and American partisanship. I hope the model may be taken as a first step in seeking the underlying processes by which voters define their political identity.

#### Appendix A. Probabilities on Which Figure 1 Is Based

Listed here are  $P(ID_2 | Ind_1)$ , the conditional probability of a move from independence into identification; and  $P(Ind_2 | ID_1)$ , the conditional probability of a move from identification into independence. N's are in parentheses.

Years of Education: 1956-60	Motivation High		Motivation Low	
	$P(ID_2   Ind_1)$	$P(Ind_2   ID_1)$	$P(ID_2   Ind_1)$	$P(Ind_2   ID_1)$
0-7	.62 (21)	.08 (80)	.50 (38)	.12 (97)
8			.46 (26)	.12 (83)
9-11	.48 (33)	.11 (83)	.41 (37)	.13 (92)
12	.39 (41)	.11 (105)	.53 (32)	.17 (77)
13-15	.36 (42)	.13 (119)		
16+	.29 (14)	.12 (49)	.40 (38)	.12 (95)
1972-76				
0-7			.32 (19)	.16 (45)
8	.53 (17)	.13 (63)	.27 (11)	.15 (55)
9-11	.55 (20)	.17 (53)	.55 (42)	.27 (85)
12	.26 (46)	.19 (83)	.31 (55)	.14 (84)
13-15	.28 (71)	.17 (129)	.33 (84)	.20 (79)
16+	.19 (42)	.17 (75)	.25 (32)	.15 (59)

Source: 1956-60 and 1972-76 panel studies of the national electorate conducted by the Center for Political Studies, University of Michigan.

#### Appendix B. Probabilities on Which Figures 4, 5, and 6 Are Based

Listed here are  $P(S_2 | W_1)$ , the conditional probability of a move from weak identification

into strong identification; and  $P(W_2 | S_1)$ , the conditional probability of a move from strong identification into weak identification. N's are in parentheses.

	Britain			
	Conservatives		Labour	
	$P(S_2   W_1)$	$P(W_2   S_1)$	$P(S_2   W_1)$	$P(W_2   S_1)$
1964-66				
0-14 yrs. ed.	.37 (98)	.20 (122)	.37 (183)	.25 (199)
15	.34 (61)	.33 (40)	.26 (62)	.24 (50)
16+	.32 (59)	.29 (82)	.33 (24)	.44 (18)
1966-70				
0-14 yrs. ed.	.38 (77)	.15 (83)	.41 (120)	.21 (165)
15	.43 (42)	.27 (30)	.31 (71)	.37 (54)
16+	.44 (62)	.18 (65)	.40 (20)	.53 (19)
1964-66, 1966-70 merged				
Class-conscious				
0-14 yrs. ed.	.35 (37)	.23 (40)	.32 (79)	.17 (121)
15+	.40 (51)	.18 (43)	.33 (51)	.20 (41)
Less class-conscious				
0-14 yrs. ed.	.38 (138)	.17 (175)	.41 (224)	.26 (243)
15+	.38 (181)	.29 (166)	.29 (126)	.42 (100)

(continued on next page)

(continued)

	U.S.A.			
	Republicans		Democrats	
	$P(S_2   W_1)$	$P(W_2   S_1)$	$P(S_2   W_1)$	$P(W_2   S_1)$
1956-60				
0-8 yrs. ed.	.29 (31)	.23 (43)	.39 (57)	.27 (78)
9-12	.35 (46)	.41 (42)	.35 (105)	.32 (105)
13+	.35 (43)	.17 (69)	.40 (55)	.18 (56)
1972-76				
0-8	.06 (18)	.42 (19)	.20 (46)	.26 (47)
9-12	.24 (34)	.46 (46)	.34 (95)	.39 (57)
13+	.29 (72)	.34 (68)	.24 (76)	.22 (54)

Source: 1964-66-70 panel study of the British electorate conducted by David Butler and Donald Stokes; 1956-60 and 1972-76 panel studies of the American electorate conducted by the Center for Political Studies, University of Michigan.

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# A Dynamic Simultaneous Equation Model of Electoral Choice

GREGORY B. MARKUS  
PHILIP E. CONVERSE  
*University of Michigan*

*This article develops a simultaneous equation model of the voting decision in a form thought to mirror the main lines of cognitive decision-making processes of individual voters. The model goes beyond earlier efforts in two respects. First, it explicitly represents the causal interdependence of voter assessments in the election situation, permitting such estimations as the degree to which correlations between voter issue positions and issue positions ascribed to preferred candidates arise because of projection onto the candidate or persuasion by the candidate. Secondly, the model is truly dynamic, in the sense that it is dependent on longitudinal data for its proper estimation. The utility of the model is certified by the goodness of fit achieved when applied to 1972-76 panel data for a sample of the national electorate.*

There is no shortage of studies which focus upon the roles of candidate personalities, partisan leanings, and contemporary issues in affecting the outcomes of elections in the United States. The value of this research is obvious, but it is probably fair to say that too much attention has been paid to the relative importance of factors idiosyncratic to particular elections and not enough effort has been directed toward the development and evaluation of an integrated and generalizable behavioral model of the voter's calculus. The result has been an uncomfortable lack of fit between verbal theories of micro-level electoral dynamics and statistical models of that process.

The goal of this study is *not* simply predictive accuracy, nor the assessment of the "relative importance" of various predictors of the vote. Nor do we intend for this work to be interpreted narrowly as a study of a particular election. What follows is a self-conscious effort at developing a model which is verisimilar to the dynamic cognitive process underlying citizens' electoral decision making. To the extent that we are able to synthesize a scattered set of verbal hypotheses and pre-theories into a precise and testable quantitative structure, we shall consider the effort a success even before confronting a shred of empirical information. Should the data corroborate the hypothesized structure or provide insights into how it might be modified—so much the better.

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The model to be presented and evaluated here has two major advantages over previous work: it explicitly embodies the simultaneous interdependence of perceptions and evaluations of political stimuli specific to a particular election; and it is a truly dynamic model in that it depicts how the campaign and the ultimate vote choice modify or reinforce prior orientations. The utility of the model will be assessed by examining its goodness of fit to the 1972-1976 panel data gathered by the Center for Political Studies.

## Background

**Predicting the Vote.** A formal model of the voter's calculus which is appealing on both normative and empirical grounds is derived from the idea of minimizing subjective expected loss (Shapiro, 1969; Davis et al., 1970; Riker and Ordeshook, 1973). The loss function is typically assumed to be quadratic, so that the loss associated with candidate  $j$  by an individual voter may be defined as:

$$L_j = (X - \theta_j)' A (X - \theta_j) \quad (1)$$

where  $X$  is an  $m \times 1$  vector of the voter's preferred positions on evaluation dimensions 1, 2, ...,  $m$ ;  $\theta_j$  is an  $m \times 1$  vector of the  $j$ th candidate's positions on these evaluation dimensions, as perceived by the voter;  $A$  is an  $m \times m$  matrix of weights reflecting the relative importance of the dimensions and their covariation.

Once the expected loss associated with each candidate is assessed, the model assumes that the citizen will vote for the candidate with

the smallest expected loss, i.e., he or she will vote "rationally." It is worth noting that, from a strictly axiomatic point of view, a choice on the basis of a candidate's personal characteristics is no less rational than one founded on a careful assessment of stated policy positions. Rationality refers to the notion of minimizing expected loss, and the model is utterly indifferent to the criteria employed by the voter in calculating these losses.

Empirically, a behavioral version of the decision rule has been shown to predict individual electoral choice very well, particularly when party identification is used as a tie-breaker in instances wherein the evaluations of the opposing candidates are similar (Kelley and Mirer, 1974; Brody and Page, 1973). Despite the predictive success of this approach, its major drawback is that it says little about the origins of candidate evaluations and thus provides a limited understanding of the broader cognitive process by which an individual arrives at a vote choice. As Brody and Page (1973, p. 10) have pointed out: "Common sense says that people probably vote for the candidate they like best." What remains is to "shift the analytical task away from an explanation of the vote to an explanation of attitudes toward candidates" (Brody and Page, 1973, p. 16). To accomplish this, the decision rule must be embedded within the context of a comprehensive model of electoral choice.

**Models of Electoral Choice.** In 1966, Goldberg formulated a causal system in which party identification was posited to influence candidate choice both directly and via its impact upon evaluations of parties and issues. This first attempt at modeling voting behavior represented a significant advance over earlier work, but it was flawed by methodological errors and, more importantly, by its lack of correspondence to a well-defined theory of the calculus by which an individual arrives at a candidate preference. Goldberg's causal system was recursive in structure, ruling out feedback from "partisan attitudes" to party identification, but he was nevertheless sensitive to the need to develop more realistic models which allowed for the dynamic interdependence of political orientations.

A major step in this direction was taken by Jackson (1975) when he presented a model wherein a citizen's policy preferences, partisan attachments, and evaluations of parties' positions were simultaneously related to each other and were, therefore, "endogenous to the electoral process." Testing this model against data from the 1964 Survey Research Center Election

Study, Jackson found a triangular causal system operating across the three factors, with party identification influencing the voter's own issue positions, those policy preferences affecting evaluations of the parties and candidates, and they, in turn, impinging upon party identification. The vote itself was the result of an interaction between partisan identification and the evaluations of current political stimuli.

Jackson's work went far toward the development of a realistic and comprehensive model of electoral choice, but it was necessarily limited by the data sources available at the time. For instance, studies have shown that citizens' perceptions of candidate issue stands are correlated with their own policy preferences (Page and Brody, 1972), but neither Jackson's nor any other research to date has succeeded in untangling the causal nexus underlying this relationship. Is it because voters project their preferences onto the candidates, or alter their own positions in response to where they perceive the candidates to stand, or because of some other process? This question has confronted students of voting behavior since the earliest survey investigations (e.g., Berelson et al., 1954), but without suitable measures of the relevant variables—and a properly specified model—any answer has had to be largely speculative.

A second and related point is that models of electoral choice have thus far been almost exclusively static (Jackson, 1975; Schulman and Pomper, 1975; Declercq et al., 1975; Achen, 1976; Page and Jones, 1978). That is, even when cast in simultaneous-equation form, they have been obliged to focus solely on the relationships among determinants of the vote at a single point in time, leaving unanalyzed the possible ways in which prior orientations and behavior shape reactions to the stimuli of a new election, as well as the modifications produced in those orientations in turn by the new election circumstances. Plainly, the unavailability of sufficiently rich longitudinal data was a major factor in restricting models to static specifications.

Freed from many of the earlier data limitations, this study builds on the work of Jackson and others but offers some significant advances. First, the model incorporates recent thinking about the social psychological processes involved in policy-oriented voting. Second, the model is dynamic, explicitly taking into account not only the simultaneous interplay of political attitudes within the context of a single presidential campaign but also the longitudinal dependence of these attitudes from one election to the next. Among other advantages, a

model in this longitudinal form can be given more satisfying specification than one tailored to synchronic measurement. This is so because useful exogenous variables are at hand in states and behaviors actually measured at earlier periods, rather than dredged up by recall. Thus we can say with considerable certainty that evaluations of Carter and Ford as presidential candidates in 1976 cannot have exerted causal influence on expressions of party identification or issue positions in 1972, and hence the latter are suitably exogenous to the nexus of attitudes in 1976. When all measurements are synchronic, it can of course be claimed that certain variables are likely to be exogenous, simply as a tactic to permit the identification of a non-recursive model. However, such arguments must always remain in the final analysis more or less suspect.<sup>1</sup> Our longitudinal data base substantially liberates us from this difficulty.

### An Overview of the Model

The preeminence of the trilogy of party affiliation, issue orientations, and candidate personalities as determinants of electoral choice is firmly established in the literature, and the model to be developed here is generally in keeping with this perspective. As Figure 1 illustrates, however, we maintain that these factors are not linked directly to the vote.<sup>2</sup> Instead, their confluence yields a set of overall candidate evaluations, on the basis of which a choice is made. More specifically, the model posits that the citizen compares his or her summary evaluations of the candidates and votes for the one most preferred, provided the

preference differential is reasonably large. However, the smaller the amount by which the voters prefers one candidate over others, the greater the influence of party loyalty in determining the final choice. For a two-candidate race at time  $t$ , the equation linking the probability of a vote for candidate 1 to candidate evaluations and party affiliation is:

$$\text{Cand.}_1 \text{ Vote}_t = a + b_1(\text{Eval.}_{1t} - \text{Eval.}_{2t}) + (b_2 - b_3 | \text{Eval.}_1 - \text{Eval.}_{2t} |) ID_t + e_t. \quad (2)$$

The first explanatory variable in Equation (2) is simply the difference in candidate evaluations, while the second term involves the absolute value of this difference and reflects the hypothesis that the impact of party ties on the individual electoral decision depends on the degree to which one candidate is preferred over the other. Note that when the evaluation differential is small, the amount by which the party identification coefficient is diminished is also small. On the other hand, if one candidate is preferred to the other by a sufficiently large margin, the coefficient for the impact of partisanship may be reduced close to zero. This aspect of the model is consonant with the spirit of the "Decision Rule" devised by Kelley and Mirer (1974) as well as with normative models of the voter's calculus. Its mathematical form is quite different, however.

It should be noted parenthetically that the model is addressed to the topic of choice among candidates and does not deal directly with the question of who votes and who does not. Implicit in this delimitation is the argument that these two concerns may indeed be fairly distinct from one another and, hence, separable for analytical purposes. This argument is made on the grounds that the decision to vote or not in a given election is determined for the most part by fairly stable attitudes toward the act of voting itself and is only secondarily affected by election-specific variables (candidates, issues, etc.). The stream of literature beginning with *The American Voter* and culminating most recently in the works of Riker and Ordeshook (1968) and Ferejohn and Fiorina (1975) supports this contention. As Campbell et al. (1960, p. 93) first put it, and as it has been demonstrated repeatedly since then, "Inquiry into the determinants of voting turnout is less a search for psychological forces that determine a decision made anew in each campaign than it is a search for the attitude correlates of voting and non-voting from which

<sup>1</sup>A common tactic with models tailored to static data is to depend on recall of earlier states for exogenous variables. Thus, for example, recall of parental partisanship may be used as an exogenous variable on grounds that current political attitudes cannot act backward causally in time to affect parental partisanship in an earlier period. When no other longitudinal information is available, such an assumption is better than nothing. However, this assumption ignores the possibility that *current* recall of earlier parental partisanship can itself be contaminated by currently evolving political attitudes.

<sup>2</sup>In Figure 1 we are adopting the convention that an arrow leading to another arrow denotes an interaction or mediating effect. In addition, when the simultaneous equation form of Figure 1 is presented, the coefficients and error term in each equation will be subscripted only with respect to that equation (i.e., the constant term for each equation is simply  $a$ , the error term is  $e_t$ , etc.). The purpose is to keep the notation simple, and it should cause no confusion to the reader.

these modes of behavior have emerged and by which they are presently supported."

**Issue Orientations.** The difficulty with assessing the impact of issues on the voting decision is that perceptions of candidate stands on issues of the day may vary significantly from voter to voter. Some of this variation will be unsystematic, arising from the casual attention most voters pay to campaign information—or arising from the ambiguous nature of the information itself. Against this background of noise, however, how voters perceive candidate policy positions may depend systematically upon their own issue preferences and overall affective orientations toward the candidates. For instance, voters may "project" their own issue stands onto candidates they like on other grounds, and by the same token they may tend to cognize the policy stances of negatively evaluated candidates in such a way as to increase the issue-related distance between themselves and these candidates (Page and Brody, 1972; Brody and Page, 1972).

In addition to the projection hypothesis, there is also the possibility of "persuasion." If a citizen were to alter his or her issue positions to coincide with those of a favorably regarded candidate, then one might say that the voter had been persuaded by the candidate (Brody and Page, 1972). A form of reverse persuasion would occur if a voter changed his or her position so as to contrast it with that of a negatively evaluated contender for office.

Lastly, the summary evaluations of the candidates are likely to be simultaneously interdependent with an individual's own policy preferences and perceptions of candidate positions. Specifically, the hypothesis is that, other things equal, the candidate thought to be most proximate to one's own position in the issue space will be favored.

The projection hypothesis is modeled as follows:

Perceived Cand. Stand.<sub>t</sub> = Actual Cand.

Stand.<sub>t</sub> +  $b_1(R's\ Stand_{t-1} - Actual\ Cand.$

Stand.<sub>t</sub>)Eval.<sub>t</sub> +  $e_t$ . (3)

The equation implies that a voter will cognize a candidate's policy position in such a way as to decrease the issue distance between the voter and a positively evaluated candidate or to increase the distance from a negatively evaluated candidate. Since the voter may enter the campaign period with a set of policy preferences, the respondent's issue self-placements at  $t-1$  are incorporated into the equation.

The operationalization of the candidates' "actual" issue stands is problematic. One approach would involve content analysis of campaign speeches (Page and Brody, 1972). Or one might rely on the judgments of a panel of electoral experts. The method employed here is not so ambitious; we shall simply use the sample mean location of each candidate. This procedure is, of course, not without its shortcomings, and it may well be that these average placements do not correspond exactly to the candidates' "true" locations in the issue space. The mean placements nevertheless permit us to ascertain how an individual's perceived locations of the rivals for office differ from where the candidates were seen by the electorate as a whole.

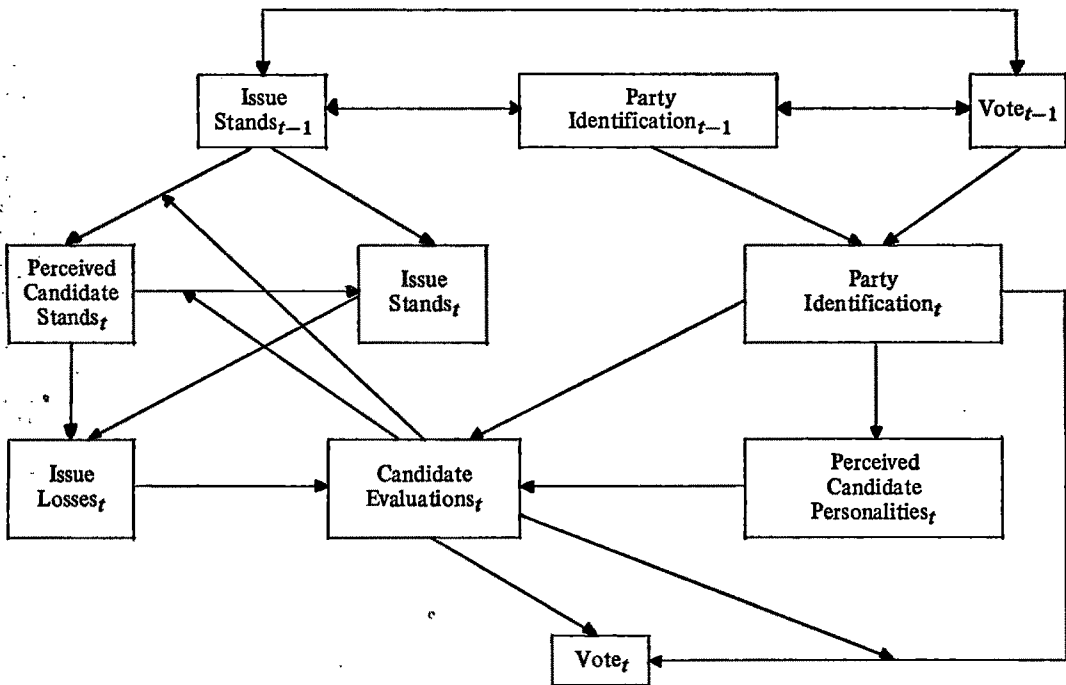
The persuasion hypothesis is modeled for a two-candidate race according to Equation (4):

$$\begin{aligned} R's\ Stand_t = & a + b_1 R's\ Stand_{t-1} + \\ & b_2(Perceived\ Cand._1\ Stand_t - \\ & R's\ Stand_{t-1})Eval_{1t} + \\ & b_3(Perceived\ Cand._2\ Stand_t - \\ & R's\ Stand_{t-1})Eval_{2t} + e_t. \end{aligned} \quad (4)$$

This equation represents the idea that a voter's issue orientations prior to the campaign may be modified as a function of the candidate's issue stands, as the voter sees them. That is, the voter will move toward a favored candidate's perceived position and/or away from that of a negatively evaluated candidate.

One might question the absence of party identification in Equation (4), particularly since some recent research has suggested an increasing alignment of issue opinions and partisan ties (RePass, 1971; Pomper, 1972). However, a closer examination of the proposed model in Figure 1 reveals that the specification does not imply a lack of relationship between partisanship and issue opinions. First, the *long-term* effect of party affiliation on policy preferences is captured in their correlation at  $t-1$ . In addition, the *endogenous* influence of party ties on issue stances is posited to flow via the effect of such ties on candidate evaluations.

This specification is supported both empirically (Markus, 1976; Converse and Markus, 1979) and by theoretical considerations. With regard to the latter, the argument is that, within the course of a presidential campaign, issues become imbued with partisan qualities insofar as the partisan rivals for office come to be identified with particular policies. The candidates form the link between partisanship and



Source: Compiled by the authors.

Figure 1. A Model of the Voting Decision

intra-campaign change in policy-related attitudes.

Equation (5) is derived from the ideas underlying the loss function model of evaluation. It posits that the net candidate evaluation differential is a function of issue-based losses and losses (or, in the positive, gains) accruing from preferences in candidate personalities and partisan identification:

$$\begin{aligned}
 (\text{Eval}_{1t} - \text{Eval}_{2t}) = & a + b_1(\text{Loss}_{1t} - \text{Loss}_{2t}) + \\
 & b_2(\text{Personality}_{1t} - \text{Personality}_{2t}) + \\
 & b_3ID_t + e_t.
 \end{aligned} \quad (5)$$

**Candidate Personality.** One would expect that, other things equal, Democrats will tend to prefer the personal traits of a Democratic candidate to those of a Republican office-seeker, and conversely for Republicans. The model, therefore, includes an equation which posits the difference in a citizen's ratings of the candidates' personalities to be a function of partisan ties:

$$\begin{aligned}
 (\text{Personality}_{1t} - \text{Personality}_{2t}) = \\
 a + b_1ID_t + e_t.
 \end{aligned} \quad (6)$$

**Party Identification.** The party identification equation is structured to reflect the possibility that while partisanship is relatively stable through time, it may nonetheless be influenced by prior voting behavior. Thus although the vote is the ultimate dependent variable within the context of a single presidential election, from a dynamic perspective it may feed forward to influence the voter's future political orientations.<sup>3</sup> The model also permits the precursors of current partisanship to interact, with voters and nonvoters differing in terms of

<sup>3</sup>It is important to keep in mind here, as elsewhere in the model equations, that we are less making an assertive assumption that prior votes *do* influence party identification, than merely building a model structure which permits such effects to be discriminated *if they exist*. Earlier recursive models of the Goldberg (1966) type were obliged to proclaim by assumption that these reverse effects of vote on party identification did not exist, at least within the time frame of the model. We do not make a contrary proclamation that such effects *do* exist, since a given testing of the model in a special case might show the relevant coefficients to be zero.

the relative stability of party identification through time:

$$ID_t = a + b_1 \text{ Demo. Vote}_{t-1} + b_2 \text{ Repub. Vote}_{t-1} + (b_3 + b_4 \text{ Voted}_{t-1}) ID_{t-1} + e_t \quad (7)$$

### Results

As mentioned earlier, the model of electoral choice was fitted against the CPS 1972-1976 panel data. These data contain information on a national sample of 1286 adult respondents interviewed in the periods immediately following the two presidential elections (see Miller et al., 1976; Miller and Miller, 1977; Converse and Markus, 1979).

Before detailing the results, we should make a few technical comments about our analytical methods. Because of the simultaneous nature of the system of equations, we employed two-stage least squares as an estimator, except in those instances where ordinary least squares estimation appeared justified. Lagged dependent variables were treated as predetermined with respect to the 1976 data, implying a lack of autocorrelation in the disturbances. Although this assumption is perhaps not fully warranted, it is not unreasonable, given the fairly long time lag between waves (cf. Hibbs, 1972). The assumption also finds support in some of our earlier analyses (Converse and Markus, 1979). Finally, the estimation proceeded in modular fashion from equation to equation, rather than being carried out initially for the structure as a whole, and thereby avoids the considerable attrition of case numbers that occurs when cases with missing data on as little as one of the totality of variables in the

structure are deleted from any role in the estimation.

**Party Identification.** The party identification variable ranges in seven integer steps from -3 (Strong Democrat) to +3 (Strong Republican). For purposes of operationalization, the other variables in Equation (7) are coded in a binary fashion, denoting whether the respondent voted in 1972 and, if so, for the candidate of which party. Nonvoters are coded zero on all three binary variables.

Estimation of Equation (7) by ordinary least squares yields the following results (standard errors in parentheses):

$$ID_t = -.37 - .20 \text{ Demo. Vote}_{t-1} + (.07) (.10) .49 \text{ Repub. Vote}_{t-1} + (.09) (.63 + .10 \text{ Voted}_{t-1}) ID_{t-1} + e_t. (.04) (.05)$$

$$R^2 = .64; N = 1252.$$

Party identification is found to be very durable from one election to the next, a result consistent with work on these and other data. Also, as hypothesized, the relationship between prior and current partisanship is somewhat stronger among voters than among nonvoters (with an estimated coefficient of  $.63 + .10 = .73$  for voters versus  $.63$  for nonvoters in 1972). But while partisan attachments move relatively little from one election to the next, they are not completely immune to electoral forces, as demonstrated by the significant coefficients attached to the partisan voting variables.

The feedforward effect of electoral choice on party identification is displayed in Table 1,

Table 1. Predictions of Party Identification<sub>t</sub>, by Prior Partisanship and Voting\*

Vote <sub>t-1</sub>	Party ID <sub>t-1</sub>						
	Strong Democrat	Democrat	Independent Democrat	Independent Republican	Republican	Strong Republican	
Nixon	-2.07 (D)	-1.34 (D)	-.61 (ID)	.12 (I)	.85 (IR)	1.58 (R)	2.31 (R)
None	-2.25 (D)	-1.63 (D)	-1.00 (ID)	-.37 (I)	.26 (I)	.88 (IR)	1.51 (R)
McGovern	-2.76 (SD)	-2.04 (D)	-1.31 (ID)	-.58 (ID)	.15 (I)	.88 (IR)	1.61 (R)

Source: Equation (7), as estimated using data from the 1972-76 Center for Political Studies National Election Panel Study.

\*Mid-points between scale values were used as cut-points to generate the parenthesized category predictions.

in which the coefficient estimates have been used to generate predictions of party identification at time  $t$  for different combinations of past identification and vote. The table shows that when partisanship is not reinforced by consistent electoral behavior, it may weaken. The predicted effect of a deviating vote in a single election is hardly dramatic; indeed, if it were, there would be reason to doubt the analysis. Nevertheless, the cumulative effects of a series of votes running counter to an individual's prior party ties might well lead to a conversion of partisan orientations at the individual level.

**Issue Opinions.** Five policy domains were selected for study here: social welfare, busing, government assistance to minority groups, tax reform, and women's rights. These issues were chosen because comparable items dealing with them were included in both the 1972 and 1976 Election Studies and because they were issues which were salient to many voters in 1976, as will be illustrated in a moment. For each policy domain, respondents were asked to place themselves and the major party candidates along a seven-point continuum with labeled endpoints.<sup>4</sup>

It is, of course, possible that the inclusion of other issues to the set of five might have altered the results of the analysis significantly, but we deem this unlikely. For one thing, the five issue domains appear to cover most of the public's major policy concerns in 1976. Our evidence for this statement is derived from an open-ended query in the 1976 interview regarding "the most important problem the country faces." Some degree of ambiguity is always inherent in mapping open-ended responses into fixed categories, but by our count over three-quarters of the answers directly concerned one or more of the five issues under study.

Perceptions of where the presidential candidates stood on the issues in 1976 varied with an individual's own policy preferences and affective orientations toward the contenders (see Table 2). Generally, a citizen's placement of a favorably evaluated candidate along an issue continuum is positively associated with the individual's own preferred position on the seven-point scale. At the same time, the perceived location of a negatively evaluated candi-

date is inversely correlated with self-placement. This relationship is by no means invariant across issue domains and candidates, nor do average perceptions of candidate positions float unanchored across the entire seven-point range. Nevertheless, the tabular results display an unmistakable regularity. Table 2 does not, however, enable one to gauge the extent of persuasion or projection, since the pattern of association is consistent with either of these hypotheses—or a combination of them. We must turn to the simultaneous equation model for further explanation of the interdependencies exhibited above.

Table 3 presents the two-stage least squares estimates of the ten (two candidates  $\times$  five issues) "projection" equations. The  $R^2$  values associated with these equations are fairly low, but then this is not unexpected: probably most of the variation in perceptions of candidate stands is simply noise arising from the presidential office-seekers' strategy of obscuring their positions on specific issues—especially in 1976.<sup>5</sup> Nevertheless, in all instances save one, the regression coefficient reflecting the extent of projection is significant, hovering around an estimated value of .005. One way of interpreting this value is that it implies that for a very favorably evaluated candidate (i.e., one receiving a translated thermometer score near +50), up to 25 percent of any difference between the candidate's objective issue position and the respondent's favored position would be "projected away."<sup>6</sup> Another way of interpreting these estimates is illustrated in Table 4. In that table, predicted candidate placements have been calculated for various combinations of self-placements on the first two issues and feelings toward the candidates. By comparing these predictions with the actual mean scores in Table 2, one can see that the mathematically parsimonious projection equations yield aggregate profiles that are consistent with the observed findings.

The two-stage least squares estimates of the equations for voters' preferences on the five

<sup>5</sup>Correlations are also probably depressed by the presence of error arising from imprecise instrumentation and the stochastic nature of the latent attitudes being measured (Converse, 1970; Achen, 1975; Converse and Markus, 1979).

<sup>6</sup>Multiplication of the .005 average coefficient estimate by the thermometer score of 50 yields the 25 percent figure. The translated thermometer used in estimating Equations (3) and (4) ranges from -50 to +50 degrees.

<sup>4</sup>The text of the issue items may be found in the CPS 1976 National Election Study codebook, available from the Inter-University Consortium for Political and Social Research.

Table 2. Mean Candidate Issue Placement, by Self-Placement and Candidate Feeling Thermometer Score\*

R's Location	Social Welfare			
	Carter		Ford	
	Warm	Cool	Warm	Cool
1 (Liberal)	2.2 ( 77)	4.8 ( 4)	3.4 ( 31)	6.2 ( 40)
2	2.5 ( 48)	3.9 ( 7)	3.6 ( 35)	5.9 ( 21)
3	2.8 ( 96)	3.4 ( 18)	3.9 ( 81)	5.4 ( 34)
4	3.2 (137)	3.0 ( 42)	4.1 (149)	5.3 ( 36)
5	3.8 (103)	2.5 ( 56)	4.4 (123)	5.0 ( 32)
6	3.5 ( 77)	2.2 ( 48)	4.5 (105)	4.2 ( 15)
7 (Conservative)	3.7 ( 87)	2.6 ( 77)	4.7 (142)	3.7 ( 28)
Grand Mean	3.2 (625)	2.6 (252)	4.3 (666)	5.2 (206)
eta-squared	.15	.13	.07	.27

R's Location	Busing			
	Carter		Ford	
	Warm	Cool	Warm	Cool
1 (Favor)	3.3 ( 28)	5.0 ( 3)	2.5 ( 10)	6.0 ( 20)
2	2.7 ( 27)	5.0 ( 2)	4.1 ( 21)	6.1 ( 8)
3	3.4 ( 18)	5.0 ( 2)	4.0 ( 16)	5.7 ( 6)
4	3.8 ( 44)	5.2 ( 12)	4.1 ( 39)	5.1 ( 19)
5	4.1 ( 32)	3.0 ( 11)	4.4 ( 36)	4.4 ( 14)
6	4.2 ( 79)	3.5 ( 35)	4.2 (105)	3.8 ( 22)
7 (Oppose)	4.2 (318)	3.3 (137)	4.5 (441)	3.3 (120)
Grand Mean	4.0 (546)	3.4 (202)	4.4 (668)	4.0 (209)
eta-squared	.06	.05	.03	.21

R's Location	Tax Reform			
	Carter		Ford	
	Warm	Cool	Warm	Cool
1 (Progressive)	2.6 (109)	3.9 ( 17)	4.0 ( 70)	5.3 ( 44)
2	2.8 ( 64)	2.9 ( 20)	3.6 ( 58)	5.0 ( 23)
3	3.1 ( 78)	3.0 ( 25)	3.9 ( 77)	4.5 ( 18)
4	3.3 ( 96)	2.8 ( 36)	4.2 (110)	4.4 ( 25)
5	3.5 ( 44)	2.8 ( 18)	4.6 ( 60)	3.9 ( 9)
6	4.0 ( 36)	3.0 ( 26)	4.5 ( 46)	4.9 ( 14)
7 (Same Rate)	4.3 ( 13)	3.4 ( 51)	5.0 (133)	4.2 ( 39)
Grand Mean	3.4 (557)	3.1 (193)	4.3 (554)	4.7 (172)
eta-squared	.17	.04	.10	.07

R's Location	Minority Groups			
	Carter		Ford	
	Warm	Cool	Warm	Cool
1 (Government Help)	2.3 ( 69)	4.0 ( 7)	2.9 ( 44)	5.8 ( 35)
2	2.5 ( 52)	3.0 ( 10)	3.3 ( 45)	4.8 ( 18)
3	2.9 (102)	3.0 ( 34)	3.4 (116)	4.6 ( 22)
4	3.4 (128)	3.0 ( 41)	3.8 (138)	4.4 ( 41)
5	3.6 ( 77)	3.3 ( 51)	4.3 (112)	4.5 ( 22)
6	3.8 ( 73)	3.1 ( 41)	4.1 ( 92)	3.3 ( 22)
7	4.2 ( 81)	2.4 ( 60)	4.5 (110)	3.0 ( 35)
Grand Mean	3.3 (582)	3.0 (244)	3.9 (657)	4.3 (195)
eta-squared	.20	.06	.12	.29



Table 2 (continued)

R's Location	Women's Rights			
	Carter		Ford	
	Warm	Cool	Warm	Cool
1 (Equal Rights)	2.6 (194)	2.5 ( 58)	2.3 (181)	3.6 ( 77)
2	3.0 ( 83)	3.2 ( 27)	3.0 ( 95)	4.1 ( 21)
3	3.3 ( 51)	3.6 ( 18)	3.5 ( 60)	4.1 ( 15)
4	3.5 (103)	3.5 ( 38)	3.7 (122)	4.5 ( 28)
5	3.9 ( 43)	3.5 ( 21)	4.0 ( 54)	3.6 ( 14)
6	4.1 ( 34)	3.0 ( 11)	4.0 ( 37)	2.8 ( 6)
7	4.1 ( 59)	3.7 ( 7)	4.0 ( 48)	2.9 ( 14)
Grand Mean	3.2 (567)	3.1 (180)	3.2 (597)	3.8 (175)
eta-squared	.16	.12	.23	.09

Source: 1976 Center for Political Studies National Election Panel Study.

\*Cool feelings include feeling thermometer scores of 0–49 degrees. Warm feelings are scores of 51–100 degrees.

issues are presented in Table 5. The analysis indicates that issue opinions possess a durability which, although much less than that of party identification, is nonetheless quite significant.<sup>7</sup> There is also evidence of the persuasion or leadership effect with regard to three policy domains: welfare, minority groups, and tax reform. The presidential candidates apparently exerted very little influence upon public sentiments towards busing and women's rights, however.

These findings recall a pattern that emerged in our first cut at these data (Converse and Markus, 1979). In that study, the stability of opinion on various issues was found to be

arrayed hierarchically, with attitudes on issues of a moral nature displaying a distinctively higher degree of temporal stability and hence apparently greater crystallization than did opinions on civil rights, domestic policy, and foreign affairs. It stands to reason that attitudes tied closely to one's sense of morality would not be very susceptible to the influence of campaign debate, and that is precisely what we find here.

Lastly, since the dependent and persuasion variables from Table 5 are expressed in the same currencies as the dependent and regressor variables, respectively, in the preceding set of equations, the relative effects of projection and persuasion may be compared.<sup>8</sup> Upon doing so,

<sup>7</sup>See Converse and Markus (1979) for a further discussion of the relative stabilities of political outlooks.

<sup>8</sup>We prefer the unstandardized coefficients to standardized ones for two reasons. First, the equations are nonlinear in the variables, rendering the usual

Table 3. Two-Stage Least Square Estimates for the Perceived Candidate Issue Position Equations

Issue	Candidate	Mean	Coefficient	Standard Error	N	R <sup>2</sup>
Welfare	Carter	3.01	.007	.001	854	.08
	Ford	4.51	.006	.001	869	.02
Busing	Carter	3.84	.005	.001	701	.06
	Ford	4.26	.007	.001	848	.06
Minorities	Carter	3.18	.005	.001	773	.04
	Ford	4.00	.005	.001	813	.04
Tax Reform	Carter	3.32	.000	.000	735	.00
	Ford	4.40	.003	.001	685	.01
Women	Carter	3.20	.002	.001	711	.01
	Ford	3.37	.005	.001	748	.03

Source: 1972–76 Center for Political Studies Election Panel Study.

Table 4. Predicted Perceived Candidate Location for Various Combinations of Self-Placement and Candidate Feeling Thermometer Score\*

Respondent Position	Social Welfare			
	Carter		Ford	
	Warm	Cool	Warm	Cool
1 (Liberal)	2.7	3.4	4.0	5.0
3	3.0	3.0	4.3	4.7
5	3.4	2.7	4.6	4.4
7 (Conservative)	3.7	2.3	4.9	4.1

Respondent Position	Busing			
	Carter		Ford	
	Warm	Cool	Warm	Cool
1 (Favor)	3.5	4.2	3.7	4.8
3	3.7	3.9	4.0	4.5
5	4.0	3.7	4.4	4.1
7 (Oppose)	4.2	3.5	4.7	3.8

Source: Equation (3), as estimated using data from the 1972-76 Center for Political Studies National Election Panel Study.

\*Scores of -25 and +25 degrees were used to represent cool and warm feelings, respectively, on a translated feeling thermometer ranging from -50 to +50 degrees. The former values are virtually identical to the observed mean feeling scores for the two sets of respondents.

one finds that with respect to both Carter and Ford the projection coefficients are larger than the corresponding persuasion values for all issues but one. The differences are by no means staggering, but they do suggest that projection is about 20 percent stronger than persuasion, on the average. One should bear in mind, however, that under certain circumstances—e.g., a new issue for which public opinion has not yet jelled—the influence of political leaders upon the electorate's policy preferences might be substantial.

**Candidate Personalities.** In 1976, respondents were asked to rate on a seven-point scale the degree to which each of the major candidates had "the kind of personality a President ought to have." The estimated equation linking the difference in ratings of Ford's and Carter's personalities to partisanship is (standard errors in parentheses):<sup>9</sup>

$$(\text{Ford Personality} - \text{Carter Personality}) = .20 + .60 ID_t + e_t \\ (.07) (.04)$$

The equation accounts for 20 percent of the variation in the dependent variable ( $N = 1193$ ), equivalent to a standardized regression coefficient of .44 for party identification. On the average, the personalities of both candidates were scored at about 1.4 on a -3 to +3 scale. The unstandardized coefficient estimates show Ford receiving a score about two points higher than Carter among Strong Republicans, however, and about 1.6 points below Carter among Strong Democrats.<sup>10</sup>

**Evaluations of the Candidates.** Equation (5) posits that a citizen's overall evaluations of the presidential candidates are formed from a mix of policy considerations, partisan predisposi-

interpretation of standardized coefficients may be arbitrarily altered by rescaling the original variables (Allison, 1977). Moreover, since standardized coefficients are by definition variance-sensitive, comparisons of their values across equations—even linear ones—can lead to misinterpretations.

<sup>9</sup>The equation was estimated by ordinary least squares, which assumes a lack of correlation between the disturbances in the party identification and per-

sonality differential equations. The consequence of this assumption being incorrect would be to overstate the impact of the independent variable. Given the plausibly moderate value of the coefficient estimate, the assumption appears not to have been grossly unreasonable.

<sup>10</sup>The predicted dependent variable value is  $.20 + .60(+3) = 2.0$  for Strong Republicans and  $.20 + .60(-3) = 1.6$  for Strong Democrats.

Table 5. Two-Stage Least Squares Estimates for the Voter Issue Position Equation

	Coefficient	Standard Error
<b>Social Welfare</b>		
Constant	2.59	.14
Issue Position <sub>t-1</sub>	.47	.03
Carter Persuasion	.006	.001
Ford Persuasion	.006	.002
$R^2 = .29$ N = 883		
<b>Busing</b>		
Constant	2.35	.19
Issue Position <sub>t-1</sub>	.61	.03
Carter Persuasion	.002	.001
Ford Persuasion	.003	.001
$R^2 = .40$ N = 685		
<b>Minority Groups</b>		
Constant	2.25	.14
Issue Position <sub>t-1</sub>	.52	.03
Carter Persuasion	.004	.001
Ford Persuasion	.004	.002
$R^2 = .33$ N = 764		
<b>Tax Reform</b>		
Constant	2.54	.22
Issue Position <sub>t-1</sub>	.40	.05
Carter Persuasion	.007	.002
Ford Persuasion	.004	.002
$R^2 = .16$ N = 657		
<b>Women's Rights</b>		
Constant	1.67	.16
Issue Position <sub>t-1</sub>	.41	.05
Carter Persuasion	-.001	.002
Ford Persuasion	.001	.003
$R^2 = .21$ N = 689		

Source: Data from the 1972-76 Center for Political Studies National Election Panel Study.

tions, and beliefs about the personalities of the rivals for office. The operationalizations of the regressors in that equation are by now familiar, with the exception of the issue-loss component. The issue loss associated with each candidate was constructed as the average squared difference between the respondent's self-placement on each issue and his or her perceived location of the candidate. We used an average rather than a simple sum of squares because not all respondents could provide both a preferred policy and a candidate location for all five issues.

This method weights each included issue equally, a procedure which appears to conflict with the notion that all issues may not be

equally salient to the voter. A number of considerations render this approach less objectionable than it might seem, however. First, taken as a whole the five issues are salient ones, and the majority of respondents were able to locate themselves and the candidates on each of the issue continua. Moreover, issues that are utterly non-salient to a respondent are given a weight of zero, since both self and candidate placements are required for the issue to be included in the loss calculus. Third, preliminary attempts to devise a weighting scheme based on open-ended and other responses actually led to a slight decrease in explanatory power for the regression equation. This came as little surprise, since a number of recent psychological studies have demonstrated that people are generally quite unreliable in assessing the relative importance of factors in determining their decisions (Nisbett and Wilson, 1977). Moreover, other work indicates that the choice of weights tends to make little difference in the ultimate predictions (Wainer, 1976; Dawes and Corrigan, 1974).

The two-stage least squares estimates for the candidate evaluation equation are displayed in Table 6. The analysis indicates that all three elements—issues, party, and personalities—were important determinants of feelings toward Ford and Carter. Taken together, these three variables account for two-thirds of the variation in the feeling thermometer differential for the presidential rivals.

The results suggest that the perceived personal qualities of the candidates weighed most heavily, at least in the direct-effect sense, in determining the public's overall evaluations of Ford and Carter. The standardized coefficient for the personality differential variable equals .43, and the unstandardized coefficient implies nearly a 40-point difference ( $6.26 \times 6$ ) in the thermometer scores of candidates rated at opposite extremes of the presidential personality scale.

Issues and party ties were about equally important in terms of their direct effect on candidate evaluations, with standardized coefficients of -.30 and .29, respectively. Keeping in mind that the issue loss differential can range from +36 (favoring Carter) to -36 (favoring Ford), note that the unstandardized coefficient for that variable implies that for each unit increase in Ford's loss value relative to Carter's, the voter's thermometer difference (Ford-Carter) decreased by a little more than one degree, other variables held constant. At the same time, the coefficient for party identification indicates a 20-degree differential favoring Ford among Strong Republicans and a 14-degree contrast

Table 6. Two-Stage Least Squares Estimates for the Evaluation Differential Equation

	Unstandardized Coefficient	Standard Error	Standardized Coefficient
Constant	2.72	.77	
Ford-Carter Issue Loss	-1.34	.38	-.30
Ford-Carter Personality	6.26	.51	.43
$ID_t$	5.66	.64	.29
$R^2 = .67$ N = 1013			

Source: Data from the 1972-76 Center for Political Studies National Election Panel Study.

favoring Carter among Strong Democrats, *ceteris paribus*.<sup>11</sup>

The fairly modest standardized coefficient for party identification might be somewhat puzzling, given the crucial role that partisanship has been assumed to play in electoral behavior. It is important to remember, however, that in addition to its direct impact on candidate evaluations, party affiliation also exerts a substantial *indirect* influence via its effect upon perceptions of candidate personalities. When this indirect influence is combined with the direct effect, it yields an "effects" coefficient of  $.29 + .43(.44) = .48$ , larger than the standardized coefficient for either candidate personalities or issue proximities (see Figure 2). This latter value may be more in line with intuitive judgments—and prior evidence—about the impact of partisanship on candidate evaluations. Moreover, under some circumstances party identification may play a significant role in determining the electoral decision quite apart from its influence on feelings toward the candidates, as we shall see in a moment.

<sup>11</sup>For Strong Republicans, the expected thermometer differential is  $2.72 + 5.66(+3) = 19.70$ ; for Strong Democrats it is  $2.72 + 5.66(-3) = -14.26$ .

The Vote Decision. Least squares estimation of the vote choice equation generates the following results (standard errors in parentheses):<sup>12</sup>

$$\text{Ford Vote} = .506 + .009 (\text{Ford Eval.} - .010) (.0004)$$

$$\text{Carter Eval.)} + (.141 - .0025 | .009) (.0002)$$

$$\text{Ford Eval.} - \text{Carter Eval.}) ID_t + e_t.$$

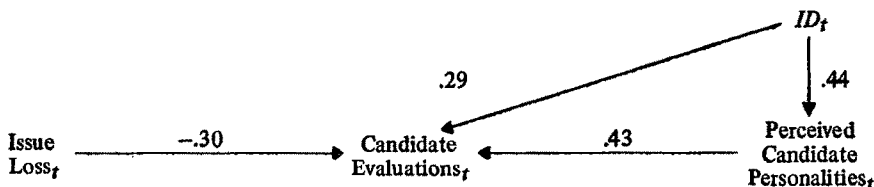
<sup>12</sup>Both two-stage least squares (2SLS) and ordinary least squares (OLS) estimators were originally employed. Although the methods yielded similar coefficient estimates, the OLS values are preferred here because they yield fewer predictions outside of the allowable 0-1 range and because the 2SLS residuals were only modestly correlated with residuals elsewhere in the system, thus permitting the more efficient OLS estimation. The 2SLS estimates (with standard errors parenthesized) are:

$$\text{Ford Vote} = .503 + .011 (\text{Ford Eval.} - .012) (.001)$$

$$\text{Carter Eval.)} + (.157 - .0036 | \text{Ford Eval.} - .019) (.0007)$$

$$\text{Carter Eval.}) ID_t + e_t.$$

$$R^2 = .65; N = 753.$$



Source: Based on the regression estimates outlined in the text.

Figure 2. Candidate Evaluations Segment of the Model of Electoral Choice

The regression analysis lends clear support to the hypothesized vote choice equation. All coefficients have the expected signs and are many times larger than their associated standard errors. The  $R^2$  value for the equation equals .64 ( $N = 884$ ), but because of the dichotomous nature of the dependent variable, the best measure of goodness of fit is provided by the fact that the equation correctly predicts respondents' votes 90 percent of the time.<sup>13</sup>

Moreover, it can be shown that the direct intrusion of party identification in influencing the vote choice when there is relative indifference in candidate evaluations is the only case in which prior variables in the model affect the vote, save indirectly through the comparative candidate evaluations. That is, the addition of other model variables, including most notably the issue losses and personality ratings, to the simple vote choice regression equation based on differential candidate evaluations and party identification, leads to virtually no increase in predictive accuracy. As a further test, we applied the estimated equation to the data from the 1972 wave of the panel to ascertain its predictive accuracy with regard to the Nixon-McGovern contest.<sup>14</sup> The result was that 97 percent of the voters were correctly classified, a very satisfactory result given that the coefficient estimates are based on an entirely different set of data. Indeed, the predictive accuracy might seem to be *too* good, but the explanation is simply that evaluations of Nixon and McGovern tended to be more disparate than those for Ford and Carter, and hence electoral choice was more predictable than in 1976.

The values of the coefficient estimates possess some charming qualities. For instance, the constant term implies a virtual 50–50 split in the vote of Independents who evaluated the two candidates equally. Furthermore, if one simulates an election wherein net short-term partisan forces (i.e., the evaluation differential) are zero and inserts the sample mean for the party identification variable, the prediction (interpreting the result in aggregate terms) is a 46 percent vote for the Republican candidate—

precisely the value of the normal vote (Converse, 1966).

Note that the direct impact of party identification upon the vote decision depends upon the size of the candidate evaluation differential. When both candidates are evaluated identically, each step toward the Republican end of the partisan continuum increases the predicted dependent score (roughly, the probability of voting for Ford) by .14. Once the difference in candidate evaluations reaches about 55 degrees, however, the predicted direct effect is partisanship on the vote is virtually nil, as indicated by its small coefficient:  $.141 - .0025(55) = .003$ .<sup>15</sup> Although the direct influence of party ties on the vote decision would be negligible under these circumstances, their indirect effect, i.e., their prior impact on the candidate evaluations themselves, nevertheless remains appreciable.

The relative effects of candidate evaluations and party identification on the choice between presidential contenders is illustrated in Table 7. As displayed there, the predicted probability of a Ford vote ranges from .00 for Strong Democrats, with a 50 degree evaluation differential favoring Carter, to 1.00 for Strong Republicans, with an equally large pro-Ford difference. The table also illustrates the varying impact of party identification, depending on the contrast in candidate evaluations: when the differential is large, the probability entries do not vary much across partisan categories; if the evaluation differential is within the bounds  $\pm 20$  degrees, however, the residual impact of partisanship on electoral choice is more potent.

### Conclusions

From a substantive point of view, a model is something more than the sum of its parts. We may therefore conclude by moving from an examination of the separate equations comprising the model to a brief summary and discussion of its broader implications.

Perhaps most importantly, the hypothesized model of electoral choice has fared rather well against the 1972–1976 panel data. No alterations in the posited specification were indicated. To the contrary, a number of  $R^2$  values approach what must be their upper bounds,

<sup>13</sup> Respondents with estimated dependent variable scores  $>.50$  were predicted to have voted for Ford, while those with estimated values  $\leq .50$  were classed as Carter voters.

<sup>14</sup> This prediction to the 1972 presidential vote refers, of course, to the vote choice equation taken alone. The full model being presented could not be estimated for 1972 separately, for lack of prior panel data in that year.

<sup>15</sup> In 1976, 15 percent of all respondents saw no difference in their overall evaluations of the candidates. One half of the sample had evaluation differentials of 25 degrees or less, and nearly 90 percent are included in the  $\pm 55$  degree span.

Table 7. Predicted Probability of a Ford Vote, by Party Identification and Candidate Evaluation Difference

Differential	Party Identification						
	Strong Democrat	Democrat	Independent Democrat	Independent	Independent Republican	Republican	Strong Republican
(Pro-Carter)							
-50	.00	.02	.04	.06	.07	.09	.10
-40	.02	.06	.11	.15	.19	.23	.27
-20	.05	.14	.24	.33	.42	.51	.60
-10	.07	.18	.30	.42	.53	.65	.76
0	.08	.22	.37	.51	.65	.79	.93
10	.25	.36	.48	.60	.71	.83	.94
20	.41	.50	.60	.69	.78	.87	.96
40	.74	.78	.83	.87	.91	.95	.99
(Pro-Ford)							
50	.91	.92	.94	.96	.97	.99	1.00

Source: Equation (2), as estimated using the 1972-76 Center for Political Studies National Election Panel Study.

given normal sampling error and other idiosyncratic sources of noise.

The analysis affirms the crucial role of candidates in the dynamics of electoral choice. Candidate evaluations have been shown to be a primary determinant of the vote, with policy considerations and even partisan orientations affecting the vote either exclusively or largely through the way they help to shape feelings toward the presidential rivals. The candidates also mediate the flow of campaign stimuli toward the cognitive predispositions comprising the citizen's network of political beliefs: they serve as the primary vehicle through which policy debate takes on partisan coloration, and through their determining influence on the vote, feelings toward the candidates even act dynamically in at least a small degree to modify or reinforce standing party ties. In the American system of elections, the choice is ultimately between competing candidates.

The fact that comparative candidate assessments are the most potent proximal determinant of the vote decision should not, however, lead us to overlook the causally prior impacts of issues and parties on these assessments. Policy considerations were shown to be significant in determining voters' evaluations of the 1976 presidential candidates. Some of these manifest linkages between issues and candidates turned out to be modestly circular in one sense or another. Thus, for example, a degree of "projection" was uncovered, whereby voters reporting issue positions of candidates they already liked would shade these perceptions toward the issue positions the voter already preferred, while assuming that disliked candidates must have more sharply dissimilar positions. In the same causal nexus it was possible to isolate a persuasion effect as well, whereby

the voter appeared to be shifting his or her own reported issue position to conform more closely with that of a preferred candidate, or to distinguish it more sharply from that perceived for a disliked candidate. The persuasion effect was less strong in these data than the projection effect, and furthermore showed signs of variation by type of issue. For issues like school busing or women's rights, where there is independent evidence of sharp crystallization of public feeling, any persuasion effect is negligible. It is chiefly on issues where voters are less personally exercised that the policy position of candidates can sway their admirers' views of the issue.

However intriguing and plausible these side effects may be, the estimates we have derived from the model make clear that they remained no more than side effects in 1976. The policy differences consensually perceived to exist between the candidates, coupled with prior differences in voter positions on these issues, had a noteworthy effect on voters' comparative assessments of the candidates, and through these invidious assessments, the policy terms ultimately left their mark on final voting decisions.

Similarly, the model helps to delineate more clearly than did prior work the important function of partisan predispositions in the processes leading to a voting choice. Earlier investigations limited to static data bases have often tried to assess the relative role of parties, issues and candidates by assigning each a single ultimate regression weight, leading to comparisons which take on the flavor of a simplistic horse race. Viewing the formulation of a voting decision in terms which are more explicitly processual may rob us of such a simple "final score," but may yield redeeming satisfactions due to greater verisimilitude.

In such process terms, the causal role of partisanship appears to be particularly important for two reasons. First, party identifications are much more stable in the intermediate term than other elements in the model. If the game were redefined as one of predicting a voting decision on the basis of political attitudes examined eight years before election day, there would be little contest: the identity of the candidates would be utterly unknown at such remove, some issues to become important later would also be unknown, and voter attitudes on other more abiding issues would be subject to considerably greater flux in the interim.

The second reason shows up clearly in the structure of the model and hinges on the fact that the party component is unique in the way it intrudes at multiple points in the process. Partisan predispositions may be outweighed by other model terms at particular stages, as other research on a static base has shown, but these loyalties keep coming back as determinants while the vote decision process unrolls. For example, Figure 2 suggested that neither issues nor partisanship were as important in their direct effects upon ultimate comparisons between the candidates as were simple judgments of their personalities. However, the very same figure makes clear that reactions to candidate personalities have already been shaped by prior partisan predispositions, so that there is an indirect path from party to candidate evaluations which is of imposing proportions in itself, quite apart from the direct path also depicted. Or at a later point, partisanship again enters the model significantly in influencing the final vote when the voter has trouble making sharp affective discriminations between the candidates.

In the same vein, given the durable nature of these loyalties over lengthy periods of time (Converse and Markus, 1979), it may well be that prior partisanship has also intruded at stages too early for adequate representation in our model. Thus, for example, we located an edge of "persuasion" whereby the voters' issue locations were mildly affected by the issue positions perceived for liked and disliked candidates. If such candidate persuasion exists, then it would not be at all surprising if there were an earlier *party* persuasion term of entirely parallel structure, accounting for some of the party-relatedness of issue positions at our first time point, yet which is exogenous to our model as presented. Such party persuasion would presumably be operative in the first stages of issue emergence, and hence likely to have more impact on the distribution of attitudes in the electorate than do candidates who arrive on the

scene after such issue positions have become at least modestly crystallized.

In short, then, while partisan predispositions are unlikely to dominate the process completely at given stages where the candidates are being assessed, these loyalties appear to make repeated inputs of substantial magnitude throughout the process.

We believe that the model as presented is an integrated and generalizable one that captures most of what is important as voters face elections involving candidate competition. The model could be enlarged to become more general still. It could, for example, add considerations of turnout, representing the fact that voting and nonvoting are for the most part habitual differences, but capturing those margins of turnout variance that are in fact current and dynamic responses to the specific election configuration, such as the likely diminution in turnout that may accompany feelings of sheer disgust about *both* candidates. The model could also be extended more deeply in time, explicating the genesis of the relationships between prior issue positions, prior party loyalties, and prior votes.

Perhaps the most important consideration to keep in mind is that whereas we have used data from the specific period 1972–76 for estimation of model parameters and assessment of general fit, the model *per se* should be seen as a shell, or a vessel, designed to apply to a wide range of specific elections. Estimates of specific parameters would naturally vary across types of elections, or for specific elections of the same series over time. To seize an example which is too obvious, for candidate elections which are truly non-partisan in fact as well as intent, the coefficients representing the various points of impact of the party term in the model would by definition fall to zero.

It is this conditional variation from one election to another that becomes of subsequent interest to understand. The present model is thus a satisfying vehicle for providing the kinds of parameter estimates that seem to express the voting calculus well in any special case.

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# Reciprocal Effects of Policy Preferences, Party Loyalties and the Vote

BENJAMIN I. PAGE  
*University of Chicago*

CALVIN C. JONES  
*National Opinion Research Center*

*Past studies have offered diverse estimates of the role of policy preferences, party loyalties, candidate personalities and other factors in voting decisions. Most have postulated recursive (that is, one-way) causal relationships among the central variables.*

*This study specifies a non-recursive simultaneous equation model and estimates its parameters for the 1972 and 1976 elections using CPS data. The estimates differ markedly from those of simple recursive models. Policy preferences appear to have much more influence on voting decisions, and party attachments much less, than was previously thought. Candidate evaluations strongly affect voters' perceptions of closeness to candidates on policy issues. Party identification may be influenced by short-term factors. Differences between 1972 and 1976 reflect the issue-oriented McGovern candidacy.*

*Simultaneous equation models offer no cure-all; in the absence of accepted theory many specifications are open to controversy. But future research must take account of reciprocal causal paths.*

Students of political behavior have long been interested in the extent of "policy voting," that is, the degree to which citizens take account of the public policy stands of candidates when they cast their votes. They have sought to discover the importance of policy orientations in voting, and how they compare with such factors as long-term partisan loyalties and perceptions of the character and personal qualities of the candidates. Answers to such questions are thought to bear not only upon the workings of individual psychology, but also upon theories of democratic politics (in terms of the rationality and cognitive capacities of the citizenry) and upon various theories of the decision process in voting.

Unfortunately, more than 30 years of research have left many issues unresolved. Varying conceptions of the central variables and differing methods of measurement and analysis

have led to widely divergent conclusions, even on such apparently straightforward matters as whether policy positions, party loyalties or candidates' personal characteristics were more important in a given election, or whether or not policy concerns increased in importance in the 1960s and 1970s in comparison with the 1950s.<sup>1</sup> It is our purpose to further this research with some new empirical evidence. In doing so we must also complicate matters, suggesting that virtually all past voting studies have erred by ignoring the possibility of reciprocal causal effects among the central variables of the electoral process.

Among the few exceptions is the work of Jackson (1975), whose non-recursive voting model of the 1964 election specified causal interdependence between partisan affiliations of voters and their evaluations of the public policy stands of the parties and candidates. Although we argue below that Jackson's model omits certain crucial reciprocal linkages, it was the first instance of a system of structural equations to consider the roles of partisanship and policy orientations as dependent, as well as independent variables, and represented a major step forward in electoral research. More recent papers by Achen (1976) and Markus (1976) also allow for some reciprocal causal links.

We are grateful for research support from the National Science Foundation, the Social Science Research Council, and the Social Science Research Committee of the University of Chicago. The origins of this study owe much to Richard A. Brody and Hayward R. Alker, Jr. A number of helpful criticisms and suggestions were offered by John Ferejohn, John Jackson, Herbert Kritzer, Merrill Shanks, Arthur Stinchcombe and others. Robert Y. Shapiro provided able research assistance. The data were made available through the Inter-University Consortium for Political and Social Research; the responsibility for analysis and interpretation is our own.

<sup>1</sup>See Repass (1976), Kessel (1972), Brody and Page (1972).

Within the last several years survey data have become available which allow researchers to design and test sophisticated electoral process models. Yet, while a great many scholars have seized upon these data to improve their operationalizations of central electoral variables, few seem to have recognized all of the implications of the newer constructs (e.g., the "proximity" measures of candidate policy evaluations) for the task of specifying the equation, or system of equations in their statistical models. We will present below a full non-recursive voting model. For data we have turned to the 1972 and 1976 presidential election surveys conducted by the Center for Political Studies (CPS) at the University of Michigan. These are the first election studies to include the measures we believe necessary for constructing an adequate model of the electoral process.

#### One-Way Causation: Recursive Models of Voting

Most published voting studies to date, from the simplest to the most complicated, have been based upon some form of recursive model of the voting decision. Causation in these models is assumed to operate in one direction only, typically from policy preferences and/or party affiliation and/or candidate evaluations to the vote.<sup>2</sup> We intend to specify and estimate a non-recursive model, in which causation may be reciprocal, simultaneously operating in both directions among several pairs of variables. For comparative purposes, however, we first consider some typical recursive models and their results. We begin with a simple bivariate model which postulates that citizens' policy preferences uniquely determine their voting choice.

We conceptualize the voting choice somewhat differently from usual. Most past researchers have focused on a dichotomous dependent variable—the respondent's prospective or retrospective report of his or her vote for one of the major party candidates. We prefer to analyze the respondent's net comparative evaluation of the opposing candidates, as measured by the arithmetic difference between the scores given to each of the candidates on the CPS "feeling

thermometer" scales.<sup>3</sup> The candidate thermometer scores can be argued to represent rather well what utility prospective voters think they would gain or lose from the election of a given candidate. The difference between thermometer scores for Democratic and Republican candidates, then, represents the net gain (or loss) in utility which the citizen would expect to receive as a result of the election of the Republican rather than the Democratic candidate. In this sense, it closely resembles Downs' concept of the "expected party differential" (1957).

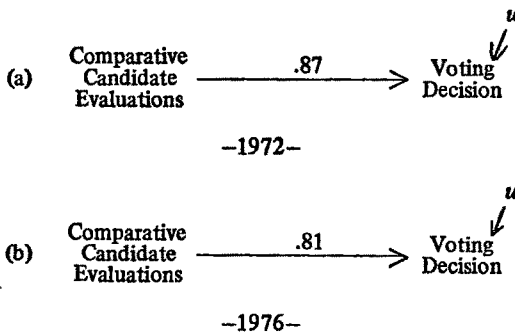
Past empirical work has shown that such comparative evaluations of candidates, whether measured by the difference in thermometer scale scores or by a net count of positive and negative open-ended comments about the parties and candidates, are excellent predictors of voting decisions (Brody and Page, 1973; Kelley and Mirer, 1974). In both 1972 and 1976, according to our own analysis, over 95 percent of the voters who scored one candidate higher than the other on the thermometer scale reported voting for that candidate. Ordinary least squares regression of the binary vote measure upon a trichotomization of the net comparative evaluation measure resulted in standardized regression coefficients or betas of .81 for 1976 and .87 in 1972, with corresponding gammas of .95 in both years (see Figure 1).

It can be argued that party loyalties have some direct effects upon the vote in breaking ties when citizens evaluate the opposing candidates equally. We would contend, however, that here partisanship is as likely to be a dependent variable, if anything, and that the apparent additions to explanatory power by party and other variables may merely reflect the imperfection of thermometer scores as measures of the overall utility expected from the election of candidates.

Comparative evaluations, in other words, are so closely related to the vote that they can be substituted for the binary measure in our analysis. Such a substitution has the advantage of providing a continuous variable which can be related to other continuous variables without need for transformations such as logit or probit—which in fact would conflict with our

<sup>2</sup>The term "recursive"—literally "running back"—is unfortunate, because it gives exactly the opposite of the correct impression. It does not actually describe relationships between variables, but refers to a property of the corresponding system of equations. It is probably too late to undo the confusion resulting from this usage.

<sup>3</sup>Thermometer scores range between 0 ("very cold or unfavorable") through 50 ("no feeling at all for candidate") to 100 ("very warm or favorable"). In the 1972 survey, when thermometer questions were asked both before and after the election, we used average scores in order to reduce measurement error.



Source: 1972 and 1976 national election studies, Center for Political Studies, University of Michigan.

Figure 1. Effects of Comparative Candidate Evaluations on the Voting Decision: 1972 and 1976

theoretical specification of an expected utility decision rule—in order to capture more accurately the underlying mathematical function associated with the vote for a particular candidate. It also has the advantage (important for our non-recursive models below) of clearly conceptualizing the attitude immediately proximal to the voting act as one which can conceivably affect, as well as being affected by, other attitudinal variables. Put most simply, we believe that the voting intention is not only a dependent variable, but may be an independent variable as well.

We will now consider several alternative measures of issue orientations, in each case—for the sake of simplicity—dealing with a summary measure of many policy preferences rather than trying to estimate separate effects for different policies. The first, which we will call the “Policy Preference Index,” is a weighted linear composite of respondents’ scores on a series of closed-ended policy items: the CPS self-rating scales. It resembles the policy index used by Nie, Verba and Petrocik (1976).

A second approach to measuring policy orientations makes use of a simple net count of pro-Republican minus pro-Democratic policy-related comments offered in response to the CPS open-ended questions concerning likes and dislikes for the major parties and candidates. It is similar to the policy variables used by Stokes (1966). This measure (which we have labeled “Net Policy Comments”) allows respondents to define their own policy concerns, rather than imposing the same scales of policy alternatives upon everyone: it counts policy orientations as meaningful only when they are salient enough to be mentioned spontaneously by a respondent.<sup>4</sup>

We need a third measure of policy orientation in order to take account of the theoretical literature on spatial modeling which has grown out of the work of Hotelling (1929), Downs (1957), Davis, Hinich and Ordeshook (1970), and others. This literature specifies precisely how policy voting could occur in terms of an individual’s decision calculus. It argues that voters take into account their perceptions of the policy proposals of the candidates as well as their own preferences, and that they vote for the candidate whose policy stands are perceived to be closer to their own preferences. Depending upon where the candidates actually stand and where the voters perceive them to be, there might or might not, according to these theories, be a linear relationship between voters’ policy preferences and their intended vote. There should, however, always be a direct relationship between comparative policy distances and evaluation of the candidates.

Accordingly, we computed an indicator of “Comparative Policy Distances” between candidates and voters. There are various ways to construct policy distance measures; ours uses the CPS closed-ended, self-rating policy preference scales together with the associated perceptions of candidates’ stands to construct a single, relative distance between the respondent and both candidates. That is, absolute distance measures were computed between the voter’s preferred position and the perceived positions of the Republican and Democratic candidates on each policy scale. For each voter, distance from the Republican was then subtracted from his or her distance from the Democrat, and the resulting signed, algebraic scores were weighted and summed over all policy scales. The result tells which candidate a voter feels closer to, and by how much.<sup>5</sup>

<sup>4</sup>We experimented with a number of coding rules governing which comments to count as policy-oriented, ranging from the vague and general to only the most specific. Inclusion of vague comments led to a much larger, and probably inflated, relationship with candidate evaluations. The estimates reported here are all based upon a narrow, specific definition of policy concerns.

<sup>5</sup>We presume linearity rather than the quadratic loss function specified by Davis et al. (1970) because we see the CPS seven-point scales, with their labeled endpoints but unlabeled interiors, as encouraging respondents to report positions and distances in utility units rather than objective policy units. If they do, the linear relationship with candidate evaluations follows directly from the rationality assumptions of spatial models. Observed relationships are very nearly linear.

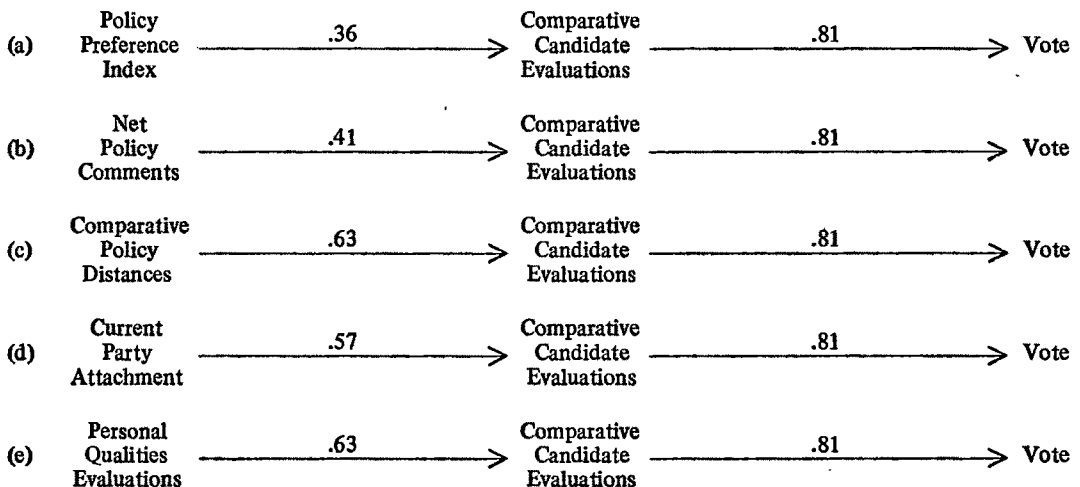
Ordinary least squares estimates (standardized) of the effect of issues upon the vote, using each of these three policy measures and the simple bivariate model, are given in Figure 2. Plainly it makes a difference how policy orientations are measured. The first two estimates, based on simple policy preferences and open-ended policy comments, are similar and of moderate size. The latter is slightly larger, suggesting that policy comments may indeed more accurately reflect issues of real concern to voters. But more striking is the fact that the third estimate, using policy distances, is considerably larger than the other two. On the face of it, this seems to support the argument that spatial models do a superior job of specifying the form and process of the voting decision. However, there is an alternative explanation which we will deal with later: namely, that distance or proximity estimates of policy voting may be artificially inflated when the possibility of reciprocal causation is ignored.

In order to round out the bivariate approach to explaining the vote, we included in Figure 2 separate estimates of the effects of two non-policy variables, subjective party attachments and evaluations of the candidates' personal and leadership qualities. Subjective partisanship is measured by the usual CPS seven-point classification of "party identification" based on whether, generally speaking, respondents consider themselves Democrats, Republicans, Independents or whatever. In magnitude, the effect of

partisanship ranks between the estimates for "Policy Preferences" and "Policy Comments," on the one hand, and "Comparative Policy Distances," on the other. This result highlights a problem faced by scholars interested in the relative importance of partisanship and policy concerns in the voting process. Obviously, a great deal depends upon how one defines and measures policy orientations at the outset.

We measured reactions to candidates' personalities by counting the net number of pro-Republican minus pro-Democratic comments in response to the CPS open-ended questions, including only those comments focusing on personal or leadership qualities and quite devoid of policy or partisan content. The standardized estimate of the effect of candidates' personal qualities on evaluations is bigger than that for subjective partisanship, policy preferences or policy comments, and equal in size to the bivariate coefficient for our comparative policy distance measure.

The most obvious objection to all these estimates is that they take account of only one independent variable at a time. If policy orientations, partisan commitment and evaluations of candidate character each have some independent influence on the voting decision, and if these variables are even moderately collinear, then the bivariate estimates will be biased.



Source: 1976 national election study, Center for Political Studies, University of Michigan.

Note: Symbols for disturbance terms are omitted for the sake of clarity.

Figure 2. Bivariate Recursive Models of Voting Behavior, 1976

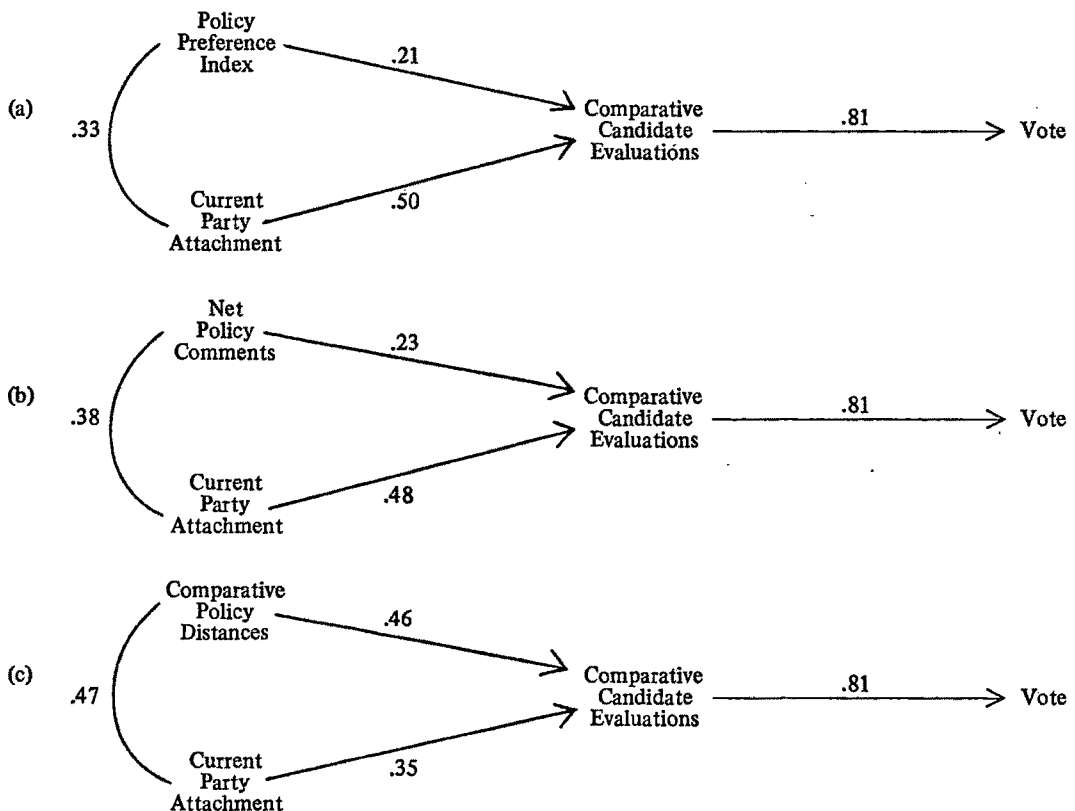
Under the usual circumstances (i.e., positive correlations among the regressors), the bias would be upward, so that bivariate methods are quite likely to overestimate the extent of policy voting—or, for that matter, the extent of party or candidate voting (Johnston, 1972). Some of the work of Boyd (1972), Pomper (1975), Aldrich (1975), Miller et al. (1976) and others is subject to this criticism. Particularly vulnerable is the issue-by-issue “normal vote” technique, since issues may be collinear with each other as well as with other variables.

Of course, most analysts are aware of this problem and incorporate two or more independent variables in their voting models. It is common, for example, to include one or another measure of policy orientations together with partisan loyalties.

Some estimates for such models are shown in Figure 3. As expected, the estimates for both

policy orientations and partisanship effects are somewhat lower than those in Figure 2. Party attachments now appear to exert more than twice as much influence upon voting intentions as do either policy preferences or the net policy comments measure. The estimate for policy distances, however, remains higher than that for partisanship when both variables are included, although the disparity is not large.

One can easily add evaluations of the candidates’ personal qualities to these models, as in Figure 4. The introduction of an additional explanatory variable results in still further reduction of the estimates for each separate effect. Yet, in relative terms, the initial findings continue to hold: in 1976 voters’ evaluations of the candidates’ personal attributes appear substantially more important than their partisan attachments in determining the vote choice. Furthermore, both current partisanship and



Source: 1976 national election study, Center for Political Studies, University of Michigan.

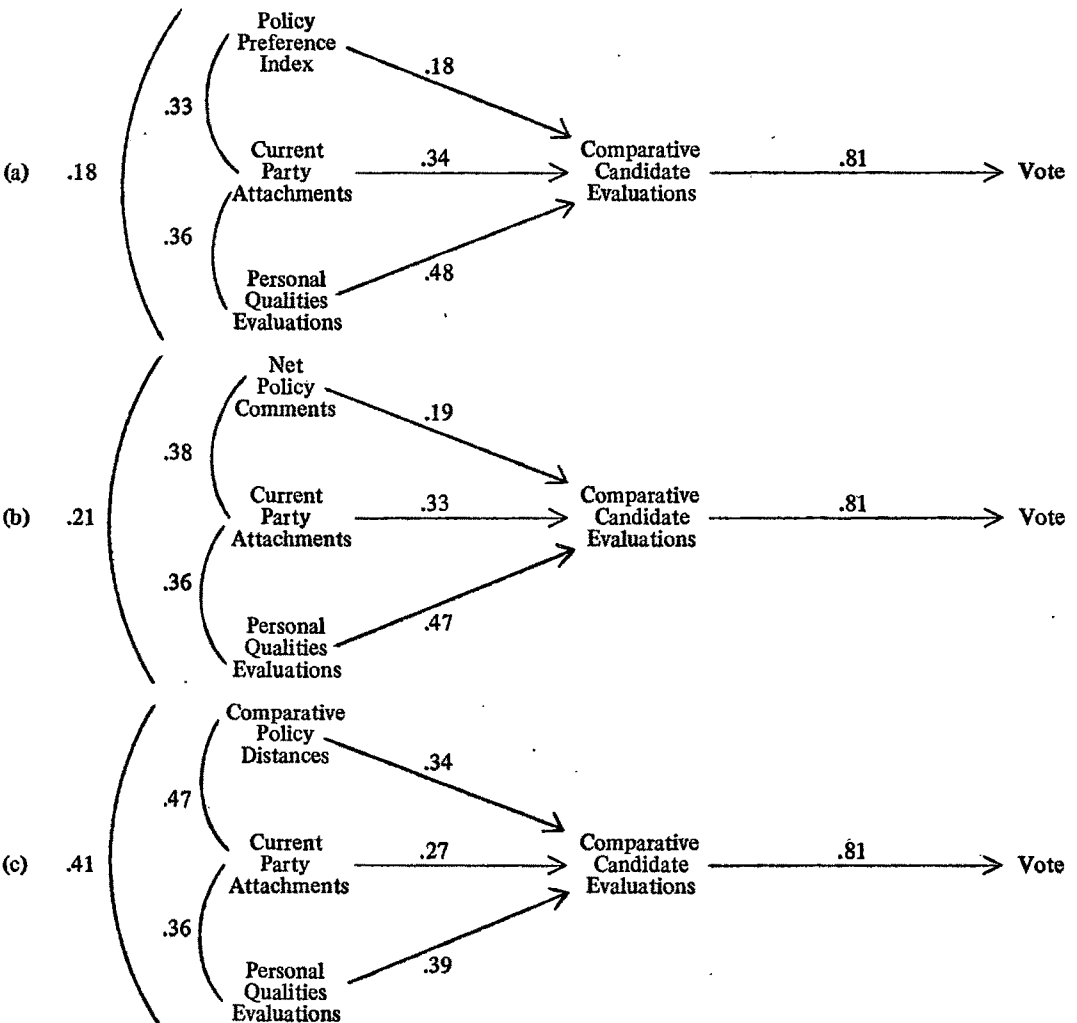
Note: Symbols for disturbance terms are omitted for the sake of clarity.

Figure 3. Multivariate Recursive Models of Voting Behavior for 1976 with Policy Orientations and Partisanship as Independent Variables

candidates' personal qualities seem to carry far more weight in the voting decision than do policy considerations when the latter are measured by either the index of policy preferences or the net count of policy-oriented comments. If policy concerns are conceptualized as relative distances from the candidates, however, we arrive at a different conclusion. Then, policy considerations seem to be just about as important as personal qualities evaluations in the determination of intended votes. The effect of partisanship is substantially lower.

An important variant of the models in Figure 4 is that formulated by Stokes (1966), which resembles Figure 4b except that a net count of party-related open-ended comments, conceptualized as a "short term force" like issues or candidate personalities, is used in place of long-term party attachments.<sup>6</sup> Making this

<sup>6</sup>Stokes weights unstandardized regression coefficients by the mean of voters' comments to estimate the net effect upon the electoral outcome of each type



Source: 1976 national election study, Center for Political Studies, University of Michigan.

Note: Symbols for disturbance terms are omitted for the sake of clarity.

Figure 4. Multivariate Recursive Models of Voting Behavior for 1976 with Policy Orientations, Partisanship, and Personal Qualities Orientations as Independent Variables

substitution decreases the party effect on the vote still further (see Figure 5). If we use this specification, the estimate for policy orientations (comments) is twice that for partisanship, while that for candidates' personal attributes increases to almost twice that for the policy measure and is nearly four times as large as the effect of party attachments.

### Two-Way Causation: Non-recursive Models of the Vote

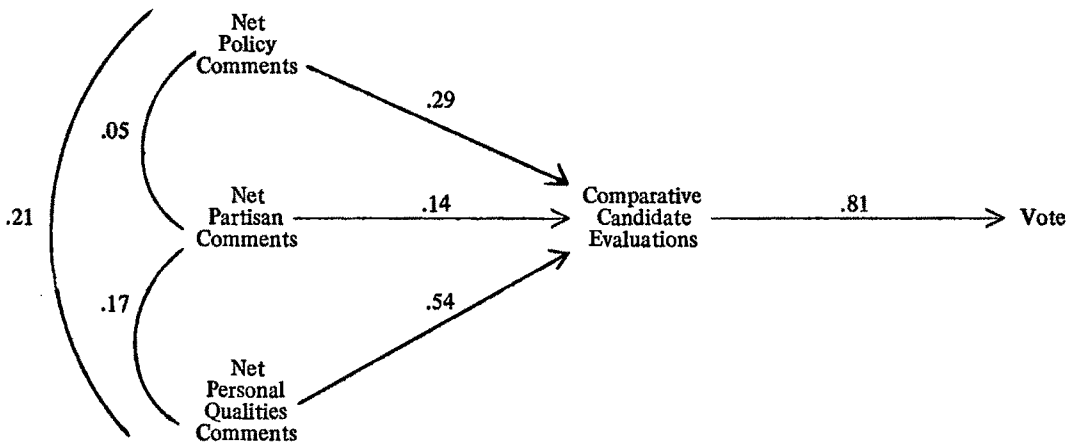
One problem with the preceding analyses is the multiplicity of conceptualizations and specifications. Undeniably, much of the confusion in debates over policy voting stems from the use of different policy measures, different specifications of equations, and different estimation techniques. Researchers simply talk past one another, treating findings as if they were inconsistent when they are not. In principle this problem is easily solved; it is merely necessary to be precise about what is being claimed, rather than talking in global terms.

A more fundamental problem with such analyses, however—a problem confounding most of the existing literature on voting be-

havior—is an incorrect assumption of one-way causation. The error of this assumption is most obvious in models employing policy proximity or distance measures. Clearly, citizens may tend to vote for the candidate to whom they feel closest on matters of public policy. Yet it seems to us quite possible—in fact likely—that citizens whose initial vote intentions may be formulated on non-policy grounds, can and do convince themselves that the candidates they prefer stand closer to them on the important policy issues. Just such a pattern is suggested by social-psychologists' studies of "projection" or "selective perception"; it also follows theoretically from the rational calculus of citizens operating with less than perfect information. Lacking other evidence, voters might reasonably infer that a candidate who agrees with them on most matters also would agree with them on any new policy matter that comes up. Thus perceived policy distances may be consequences as well as causes of intended votes. There is some empirical evidence, in Berelson et al., *Voting* (1954) and elsewhere, that overall evaluations of candidates do in fact affect perceptions of candidates' policy stands.

By this logic, two-way causation may have biased upward the estimate of the effects of policy distances upon vote intentions in Figures 2c, 3c, and 4c, and it might well bias upward the estimates of policy voting in any recursive model using proximity measures of this kind, even those which "control" for partisanship and other independent variables (Shapiro,

of short-term force. Throughout this article we report the importance of variables only in terms of standardized regression coefficients (*b*'s) and coefficients of determination (*R*-squares).



Source: 1976 national election study, Center for Political Studies, University of Michigan.

Note: Symbols for disturbance terms are omitted for the sake of clarity. Policy orientations, partisanship, and personal qualities evaluations measures all based upon net counts of open-ended comments.

Figure 5. Multivariate Recursive Model of Voting Behavior, 1976

1969; Repass, 1971; Aldrich, 1975; Miller et al., 1976).

It must be recognized, however, that much the same problem can occur in voting models which employ policy measures that do not explicitly include perceptions of candidates' stands. For example, recall that policy comments are drawn from responses to the CPS questions concerning party and candidate likes and dislikes. Implicit in each response is a judgment—that is, a perception—about where the parties or candidates stand on policies salient to the voter. Despite the open-ended, voluntary character of these responses, they could still be based upon rationalized and/or incorrect perceptions of the candidate or party positions. In other words, policy comments, too, may be consequences as well as causes of overall candidate evaluations, which again could lead to inflated estimates of the extent of policy voting (Stokes, 1966; Campbell, Converse, Miller and Stokes, 1960; Pomper, 1976; Kagay and Caldeira, 1975).

Moreover, we see no reason to assume that voters' policy preferences themselves are unaffected by general candidate evaluations. When an attractive candidate takes a strong stand on a matter of public policy, might he or she not persuade some supporters—especially those for whom the issue is of comparatively low salience—to bring their opinions into agreement? Or, less grandly, might not some of a candidate's supporters, in lieu of genuine opinions, give facile responses to policy questions corresponding to what they think their preferred candidate stands for? Because candidates are often ambiguous, it can be argued that such persuasion effects are likely to be less important than the effects of projection or selective perception, but we see no reason to exclude altogether the possibility that persuasion occurs.

In short, we are suggesting that all analyses which postulate policy variables to be recursive, uni-directional influences on candidate evaluations or vote choices may be overestimating the extent of policy voting. All the previously cited voting studies (except those by Jackson, Achen, and Markus), and many others not cited, are offenders in this respect. Even the pioneering causal model of Goldberg (1966) is entirely recursive and subject to this criticism, as is the recent work of Nie, Verba and Petrocik (1976).

At the same time, we maintain that the many researchers who do not use policy proximity measures or some other means for taking explicit account of perceived candidate positions are likely to *underestimate* the degree of relationship between policy orientations and

intended votes. There is no a priori way to tell for certain which direction of error predominates.

To extend this reasoning a bit further, there is no reason to consider subjective partisanship (i.e., "party identification") to be sacrosanct. It seems quite plausible to us that policy preferences—and relative policy distances from candidates—may be both causes and consequences of party loyalties. Just as opinion leadership by party figures might cause some citizens (even perfectly rational ones) to change their policy preferences, we strongly suspect that some voters' policy preferences—especially those touching on economic, social welfare or racial issues—have some effect on their choice of party in the first place. And, indeed, we would argue that policy distances from particular candidates, who may or may not take positions exactly along the lines of older party cleavages (thus reinforcing or weakening traditional policy profiles of the parties), are likely to affect the strength, if not the direction, of subjective partisanship during a given election campaign. Scholars interested in the relationship between partisanship and policy orientations have typically assumed that causation worked exclusively in one direction between the two, usually from party to issues. They may thereby have inflated their estimates of the effects of party identification (Campbell et al., 1960).<sup>7</sup>

Even Jackson's (1975) pathbreaking non-recursive model postulates that party affects policy preferences but not vice versa. Jackson (1975) and Markus (1976) also rule out any possible influence of candidate evaluations upon policy preferences.

Finally, we must consider the possibility that voters' party loyalties may both affect and be affected by comparative candidate evaluations or intended votes during a particular campaign period. While most researchers have conceptualized partisanship as a sort of blind habit, a relatively long-term exogenous factor affecting vote choice, we expect that to some

<sup>7</sup>A particularly misleading procedure is the "normal vote" technique. It does not attempt to give unbiased estimates of policy and party effects through regression analysis, but simply assigns the joint covariance of policy and party with the vote in two different extreme ways, so that the reader can choose between "long-term" and "short-term" effects. Except by chance, neither of these magnitudes correctly estimates the extent of either policy preferences or party upon the vote (or of party or policy each upon the other); indeed, the technique does not even identify boundaries around the maximum or minimum possible effects. See Boyd (1972).



(perhaps modest) extent, preferences based on a candidate's character or policy stands will affect the intensity—and at times even the direction—of party affiliations. It seems to us an overinterpretation of the temporal stability of partisan attachments to assume that they are altogether impervious to candidate choices in ordinary elections. Candidates, after all, reflect credit or blame upon the parties that nominate them. Thus the many studies of party identification as a determinant of candidate evaluations or voting choices, from *The American Voter* onward, may have overestimated its effect by ignoring the opposite possibility (Campbell et al., 1960). The Michigan Tradition dies hard.

Nor is *The Changing American Voter* immune to this criticism (Nie, Verba and Petrocik, 1976). Comparisons between groups or candidates or over time are also subject to error because the biases in estimates are not necessarily constant from group to group or candidate to candidate or year to year.

We have made the sweeping claim that virtually all studies of policy orientations, partisanship and the vote—certainly including the primitive efforts reported in Figures 2 through 5—are subject to simultaneity bias and are potentially quite misleading. It is fair enough to ask whether we have anything constructive to add to this work of destruction.

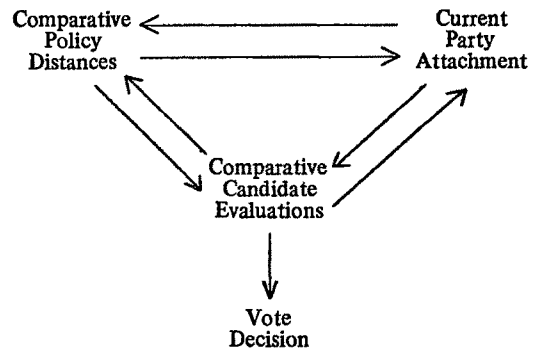
The 'most appropriate way to handle the problem, we would argue, is through the use of non-recursive, simultaneous equation models which explicitly allow for the possibility of causal processes operating in both directions between variables (Johnston, 1972; Theil, 1971; Hanushek and Jackson, 1977; Duncan, 1975).<sup>8</sup> We can begin to apply such techniques to the voting problem by specifying the central

set of variables which we believe to be mutually endogenous—that is, which reciprocally affect each other: comparative candidate evaluations, relative policy distance between the voter and candidates, and current subjective partisanship. We will continue to treat measures of reported vote as direct consequences of overall candidate evaluations.

The relationships postulated among these variables are diagrammed in Figure 6. Within the context of this model, we seek to investigate not only the effect of policy orientations upon the vote, but also five other processes of interest: namely, the reverse effect of overall candidate evaluations upon comparative policy distances, and the two-way linkages between current partisanship and policy distances as well as between party attachments and comparative candidate evaluations.

We cannot estimate any of the coefficients in Figure 6, as it stands, because the model is hopelessly underidentified. That is, there are only three empirically observable relationships among the central endogenous variables available to estimate the six causal processes of theoretical interest. Ordinary least squares regression is clearly not appropriate for estimating these causal paths since the required assumptions of independence between the regressors and error terms in each equation cannot be justified. To distinguish between the effects possibly operating in both directions between any two endogenous variables, we must bring additional information into the estimation process. The sort of information needed may be provided by a set of variables which are exogenous to the reciprocal processes specified in Figure 6—variables which can be assumed (on theoretical grounds) to be unaffected by any

<sup>8</sup>Time-series techniques offer an alternative way to sort out causal orderings, but even when time-series or panel data are available, they offer no panacea. In the first place, whether or not measurements are widely separated in time, there is danger in treating lagged variables as causes of current values of the same variables (e.g., party identification): error terms are often autocorrelated as a result of the omission of other factors (e.g., policy preferences) which affect both, and this biases estimates—usually upwards. To capture distinct stages in the development and change of cognitive elements, to ascertain what changes first, measurements might well have to be repeated almost instantaneously. Moreover, quasi-experimental measurements before and after exogenous events are likely to reveal more about the impact of those particular events than about the net mutual influences of different attitudes in the whole electoral process.



Source: Compiled by the authors.

Figure 6. Reciprocal Causal Paths in a Non-recursive Voting Model

endogenous variables, yet which have direct effects on some, but not all, of the endogenous variables (Fisher, 1966).

It is in the search for suitable exogenous variables that difficulties mount, for most of the pertinent social theory is either not very powerful or not universally accepted. The grounds for specifying that a given variable theoretically cannot affect or be affected by another are seldom overwhelming. The situation is worse than usual when one deals with psychological measurements or attitudinal variables, since practically any attitude might conceivably affect any other. There are times when we seem to be studying relationships between mush and slush.

Fortunately, however, it is possible to specify certain exogenous variables for our model with reasonable (and sometimes considerable) confidence, allowing us to identify several of the causal paths. Theory and past empirical research suggest, for example, that the party choices of voters' parents have a direct effect upon their own partisan proclivities, but do not affect their perceived policy distances or overall evaluations of the candidates, except insofar as they act through their own party affiliation. Parents' partisan commitments, therefore, can be specified as operating through uni-directional causal paths upon voters' partisanship only, and their direct effects excluded elsewhere in the model. This will help to identify the effects of voters' partisanship on their policy-oriented evaluations and their vote intentions.<sup>9</sup>

Similarly, we can with some confidence specify that voters' assessments of the character traits and personal qualities of the candidates (as measured by spontaneous responses to open-ended items about candidate likes and dislikes) should have direct effects upon overall comparative candidate evaluations, but should not be substantially affected by overall evaluations in return. In addition, we would argue that character and personality evaluations have no direct links to the voters' policy evaluations of candidates or to their current subjective

partisanship, but rather that these effects are transmitted indirectly through the more general overall evaluations of candidates. Thus, the assumption of a recursive relationship from personal qualities assessments to overall candidate evaluations (and the concomitant exclusion of direct effects elsewhere in the model) allows identification of the causal paths from candidate evaluations to both partisanship and comparative policy distances.

Our confidence in this specification depends heavily upon our use only of specific, spontaneous comments about candidates' characteristics, which are far more likely to be causes than effects of candidate evaluations.

It is somewhat more difficult to specify variables which affect voters' comparative policy distances without also affecting the other endogenous variables in the system. We begin, however, by taking advantage of the fact that a number of the voters' background or demographic attributes—factors such as race, sex, age, income and education—affect policy proximities by initially affecting the voters' policy preferences. There is little question that voters' background characteristics are truly exogenous to the electoral process. But the question arises whether they also affect partisanship or general candidate evaluations directly, or whether their effects are transmitted entirely through perceived policy distances. We will assume the latter in our initial model and conduct some tests on the effects of that assumption below.

We have, now, a non-recursive model in which all hypothesized causal paths are identified. The following structural equations are diagrammed in Figure 7.

$$\begin{array}{rcl} \text{Comparative} & & \text{Comparative} \\ \text{Candidate} & = b_1 & \text{Policy} + \\ \text{Evaluations} & & \text{Distances} \\ & & \\ & \text{Current} & \text{Personal} \\ & b_2 \text{ Party} & = b_3 \text{ Qualities} + u. \\ & \text{Attachment} & \end{array} \quad (1)$$

$$\begin{array}{rcl} \text{Comparative} & = b_4 & \text{Comparative} & \text{Current} \\ \text{Policy} & \text{Distances} & \text{Candidate} & \text{Party} + \\ & & \text{Evaluations} & \text{Attachment} \\ & & & \\ & b_6 \text{ Race} + b_7 \text{ Age} + b_8 \text{ Sex} + b_9 \text{ Education} + & & \\ & b_{10} \text{ Income} + u. & & \end{array} \quad (2)$$

<sup>9</sup>Since parents' party is measured in the CPS surveys by the respondent's report, it is conceivable that the reported party affiliation of parents is affected by respondents' partisanship through a conscious or unconscious desire to have it be the same (or different), or simply as an aid to faulty memory. Independent studies based on interviews with parents, however, indicate that the bias in the reports is probably not very serious for our purposes (Jennings and Niemi, 1968). This is particularly true since other proximate influences on partisanship reduce the importance of parents' party in our models.

$$\begin{aligned}
 &\text{Current Party Attachment} = b_{11} \text{ Comparative Policy Distances} + \\
 &b_{12} \text{ Comparative Candidate Evaluations} + b_{13} \text{ Father's Party Attachment} + \\
 &b_{14} \text{ Mother's Party Attachment} + u. \quad (3)
 \end{aligned}$$

Simply providing for identification of path coefficients does not guarantee adequate specification of the causal model, however, and we believe that additional exogenous variables must be included to overcome biases of a different sort. When important independent variables are omitted from any equation of the model, coefficients for the included variables will still be biased if, for whatever reason, the omitted and included variables are correlated. Several such variables are suggested by past research devoted to the individual equations of our model.

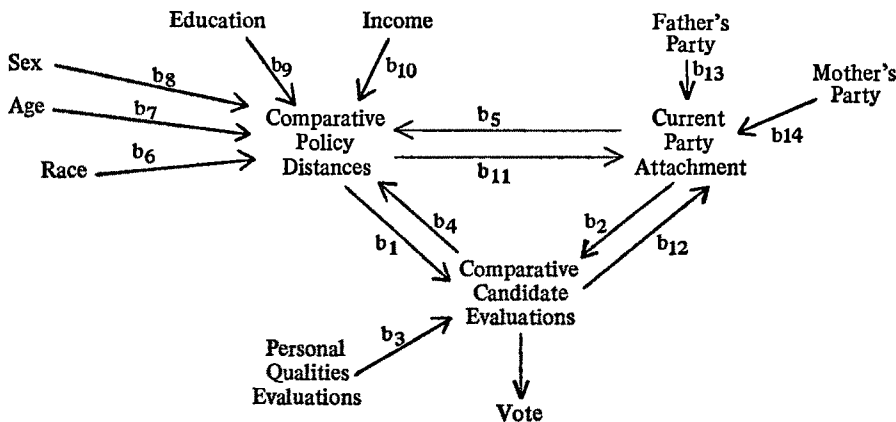
The literature on party identification points to five additional potential influences upon current subjective partisanship: the voter's sense of long-term, traditional involvement with a particular party; degree of consistency in party voting for president; religious preference; region of residence; and general ideological leaning. Some of these factors should carry greatest weight among voters whose party loyalties differ from those of their parents or who were not influenced in any partisan direc-

tion by their parents; others mediate parental influence.

For many voters, both the direction and strength of current partisanship may be expected to depend in part upon their degree of involvement with their chosen parties, considered either as reference groups to which loyalty is owed, or as information sources providing interpretations and evaluations of the political environment. We measured this sort of involvement by a net count of positive and negative comments which focus only on parties as traditional referents, wholly excluding both policy and candidate personality connotations: "I've just always been (or voted as) a Democrat," or "I like Nixon just because he's a good Republican."

An even stronger factor affecting current partisanship may be our measure of presidential voting consistency, which combines information about citizens' regularity of voting in presidential elections with data on whether they always supported a particular party for president.

In order to take account of socialization or information sources outside the family for those voters with neither a strong sense of partisan tradition nor a history of regular party voting, we included variables which classify citizens according to their region of residence (10-state South versus all other states) and their religious preferences (Catholics, Jews, Fundamentalist Protestants and those with no religious preference versus all others). Finally, for those voters whose partisanship derives from



Source: Compiled by the authors.

Note: Disturbance symbols have been omitted for the sake of clarity.

Figure 7. Non-recursive Voting Model with Incomplete Structural Equations

their view of the parties as purveyors of more or less coherent social and economic programs, we included a simple trichotomous classification of their ideological leaning based on their self-placement on the CPS closed-ended liberal/conservative scale. Rather than attempt to classify voters on this indicator with excessive (perhaps artificial) precision, we collapsed the seven points into a trichotomy of "pro-left," "pro-right" and neutral positions, combining voters who placed themselves at the mid-point with those who did not place themselves on the scale at all, as ideologically neutral.

To the five background variables exogenous to Comparative Policy Distances, we added a single additional measure—the same ideological classification included in the partisanship equation. Inclusion of so simple a classifying index allows us, without undue worry about reciprocal causation, to estimate the extent to which voters' philosophical orientations not rooted in racial, sexual or socioeconomic differences may affect policy evaluations of the candidates. While attitudes are always suspect when used as exogenous variables, the high salience and stability of voters' self-classification as liberal or conservative to the moderate number of respondents who embrace such a label bolsters this specification. The direction of such a stance is considerably less subject to electoral influence than is the intensity of commitment.

The special nature of the Carter candidacy in 1976 raised the possibility of two additional factors which might have independent effects on comparative candidate evaluations, namely Carter's southern origins and his highly publicized religious outlook. Therefore, we included in the equation for overall candidate evaluations two more exogenous variables. The first is the same indicator of region of residence which appears in the partisanship equation; the second is the dichotomous classification of citizens' religious preferences which also appears in the partisanship equation.

With the addition of these exogenous variables, our model may be described by structural Equations (4) through (6) (diagrammed in Figure 8):

$$\begin{array}{lcl}
 \text{Comparative} & & \text{Comparative} \\
 \text{Candidate} & = b_1 & \text{Policy} + \\
 \text{Evaluations} & & \text{Distances} \\
 \\ 
 b_2 & \text{Current} & \text{Personal} \\
 & \text{Party} & \text{Qualities} + b_4 \text{ Region} + \\
 & \text{Attachment} & \text{Evaluations} \\
 \\ 
 b_5 & \text{Religion} + u. & 
 \end{array} \quad (4)$$

$$\begin{array}{lcl}
 \text{Comparative} & & \text{Comparative} & & \text{Current} \\
 \text{Policy} & + b_6 & \text{Candidate} & + b_7 & \text{Party} + \\
 \text{Distances} & & \text{Evaluations} & & \text{Attachment} \\
 \\ 
 b_8 & \text{Race} + b_9 & \text{Age} + b_{10} & \text{Sex} + b_{11} & \text{Education} + \\
 \\ 
 b_{12} & \text{Income} + b_{13} & \text{Ideology} + u. & & \\
 & & & & (5) \\
 \\ 
 \text{Current} & & \text{Comparative} & & \\
 \text{Party} & = b_{14} & \text{Policy} & + & \\
 \text{Attachment} & & \text{Distances} & & \\
 \\ 
 b_{15} & \text{Comparative} & & \text{Father's} & \\
 & \text{Candidate} & + b_{16} & \text{Party} & + \\
 & \text{Evaluations} & & \text{Attachment} & \\
 \\ 
 b_{17} & \text{Mother's} & & \text{Traditional} & \\
 & \text{Party} & + b_{18} & \text{Party} & + \\
 & \text{Attachment} & & \text{Involvement} & \\
 \\ 
 b_{19} & \text{Partisan} & & & \\
 & \text{Voting} & + b_{20} & \text{Region} + b_{21} & \text{Religion} + \\
 & \text{History} & & & \\
 \\ 
 b_{22} & \text{Ideology} + u. & & & (6)
 \end{array}$$

Since each equation of the model includes overidentified paths, we estimated the standardized coefficients for the model using two-stage and three-stage least squares programs.<sup>10</sup> These estimates are also presented in Figure 8.

To us, the most striking aspect of the estimates in Figure 8 is the extent to which they differ from the corresponding coefficients obtained using simple recursive models. Comparative policy distances now appear to be the strongest single factor affecting intended votes, with a standardized, partial (3SLS) regression coefficient of .44. This estimate is greater than that produced by any of the multivariate recursive models examined above; it is roughly comparable in magnitude to the bivariate coefficient for the net policy comments measure (recall Figure 2b above)—although it would be wrong to conclude that recursive models using comment counts necessarily give good estimates of policy voting generally, in all elections.

This major role played by policy orientations in the voting decision is, of course, very much in the spirit of rational-man spatial models. It is noteworthy that our estimation

<sup>10</sup>The Statistical Analysis System (SAS) software package was used. The third stage offers improvements in the efficiency of estimates from the previous stage by taking account of the correlations among the equations in the system (Johnston, 1972). In this case, 3SLS estimates differed little from those at the second stage. All variables were standardized before computations were made.

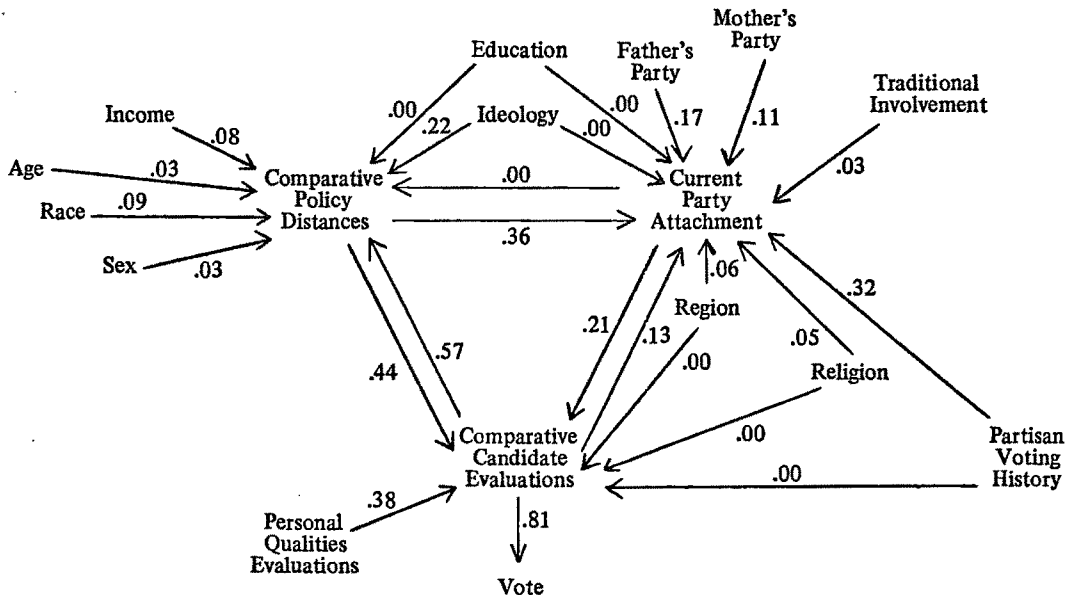
procedure, which takes explicit account of theory by using policy proximity measures, reveals a greater extent of policy voting than do most other methods. At the same time, however, evaluations of the candidates' personal and leadership qualities run a close second in importance, with a standardized coefficient of .38 (about equal to the estimate derived from the recursive model depicted in Figure 4c). Indeed, we cannot be sure that the policy effect was significantly greater than that of candidate personality. Spatial models in their narrowest form, postulating exclusively policy-oriented voting, are plainly inadequate. But we would argue that this does not reflect badly on the citizenry. In a presidential system, rational individuals must pay attention to leadership characteristics, if only to ensure that their favored candidate is competent to carry out the promised policy stands.

In 1976, citizens' party attachments had much less direct influence upon vote intention than either policy orientations or evaluations of personal qualities. The coefficient of .21 is lower than that for any of the recursive estimates presented above (except that for the attenuated partisan comments measure in Figure 5). Party loyalties, in other words, had

some independent effect on the vote—again contrary to narrow notions of voter rationality, but not to a broader conception. (Surely citizens are well advised to view the parties to some extent as governing teams, with records of past performance which bear upon future prospects. What V. O. Key called the “standing decision” to support one or another party—subject, of course, to modification in the light of experience—is an efficient and generally reliable aid in voting.) Yet our estimate indicates that partisanship was by no means a straitjacket. It was clearly not the most important factor affecting the vote, as has been alleged of the 1950s and early 1960s.

Despite the special features of Carter's campaign, in 1976 the estimated coefficients for both region and religion did not differ significantly from zero. We suspect that this result follows from the fact that evaluations of personal qualities and current partisanship already take account of such effects. With these variables included in the candidate evaluations equation, the region and religion variables are superfluous.

Two important findings, which can emerge only from a non-recursive model like ours, concern the reciprocal effects of both intended



Source: 1976 national election study, Center for Political Studies, University of Michigan.

Note: Disturbance symbols and inter-correlations among exogenous variables have been omitted for the sake of clarity.

Figure 8. Full Non-recursive Voting Model with Overidentified Structural Equations, 1976

votes and policy distances upon current partisanship. When all of the customary influences on the formation and maintenance of party attachments are allowed for, intended votes still show a modest independent effect (.13) on voters subjective partisanship. In 1976, then, partisanship was not an exogenous influence on the vote, impervious to other electoral stimuli. Instead citizens apparently alter their party loyalties when the parties nominate especially desirable or undesirable candidates, just as sensible voters would be expected to do. By ignoring this effect of overall evaluations of the presidential candidates upon partisanship, many scholars have overestimated the effects of party affiliation upon voting decisions.

What may be more surprising to some is the much stronger effect (.36) of comparative policy distances from the candidates upon party attachments. Conceptually, it seems logical enough that voters who see their policy interests served much better by one candidate than another might be inclined to consider themselves supporters of that candidate's party. The question arises, however, why such a judgment on policy grounds would not be translated into an overall positive evaluation of (i.e., an intention to vote for) that candidate, and only then go on to influence partisanship. The answer seems to involve the fact that policy judgments form only one of many independent dimensions of candidate evaluations; preference for one candidate on policy grounds does not guarantee a favorable evaluation overall. In fact, in 1976, over 15 percent of the survey respondents fell at the neutral or indifference point on our measure of comparative candidate evaluations. While 65 percent were classified as being closer to Carter on policy matters (35 percent closer to Ford), only 33 percent preferred Carter's personal and leadership qualities and 40 percent preferred Ford's. Thus even when comparative policy distances did not automatically lead to vote intentions, they appear to have influenced voters' feelings of attachment to the political parties.

For a given election, the overall magnitude of either of these endogenous effects on partisanship would depend, of course, upon citizens' partisan attitudes before the start of the campaign and upon their various evaluations of the nominees. The effects are not overwhelmingly large; we would not argue from these findings that major shifts of pre-existing party allegiances result from a campaign like that of 1976. The several variables which we specified as exogenous to current party attachment have significant (and in some cases, sizeable) coeffi-

cients, clearly indicating the presence of powerful stabilizing influences. The effects of parental socialization and of consistency in party vote for president, for example, no doubt serve to anchor some voters against the ebb and flow of campaign developments. Yet change in party attachment, whether through the weakening or strengthening of former ties, or by the creation of a sense of partisanship among new voters, clearly followed from evaluations of campaign stimuli. This finding, we hope, will help lay to rest the notion that partisanship can be treated as an unmoved mover in the analysis of voting behavior.

Perhaps the most theoretically important of all our estimates is the strong effect (.57) of overall candidate evaluations upon perceived policy distances. The existence of such an effect squarely controverts the assumptions of spatial models, which specify that voting decisions are the result of calculations of policy proximities or distances from the candidates, and that the policy preferences and perceptions of candidates' stands used in these calculations are independently arrived at. The evidence is clear that influence runs both ways: voters' evaluations of Carter's and Ford's policy stands were strongly conditioned by their overall judgments about the candidates. Those who generally favored Carter, let us say, tended to locate themselves and Carter close together on the policy scales; those who disfavored a candidate tended to place themselves and that candidate farther apart. This finding goes against the fixed-preferences and perfect-information assumptions widespread in economists' views of politics (and, for that matter, economics as well). It calls for considerable rethinking of some notions of electoral democracy.

At the same time, it would be quite erroneous to conclude that we have found voters to be irrational or deficient with respect to some democratic ideal. In the first place, the influence of candidate evaluations upon perceived policy distances from candidates encompasses a complex bundle of processes (projection, persuasion, rationalization), in which either voters' policy preferences or their perceptions of candidate stands are affected by their evaluations of the candidates. Some of these processes are useful or even essential to the workings of democracy; we cannot at present sort out which are operating. (To distinguish the effects on perceptions from those on preferences would require a four-equation model with two complicated nonlinear equations.) In the second place, most if not all of these processes are best understood as reactions by rational citizens

to the problems of obtaining political information. To the extent that some reactions interfere with electoral democracy, much of the blame must fall upon the political environment and the kinds of information made available (or lacking), rather than upon the voters themselves.

It is quite possible—and compatible with our findings—that some voters who have no information about the candidates' policy stands, but who have developed a preference for one candidate (based on party ties or personal attributes) project their own preferred policy positions onto their favorite candidate, especially when pestered to answer survey questions about where candidates stand. In addition, there are no doubt voters for whom policy considerations are of sufficiently low salience that they blithely adopt as their own the policy prescriptions of whichever candidate they have come to prefer for non-policy reasons. Yet persuasion (i.e., a candidate's influence upon policy preferences) can take a much less casual form—a form equally compatible with our findings—when citizens listen to information and argument about policy and are genuinely convinced by a candidate they trust. In a complicated world of imperfect information, rational citizens must be open to persuasion.

Moreover, the existence of strong effects of policy orientations upon candidate evaluations suggests that few voters, if any, rely exclusively upon simple rationalization in arriving at their final evaluations of the presidential contenders' policies. It is much more likely that a given voter will have had at least some realistic notion of his or her comparative distances from the candidates which conditioned more general evaluations in the first place. Lacking perfect information, such voters might then combine what data they have on policy distances with party cues and judgments about the candidates' personal attributes into a preliminary comparative evaluation of the nominees, which they then may use to infer their ultimate net distance from the candidates over a wider range of policy areas. In this light, what at first seemed to be rationalization may in fact be rationality.

Again, we would emphasize that if voters make foolish and easily avoidable errors about candidates' policy stands (and no evidence yet establishes this to be so), this probably results as much from the obscurity and ambiguity of what candidates say as from limitations of citizens' cognitive processes (Page, 1978, Ch. 6).

All the findings we have discussed so far pertain to the 1976 presidential election. Both

as a check on our methods and for its own substantive interest, we present in Figure 9 comparable estimates for 1972. All measures were constructed in precisely the same way as for 1976, and the series of alternative specifications used for testing purposes were identical.

Comparison of Figures 8 and 9 reveals many similarities in overall structure, especially in the effects of exogenous variables. This increases our confidence in the model and the estimation procedure. At the same time, the comparison uncovers some sharp differences, which are readily interpretable in terms of differences in the electoral environment. Plainly the Carter-Ford contest and the Nixon-McGovern campaign differed in ways which aroused dissimilar reactions among the voters.

As in 1976, we find in 1972 clear evidence of reciprocal causation between policy orientations and overall candidate evaluations. Many citizens voted on policy grounds, but many also were influenced in their assessment of policy distances by their overall evaluations of the candidates. As in 1976, the effect of candidate evaluations upon perceived policy distances (.81) appears slightly stronger than the effect in the opposite direction (.76), but this difference was much smaller in 1972 than in 1976 and is not statistically significant. No doubt the projection or rationalization of issue stands was relatively more difficult in the "clear choice" setting of 1972.

What stands out most strongly is the much greater magnitude of both roles of policy orientations (as both independent and dependent variables) in 1972. The Nixon-McGovern election was truly an issue-oriented affair; no other factor approached the importance of public policy orientations in affecting voting decisions. Indeed, this feature of the election was so marked that it can be discerned no matter how crudely the data are analyzed, as when we replicated for 1972 various simple recursive models. Yet, at the same time, the clarity of the choice on policy grounds evidently did not prevent a very substantial amount of alteration in perceptions of candidate stands and/or policy preferences in response to feelings about the candidates.<sup>11</sup>

The impact upon votes of citizens' evalua-

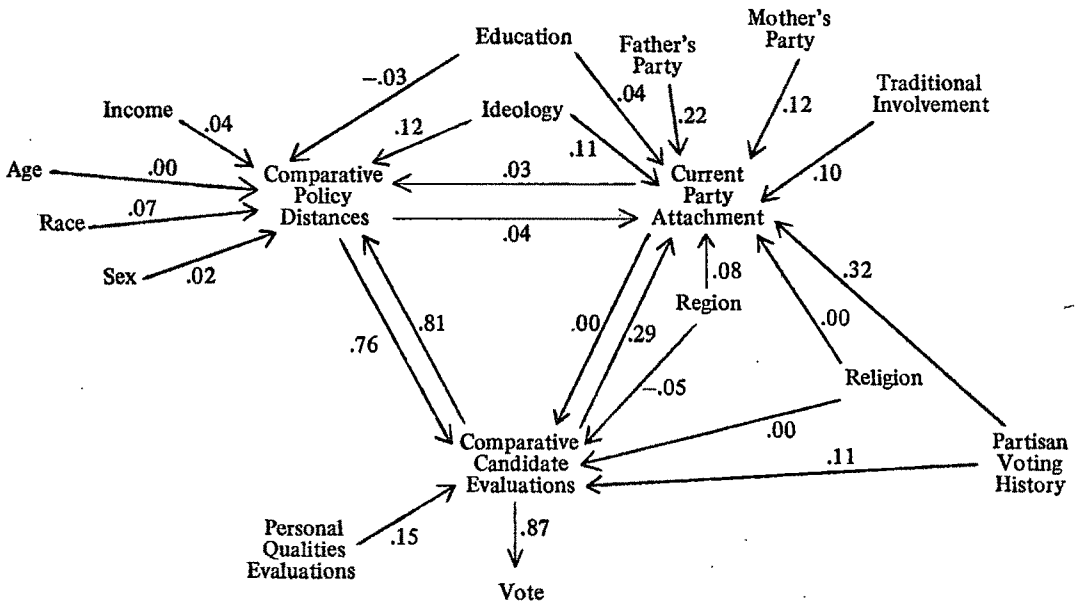
<sup>11</sup>In point of fact, the clarity of the choice in 1972, like that in 1964, actually left much to be desired. Nixon's policy utterances were vague throughout, and McGovern gradually retreated into ambiguity after taking unusually specific stands on the defense budget, income redistribution, and the like (Page, 1978).

tions of personal qualities was surprisingly low (.15) in 1972, suggesting that explanations of the vote which emphasize McGovern's allegedly poor judgment and personal instability as important direct factors are probably somewhat wide of the mark. According to our estimates, policy distances had an effect over five times as great as did reactions to the candidates' personal attributes. Still, this finding is not inconsistent with the possibility that the outcome of the election (as opposed to the total variance in voting decisions) was heavily influenced by judgments about the candidates' personal and leadership qualities. Calculations of Stokes-type net effects, which take account of the strongly anti-McGovern drift of this factor, indicate that it was quite important (Popkin et al., 1976).

Among the most noteworthy aspects of Figure 9 is the nearly total absence of any effect of party attachments upon the other two endogenous variables. Our estimates reveal that in 1972—quite unlike 1976—party loyalties played no part in the formation either of voting decisions or perceptions of closeness to the candidates on policy matters. McGovern dramatically dissociated himself from the Democratic party's mainstream; and, by the same

token, the central core of the Democratic party abandoned McGovern to his own devices. Party *per se* had no independent effect at all on the vote. Furthermore, the absence of a significant effect of policy distances upon partisanship, together with the very strong impact of policy distances upon intended vote, indicates that the policy choices around which voting decisions revolved (the Vietnam War, urban unrest, campus disturbances, alternative life styles) cut across the grain of older party cleavages.

There was, on the other hand, apparently some effect of intended votes upon partisanship. That is, although party loyalties did not affect votes, intended votes did affect partisanship—presumably because some Democratic defectors to Nixon felt their Democratic affiliations to be weakened. There are, however, some doubts about this finding. In spite of the moderate size of this coefficient (.29), the conventional significance test (which is asymptotically valid for two- and three-stage least squares estimation) indicates that it is not significantly different from zero at the .05 confidence level. Examination of the intermediate two-stage calculations reveals a high degree of collinearity between the “decontaminated”



Source: 1972 national election study, Center for Political Studies, University of Michigan.

Note: Disturbance symbols and inter-correlations among exogenous variables have been omitted for the sake of clarity.

Figure 9. Full Non-recursive Voting Model with Overidentified Structural Equations, 1972



versions of the policy distance and candidate evaluations variables. This results in high standard errors of estimates for the respective coefficients (i.e., lowers their precision), which substantially raises the difficulty of disentangling the independent effects for these two variables. Since we presently lack any satisfactory way to deal with multicollinearity in the second stage of two-stage least squares, it is necessary to reserve judgment on the significance or insignificance of the 1972 impact of intended vote upon partisanship.

Among the exogenous factors in 1972, there are also some differences from 1976 related to the nature of the candidate pairings in those two years. For the most part, however, the exogenous variables—especially those which represent stable characteristics of respondents—exerted roughly comparable amounts of influence in both elections. In general, the overall structural similarity between the estimated models for 1972 and 1976 is an encouraging sign that the estimates are correct and not subject to vagaries of the estimation technique or arbitrary variations with independent data sets.

Thus the variation in the effects of ideological orientations is easily explained in terms of the differences in the kinds of policies at issue during the two campaigns. In 1976, policy debates tended to line up much more with older, New Deal notions of liberalism and conservatism than was the case in 1972. Our measure of ideological leaning (which relates most closely to voters' attitudes on New Deal type issues) therefore had a greater effect on policy distances in 1976 than in 1972. In addition, whereas in 1972 voters' ideological orientations affected both policy distances and party attachments directly, in 1976 the effects of ideology were passed entirely through policy distances, affecting partisanship indirectly via the path from policy distances to partisanship.

Changes in the pattern of effects for partisan voting consistency are of a similar nature. In 1972, party attachments had no discernible effect on comparative candidate evaluations. Yet the more consistently citizens had voted for the same party for president, the more likely they were to prefer that party's candidate to the opposition in 1972. Presumably it took a rather special kind of partisan attachment rooted in a firm history of voting support to make Democrats (independently of other factors) prefer McGovern to Nixon. In 1976, however, we find that the direct path from voting history to comparative evaluations no longer functioned. Again, controlling for other factors, voting history directly affected only

current party attachments, with candidate evaluations being affected only indirectly.

Finally, it seems to us that the differences in the effect of personal qualities evaluations between the elections is largely due to the great differences in the intensity of the effects of policy issues in the two campaigns. Policy distances were so overwhelmingly important in 1972 that judgments about personal attributes were left with relatively little independent explanatory power.

### Conclusions

The clearest implications of our findings concern errors inherent in recursive voting models. In the first place, researchers who rely on single-equation techniques simply fail to reproduce faithfully the underlying complexity of the electoral decision process. Beyond this, even if multiple equations are specified in elaborate hierarchical recursive models, the estimated coefficients are still subject to simultaneity bias.

This defect is most damaging when recursive models are used with variables (such as the policy distance measures) which are especially likely to have been influenced by the very factors of which they are presumed causes. Our results suggest that the use of such measures should be restricted to non-recursive simultaneous equation models and that only those estimation techniques appropriate for such models should be applied. Yet practically all studies of issue voting, party identification and the like have specified uni-directional causal relationships without having theoretical justification for doing so. Our findings indicate that the degree of error introduced by mis-specification and simultaneity bias in such models can be quite substantial.

Our most important empirical discovery involves the reciprocal causal paths between policy orientations and overall candidate evaluations. In both 1972 and 1976 there was a high degree of policy-oriented voting—considerably more than is revealed by some less powerful analytical techniques. The identification in both years of a substantial effect from intended votes to policy distances, as well, confirms what many theorists have suspected for some time: policy-based evaluations of presidential candidates are endogenous to the electoral process.

We have argued that the effect of intended votes upon policy distances is consistent with individual rationality, since it may be perfectly rational to be persuaded by a favored candidate's policy stands, or even to infer a candidate's positions on policies (and, hence, one's

degree of proximity to them) from other characteristics. We have pointed out, however, that this is inconsistent with simple spatial models of voting behavior, in which preferences are assumed to be fixed, and perceptions of candidate stands are assumed to vary only randomly (if at all) among the electorate.

In addition, the estimates from our two data sets imply that the effect of partisanship on the vote varies considerably across elections, depending largely upon the nature of the candidate pairings and the extent to which current policy issues conflict or coincide with established party cleavages. When the policy debates of a campaign are such that the parties are seen to have relatively distinct and internally coherent positions, and when the presidential nominees are perceived as being reasonably typical representatives of their respective parties' interests and stands (as was largely the case in 1976), then voters' current party attachments may both affect and be affected by policy orientations and overall candidate evaluations. When these conditions are not met (and surely 1972 was a quintessential case where they were not), partisanship is isolated from the electoral process.

Further, when party loyalties do enter in, they do not function purely as fixed determinants of the vote; those loyalties can themselves be affected by attitudes toward the current candidates. Even short of major realignments, party affiliations are effects as well as causes in the electoral process.

We have contended that non-recursive simultaneous equation models are necessary in order to eliminate substantial, systematic biases from estimates based on cross-sectional data. At the same time, we must concede that the modeling procedures and estimating techniques we advocate are by no means free from specification problems. One of the chief difficulties is locating variables in existing data which are genuinely exogenous—that is, which are truly free from reciprocal influence by their target endogenous variables, and which can be excluded on persuasive theoretical grounds from having direct effects on certain other endogenous variables. In the search for plausible and identifiable specifications, researchers must be quite careful to state their theoretical justifications, and also to make clear, by the use of sensitivity testing, just what the consequences of alternative specifications would be.

Simultaneous equation analyses are, to an important extent, dependent for their success upon the decisions of those who design survey instruments and collect the data. If important variables are not measured or are measured

poorly, one cannot place much confidence in empirical findings. By "important variables," we mean not only those of major theoretical and substantive interest, but also potential exogenous variables, some of which may be of little interest in themselves but which have the theoretical properties necessary to assist in estimation.

Lacking tailor-made measures, we have tried to exercise great caution in the specification of exogenous variables—choosing wherever possible items on which voters had fixed characteristics, or had reached established values prior to the campaign under study. Where this was not possible, e.g., in the cases of evaluations of candidates' personal qualities or of voters' ideological leanings, we sought constructs which approached the theoretical ideal as closely as possible. Our measure of personal qualities evaluations, for example, was operationalized so as to minimize the possibility that individual voters' scores would be affected by other factors, while maximizing the probability that they would reflect the true extent to which personal qualities evaluations formed the basis of the voter's electoral decision. We doubt that this construct perfectly satisfies all of the theoretical requirements of an ideal exogenous variable. We believe, however, that it is a defensible specification, and that any biases introduced by this type of measure will be small indeed when compared with those resulting from single equation or recursive estimation. The estimates of effects from candidate evaluations to policy distances and partisanship, which depend heavily upon this specification, may be slightly inflated, but we are satisfied that they are very nearly correct.<sup>12</sup>

Future presidential elections will no doubt be accompanied by voter surveys of varying

<sup>12</sup>Extensive sensitivity testing was conducted using a series of alternative specifications for each of the three equations. For example, personal qualities evaluations were tested for direct effects in both the partisanship and the policy distances equations, with negative findings. Similar testing showed that the potentially controversial specification of some demographic variables as affecting perceived policy distances but not (directly) partisanship, has surprisingly little effect upon the estimates among endogenous variables. In fact, so long as parental partisanship was included in the equation, none of the demographic variables was found to have significant effects upon current party affiliations. In the same vein, when our predictors of partisanship were inserted in the equations predicting overall candidate evaluations and policy distances, only those specified in our final models showed significant direct effects.

purposes and designs. The opportunities for further developments in non-recursive modeling remain open on a good many fronts. The refinement of existing exogenous measures, the use of panel studies and the incorporation of quasi-experimental designs in future surveys all offer possibilities for improvements in the analytical techniques advocated here. Even limited success in this direction will add far more to our understanding of voting behavior than will repetition of error-ridden, mis-specified models.

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# Political Participation and Government Responsiveness: The Behavior of California Superior Courts

JAMES H. KUKLINSKI  
*Indiana University*

JOHN E. STANGA  
*Wichita State University*

*Students of democratic politics have long been concerned with the role of political participation in linking government and the people it serves. Whereas participation is generally defined in terms of voting, this article defines participation as the communication of citizen preferences to public officeholders. We show that aggregate sentencing decisions of California superior courts changed to reflect more closely prevailing public opinion after a large percentage of the populace expressed their preferences on a marijuana issue. The fact that members of California superior courts are seemingly immune from any effective electoral sanction serves both to underline the importance of this form of participation to a responsive system of government and to caution against conceiving of the participation-responsiveness relationship only in terms of punitive electoral devices.*

In their major work on political participation in the United States, Verba and Nie state (1972, p. 300), "Responsiveness is what democracy is supposed to be about and, more specifically, is what participation is supposed to increase." Although this theme is prominent among students of American politics, a satisfactory answer to the question, "Does participation make a difference?" is not apparent, in part because researchers have tended to equate participation with voting. It is natural that scholars should focus their attention on the most common mode of participation, but concentration on voting also seems to reflect an underlying assumption that the predominant if not single purpose of participation is to exert pressure on public officials. Mass voting presumably sustains politicians' fear of being tossed out of office, which in turn ensures that

government will remain sensitive to the citizens it serves (cf. Schumpeter, 1942). Widespread acceptance of participation as coercion is patent: most research on government responsiveness has focused on legislative bodies, whose members are popularly elected to short terms of office.

Verba and Nie (1972, p. 47 ff.) remind us that participation can serve a second and distinct purpose: to communicate information about citizen preferences. Here efforts to influence government take the form of increasing officials' awareness of the public's wishes. Whereas electoral coercion involves applying diffuse pressure without specifying *how* leaders should respond, citizen communication of preferences provides relatively precise guidelines within which officials are to make policy decisions.

As a research matter, considering participation as a communication activity has two positive consequences. First, it underscores the often-overlooked point that responsiveness is best construed as a *configurative* phenomenon, involving officials and the mass public simultaneously. To conclude that government is unresponsive when the public is apathetic or noncommunicative may be ill-conceived, or at best trivial. Even government leaders dedicated to carrying out the public will would be hard put to do so if that will were unknown. Second, interest is extended to non-elected officials. Study of the participation-responsiveness nexus can include the large proportion of government officials (many bureaucrats and judges, for example) who are isolated from any real electoral sanction but who play important, often

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decisive, roles in the policy-making process.

Only recently have researchers begun to look at the relationship between this second function of participation and government responsiveness (Verba and Nie, 1972; Hansen, 1975, 1978; Stone, 1976; Karps, 1978). They have measured, typically, the congruence of mass public and elite *attitudes*, from which *behavior* can only be *inferred*. The use of data collected at a single point in time, moreover, precludes separating agreement resulting from simple elite-mass sharing of policy attitudes and that due to the actual response of officeholders. In short, the causal link that the authors seek to identify is remote at best.<sup>1</sup>

The data reported in this article provide a somewhat unique opportunity to consider more directly than earlier work whether participation, henceforth defined as the communication of policy preferences, encourages public officials to respond. As a corollary to this basic question, we shall speculate about the centrality of electoral coercion to the relationship. To find that the communication of preferences inspires government action, even in the absence of direct electoral challenge of officials, would underscore the connection between the availability of information about citizen preferences on the one hand and the responsive quality of our representative institutions on the other.

#### Data, Method, and Level of Analysis

When participation is viewed as an instrumental act through which citizens try to influence government, a systemic or "macro" perspective is of considerable value. This perspective not only allows the researcher to determine whether aggregate communicated preferences affect the collective decisions of a government decision-making body, but it also accords with Pitkin's explication of responsiveness (1967; see also Prewitt and Eulau, 1969) as an emergent property of a complex system of interactions among citizens and government officials.

In keeping with this perspective, we have chosen the California superior court as our unit of analysis (Cook, 1967, pp. 65-66). The superior court can be regarded as consisting not only of the judges who sit on the bench,<sup>2</sup> but also of the police, parole officers, prosecutors, and defense attorneys whose interactions shape

its final decision. Because these lower courts are legally defined as policy-making bodies having general jurisdiction coterminous with the state's counties, it is possible to ask whether the decisions of the courts are related to variations in public preferences across the counties, and whether this relationship, in turn, is affected by citizen communication of policy preferences.

These questions require data on both the public's preferences and judicial decisions within a specific policy area. We chose as a measure of opinion aggregate voting returns on an initiative ballot proposition proposed during the 1972 general election to remove criminal penalties for the personal use of or preparation for personal use of marijuana. As presented to the voters, the initiative reads:

[This marijuana proposition] removes state penalties for personal use. Proposes a statute which would provide that no person eighteen years or older shall be punished criminally or denied any right or privilege because of his planting, cultivating, harvesting, drying, processing, otherwise preparing, transporting, possessing or using marijuana. Does not repeal existing, or limit future, legislation prohibiting persons under the influence of marijuana from engaging in conduct that endangers others (State of California, 1972, p. 31).

For each county we use the percent support for the marijuana issue.<sup>3</sup> The higher the percentage, the more liberal is a county toward the personal use of marijuana.

In addition to providing a valid measure of public preferences, the initiative vote serves our research objective in two ways. First, it is an initiative. Members of the public both brought the issue and chose to express themselves on it. Over 90 percent of those voting in the general election registered their opinions on the marijuana question. Second, since the vote itself represents a participatory act, we can ask whether participation leads to government response by comparing the levels of policy agreement before and after the communication of preferences.

We therefore computed a measure of judicial policy for each of three years—1971, 1972, and 1973. The policy measure is the mean sentencing severity score of all decisions involving possession of marijuana convictions. It is based on a scale first proposed by the Administrative Office of the United States Courts and subsequently employed by Cook (1973), Tiffany et

<sup>1</sup>A notable exception to both criticisms is Stone (1976).

<sup>2</sup>At the time of this study, the number of judges on the bench ranged from 161 in Los Angeles County to one per court in the least populated counties.

<sup>3</sup>Support for the referendum varied from over 50 percent in the Bay Area to less than 20 percent in some southern counties.

al. (1975), and Gibson (1976). The general categories of the scale are: (1) suspended sentence, (2) unsupervised probation, (3) fine, (4) supervised probation, and (5) imprisonment. Since we are looking at only marijuana convictions, we adjusted the original scale to discriminate among sentences more finely.<sup>4</sup>

Sentencing scores were computed for each superior court by year. For purposes of analysis, we have excluded for any given year courts with less than five convictions in that year.<sup>5</sup>

The marijuana decisions, from which the judicial policy measure is constructed, are ideal to study the relationship between political participation and government response. For one thing, the state legislature has given broad sentencing discretion to California superior courts. During the period under examination, judges had authority in marijuana cases to impose prison terms of one to ten years, to order jail sentences, to fine offenders, or to

suspend any sentence with or without probation. For another, both the sentencing decisions and public opinion deal specifically and only with the simple possession and use of marijuana. There should be no doubt that we are measuring a direct and well-defined policy linkage between the public and one set of its officials.

### Participation and Response

To explore whether political participation has an effect on government behavior, we employ, initially, the simple regression model

$$\text{Sentencing Severity} = \alpha - \beta \left[ \begin{array}{c} \text{Policy} \\ \text{Preference} \end{array} \right] + \epsilon \quad (1)$$

for each of three years. Our hypothesis is that the estimated regression coefficient of the 1973 equation (disregarding the sign) will be larger than those of the two earlier years, given the communication of policy preferences.<sup>6</sup>

The least-squares estimates reported in Table 1 confirm the hypothesis. While public preferences are related to sentencing in the expected direction in 1973, the coefficients for the two earlier years are not significantly different from zero, as determined by the *t*-ratios.<sup>7</sup> A compari-

<sup>4</sup>Our adapted scale, for example, enables us to treat incarceration of any length as a more severe sentence than sentences not imposing incarceration but requiring long periods of supervised probation. The scale ranges from 0 to 150. Although not interval in the strictest sense, it is used as an interval measure here.

In addition to calculating the sentence severity of those convicted of possession of marijuana, we computed the sentence severity of those charged with possession and convicted of the original charge or a lesser offense. When the latter measure was used as the dependent variable, the relationships were virtually identical to those we report. We employ the former measure because we lose fewer cases with it. We also used as a dependent variable the percentage of dismissals in possession cases. We did not find this measure to relate to public opinion in any of the three years.

<sup>5</sup>All the eliminated courts had only one judge.

<sup>6</sup>An underlying assumption of the regression model is that officials will interpret a county's initiative vote relative to the returns of the other counties. This makes sense, since some sort of referent is needed to give a county initiative vote meaning.

<sup>7</sup>Given the theoretical plausibility of a nonlinear relationship between opinion and sentencing policy, we were careful to examine for such a relationship for each of the three years. None was identified. Given the small *N*'s, we also confirmed the stability of the

Table 1. The Relationship of Public Preference and Sentencing Severity, Marijuana Cases, 1971-1973

Year	Intercept ( $\alpha$ )	Public Preference* ( $\beta$ )	<i>R</i>	$\bar{R}^2$
1971 (N = 47)	133.3	-.58 (1.9)	.28	.08
1972 (N = 47)	120.1	-.32 (1.2)	.17	.03
1973 (N = 45)	140.8	-.84 (2.4)	.44	.19

Source: State of California, *Statement of the Vote* (1972); California Bureau of Criminal Statistics.

\*Figures in parentheses are ratios of the regression coefficients (disregarding signs) to their standard errors (*t*-ratios). Values between 2.0 and 2.3 indicate significance at the .05 level; values greater than 2.3 indicate significance at the .01 level.

son of slopes test, moreover, shows that the 1971 and 1972 coefficients are not significantly different from each other, while the 1973 slope is significantly greater than both slopes of the earlier years. Differences in opinion on the marijuana issue lead to meaningful differences in judicial policy following participation of the electorate.

The coefficients of determination ( $R^2$ 's) reveal a similar pattern. Public opinion explains little of the variance in sentencing in the first two years—8 percent in 1971 and a meager 5 percent in 1972—but, in contrast, a more respectable 19 percent of the variance in 1973. Not only is the structural relationship between public opinion and sentencing significantly greater in 1973, but the overall fit of the data to the regression line is better in the latter year as well.

The *absolute* changes in sentencing provide the most compelling (if somewhat redundant) evidence that the proposition vote, as a clear articulation of citizen opinion, drew forth a response from the courts. A schema for analyzing such changes is presented in Table 2. Examining data from the earlier years, 1971 and 1972, one finds that three sentencing situations are possible relative to opinion: courts are oversentencing, undersentencing, or sentencing consistently with opinion. If response did occur, the first group should become more lenient in its sentencing in 1973; undersentencing courts should become more severe, and courts nearest to opinion should display relatively little change.

Table 3 reports the mean change for each of the three categories of courts.<sup>8</sup> The differential movements accord fully with our expectations. Courts originally oversentencing become discernibly more lenient following expression of public preferences on the marijuana issue, while those originally undersentencing increase the severity of their sentences. The only anomalous

Table 2. A Schema for Measuring Change in Court Sentencing Behavior

Relative Public Support for Legalization of Marijuana	Relative Sentencing Severity in 1971 or 1972		
	Lenient	Moderate	Severe
High	C	O	O
Medium	U	C	O
Low	U	U	C

Source: Compiled by the authors.

Key: O = oversentencing  
U = undersentencing  
C = sentencing consistently

finding in Table 3 is the 1972–73 change among courts originally sentencing consistently with opinion. A careful analysis of the individual courts within this category indicates that this higher-than-expected mean is attributable to the change in sentencing policy of two courts. If these courts are removed from analysis, the mean change drops to +5.6.<sup>9</sup>

The figures in Table 3 take on more meaning when given substantive interpretation. They represent, for example, the difference between a probation of six months and a probation of three years; or the difference between a \$50 fine and no fine at all. In short, the changes in sentencing policy had obvious and meaningful impacts on the lives of individuals convicted of possession.

Our initial findings give us reason to believe, then, that the explicit communication of preferences elicited a response from California superior courts. Not only did the cases increase policy agreement in marijuana cases increase significantly after the initiative vote, but also the courts displayed appropriate and differential movement: those courts less in line with opinion in the earlier years showed greater absolute change in sentencing behavior and adjusted their decisions in the predicted direction.

**Alternative Explanations.** Supportive as the above findings may be, we cannot confidently conclude at this point that superior courts in fact responded to the communication of policy

coefficients by recomputing the regressions after removing one data point at a time.

<sup>8</sup>For purposes of analysis, we collapsed the nine cells (originally created by dividing the initiative vote and sentencing scores at  $\pm 1$  standard deviations) indicated in the hypothetical example above into three categories. This alleviates somewhat the problem of small N's. However, we did look at the patterns across the nine cells. As expected, there was a strong tendency for courts most out of line with opinion to display the greatest change in sentencing, whether the courts were originally over- or undersentencing. We found no evidence that the movement occurred predominantly among rural or urban courts.

<sup>9</sup>Although we were unable to identify factors that directly explain their behavior, these two courts only barely met the minimum number of required cases for inclusion in the analysis. An examination of the remaining categories of courts assured us that the other reported means were not artifacts of deviant cases.

Table 3. Change in Sentencing Policy, by Original Level of Policy Agreement\*

Years	Originally Oversentencing	Originally About Correct	Originally Undersentencing
1971-73	-12.2 (N=15)	-1.1 (N=20)	+11.2 (N=8)
1972-73	-16.6 (N=14)	+9.3 (N=25)	+13.4 (N=5)

Source: State of California, *Statement of the Vote* (1972); California Bureau of Criminal Statistics.

\*Entries are mean changes in sentencing scores.

preferences. If other factors caused the behavior patterns and relationships just discussed, such a conclusion would be spurious.

Our systemic perspective leads us first of all to account for the complexity of the judicial process. The prosecutor's recommended sentence, negotiated guilty pleas between prosecutors and criminal defense attorneys, and the character of the probation-parole officer's presentencing report, are all part of that process. Moreover, police can and do establish different enforcement policies with regard to laws such as those involving possession of marijuana. Since each of these factors can affect the final sentencing decision (Eisenstein and Jacob, 1977), *the question is whether public opinion (specifically in 1973) has an effect independent of them.*

Although we do not have in-depth information regarding these matters, we are able to add three variables to the original bivariate regression model that might alter our original conclusion. The more complete model is

$$\begin{aligned} \text{Sentencing} \\ \text{Severity} = \alpha - \beta_1 P + \beta_2 C + \beta_3 G + \\ \beta_4 D + \epsilon \end{aligned} \quad (2)$$

where  $P$  is the public's policy preference,  $C$  the percentage change in the number of marijuana convictions between the sentencing year and the year before,  $G$  the percentage of guilty plea convictions, and  $D$  the percentage of those originally charged with offenses legally more serious than possession. We assume that  $C$  is a reasonable surrogate for prosecutorial or law-enforcement policies. A marked increase in marijuana convictions, for example, would more than likely result from a combined decision on the part of police and prosecutors to crack down on violators.  $G$  is used as an indirect indicator of the role of prosecutors in processing marijuana cases. Similarly, we include  $D$  in recognition of the fact that downgrading original charges through negotiation

may be more common in some courts than others. Patterns of negotiation between prosecutors and defense attorneys, in turn, may condition final sentencing decisions.

Addition of the three variables to our original regression model leads to the equations reported in Table 4. The estimates reveal that none of the new variables has a significant effect on sentencing policy in any of the three years. In every instance the coefficient is smaller or only little larger than its standard error. More important here, our original conclusion about the relationship between opinion and sentencing policy remains intact. Opinion is significantly related to court decisions in 1973 but not in the two prior years.<sup>10</sup>

An additional potential confounding factor is a change in sentencing policy in *all* drug-related cases. A growing recognition of drug trafficking resulting from the activity surrounding the initiative issue itself, for instance, might have led judges or prosecutors in some counties to take tougher stands across the whole range of drug cases. To consider this possibility, we constructed by court and year a mean standardized sentencing score based on four categories of drug convictions other than those related to marijuana.<sup>11</sup> When these scores are tested

<sup>10</sup>In light of recent research on sentencing (Hagan, 1974), we also controlled for the percentages who (1) were nonwhite, (2) were represented by private counsel, and (3) had major prior criminal records. Since problems of high multicollinearity were identified by an "R<sup>2</sup> delete" test (Kmenta, 1971, pp. 389-90), we regressed the sentencing scores on combinations of two independent variables. The original conclusion remains intact: public opinion has an independent effect on sentencing in 1973 but not in the two previous years. In an effort to avoid ecological fallacy problems, we also considered the impact of all variables on *individual* sentencing scores. Again, the substantive conclusion does not change.

<sup>11</sup>The raw sentencing scores of the four drug offenses were converted into standardized scores. A mean of the standardized scores was calculated for



Table 4. Regressions of Sentencing Severity on Public Preference and Selected Indicators of Presentencing Behavior, 1971-1973

Year	Intercept ( $\alpha$ )	Public Preference ( $\beta_1$ )	Percent Change in Marijuana Convictions ( $\beta_2$ ) <sup>a</sup>	Percent Guilty Plea Convictions ( $\beta_3$ )	Percent Downgraded ( $\beta_4$ )	$R^2$
1971 (N=47)	128.0	-.41 (1.5) <sup>c</sup>	— <sup>b</sup>	.17 (0.5)	.18 (1.4)	.02
1972 (N=45)	100.5	-.34 (1.2)	.07 (1.1)	.14 (0.4)	.20 (1.5)	.02
1973 (N=44)	95.1	-.93 (3.0)	.01 (0.2)	.48 (1.4)	.07 (0.6)	.16

Source: State of California, *Statement of the Vote* (1972); California Bureau of Criminal Statistics.

<sup>a</sup>Variable entered in logarithmic form.

<sup>b</sup>Not included in equation because of resulting reduction in cases.

<sup>c</sup>Figures in parentheses are ratios of the regression coefficients (disregarding signs) to their standard errors.

against our opinion measure, we find no evidence of change in the opinion-policy relationship. In fact, a low relationship in 1971 ( $R^2 = .09$ ) becomes nearly nonexistent in 1973 ( $R^2 = .02$ ).<sup>12</sup> Our original findings do not appear to be the product of a general policy change in all drug cases.

Yet another alternative explanation is that the identified change in sentencing was brought about by new judges coming to the bench. Major shifts in personnel would at least raise the possibility that the higher level of policy agreement in 1973 reflects little more than the addition of new judges whose own attitudes were similar to those of the general public from which they were selected. In this instance, the improved relationship between opinion and sentencing policy would be a product of recruitment patterns rather than actual response.

The California Administrative Office of the Courts keeps the most complete information on personnel matters. Although the office's detailed records do not cover the entire three-year period, they do encompass the months of most concern to us.<sup>13</sup> These records indicate that eight vacancies resulting from death were filled in 1973. Ten other judges were appointed

during the year, bringing to 18 the total number of new judges added to the bench. Not only is this change in personnel (4 percent) minimal, but equally important, the new appointments were made predominantly in the large urban courts comprised of relatively large numbers of judges. Not one of the appointments was made to a superior court consisting of only one judge (at least this was true of those courts included in our analysis), where the potential impact of a new appointment on overall court sentencing is greatest. In addition, the changes in the sentencing of courts that underwent replacement of judges are not greater, on the whole, than those of courts that did not undergo replacement. Nor are the changes concentrated more heavily in the former courts.

Since the preceding discussion emphasizes that the number of judges remained quite constant during 1973, as it did throughout the entire period, we must consider finally the possibility that caseloads increased substantially during 1973. In particular, caseloads may have increased in the urban courts, which tend to serve publics that are relatively lenient toward marijuana use. Higher caseloads could increase the bargaining out of minor offenses such as possession of marijuana (but see Feeley, 1975), leading to a spurious relationship between sentencing and opinion. We find that caseloads remained strikingly constant throughout the three-year period. Total cases filed per judgeship for the fiscal years 1971, 1972, and 1973 are 1191, 1109, and 1116, respectively (Judicial Council of California, 1975, p. 83). Fluctuations across the individual courts, moreover, are small and apparently random. There is no compelling evidence that caseloads consistently became greater in urban courts.

each superior court having five or more cases in at least two of the four drug offense categories. Gibson (1976) has employed a similar measure.

<sup>12</sup>The 1972  $R^2$  is .01.

<sup>13</sup>The California Administrative Office of the Courts does not publish its in-depth information on personnel matters. All figures cited here were made available in a personal communication with the office (March 7, 1979).

**The Aggregation 'Problem.'** While the data indicate that superior courts responded to the clear expression of public preferences, a crucial question must yet be posed: to what extent do the aggregated mean sentencing scores reflect sentencing *within* courts? The systemic perspective adopted in this article led us to aggregate individual sentencing decisions; but the variability of the mean sentencing scores must be small if our findings are to be valid. Otherwise it could be argued that the mean is simply a product of a wide range of decisions that have no common feature except an arithmetic one.

This concern can be addressed in terms of the coefficient of variation, the standard deviation divided by the mean. A value of 1.0 indicates that the standard deviation is as large as the mean; confidence that no aggregation problem exists increases as the value moves toward zero. Coefficients substantially less than 1.0, in other words, are what we hope to find.

Table 5 reports the mean coefficient of variation by year. The coefficients are both small and strikingly consistent across all three years. And the small standard deviations of the mean coefficients themselves indicate that the coefficients vary only slightly across courts in a given year. Our findings do not appear to be statistical artifacts of data aggregation.

Table 5. Mean Coefficient of Variability by Year

Year	Mean Coefficient	Standard Deviation of Mean Coefficient
1971	.40	.18
1972	.40	.15
1973	.38	.20

Source: State of California, *Statement of the Vote* (1972); California Bureau of Criminal Statistics.

### The Absence of Electoral Accountability: A Speculation

To consider more fully the implications of the preceding analysis for democratic governance, we now speculate about one final bit of evidence, namely, that the communication of citizen preferences appears to have elicited government response in the absence of any meaningful electoral accountability of public officials. Consider, first of all, the situation of California superior court judges. These judges have been expressly removed from the vagaries of the electoral process through the institution

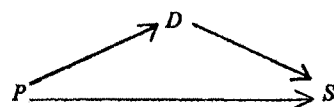
of staggered six-year terms. Not only are they chosen in technically nonpartisan elections, but also significant numbers in fact obtain their positions through gubernatorial appointment rather than election. Between 1941 and 1959, for example, 97 percent of the superior court judges in Los Angeles County serving for the first time were gubernatorial appointees (Cook, 1967, p. 48). Most significant, sitting judges rarely face "reelection" opposition. In 1972, one of the years included in this study, only 4 of 149 elected judgeships were contested (State of California, 1972, pp. 33-34). Although our data do not allow us to say unequivocally that these judges did not anticipate the possibility of electoral opposition, the likelihood of being challenged was remote at best. And since very few judges even faced opposition in 1972, the changes in sentencing policy clearly were not products of tough challenges in that year's general election.<sup>14</sup>

Although district attorneys are more immediately accountable to the people than superior court judges, highly publicized and competitive campaigns occur only occasionally. For many district attorneys, early decisions not to seek reelection eliminate electoral accountability completely (see Prewitt, 1970). Moreover, cases such as those involving possession of marijuana often are handled by assistant prosecutors functioning as members of large prosecutorial staffs. In 1972, for example, the Los Angeles district attorney had more than 400 assistants (Greenwood et al., 1973, p. 7). Large prosecutorial offices effectively isolate assistant prosecutors from centralized supervision (Eisenstein and Jacob, 1977). Finally, there is no evidence that prosecutors or their staffs became more or less severe in the pre-sentencing process as a result of the initiative vote.<sup>15</sup>

If we are correct in suggesting—and the limitations of our data allow us only to suggest—that government response occurred in

<sup>14</sup>Electoral defeat of superior court judges has become more commonplace in recent years.

<sup>15</sup>To consider the possibility that the expression of public preferences affected prosecutors' pre-sentencing behavior, which in turn conditioned sentencing, we tested the causal model (using the notation of Equation [2]):



In all three years, both links of the indirect path are very weak.

the absence of meaningful electoral accountability, at least two implications follow. The first is that electoral accountability may not be, as sometimes is supposed, the *sine qua non* of political responsiveness. Noncoercive or voluntary response may be more common than is generally assumed. Indeed, several other recent research efforts also report findings indicating that public officeholders are inclined to respond even in the absence of electoral coercion. Cook (1977, also 1979; Kritzer, 1978, 1979) has presented evidence, for example, that federal district court judges responded to their political environments with regard to sentencing in draft cases during the height of the Vietnam War. Such judges are appointed for life terms. Similarly, Prewitt (1970) has shown that Bay Area city councils comprised predominantly of volunteers (members not seeking reelection) display notable predilections toward responsive behavior. Some 35 percent of these councils (as opposed to 53 percent of the councils comprised of very few volunteers) are characterized as inclined toward voting with perceived majority preferences. While Prewitt understandably interprets his data in terms of the electoral accountability thesis (i.e., elections do matter), his emphasis leads him to overlook a fundamental question: why should there be any tendency toward responsive behavior when electoral accountability is essentially irrelevant? An answer seemingly lies with Key's observation (1961, p. 538) that a fundamental norm among public officials is "a regard for public opinion, a belief that in some way or another it should prevail."

The second implication, closely related to the first, is the one we wish to stress: *the communication to governmental actors of policy preferences held by citizens may well be a central component if not the core of a responsive system of government.* The popularity of the electoral accountability thesis has perhaps led researchers to overlook the role of this noncoercive linkage in democratic governance (Luttbeg, 1974, pp. 6–10). Yet it is this linkage that allows citizens to influence policy making directly. Because information can be provided prior to or during the deliberation of policy choices, the public is not limited, as it is with voting, to a retrospective judgment. It is both enlightening and revealing that Pitkin (1967, p. 232, emphasis added) should conclude her thorough treatment of political representation by stating, "The governed must be capable of action and judgment, capable of *initiating* government activity, so that government can be conceived as responding to them." The cardinal tenet of democratic theory that government

should respond to public opinion, in some measure and in most circumstances, places responsibility on *both* officeholders *and* citizens. Fulfillment of one obligation is predicated upon fulfillment of the other.<sup>16</sup>

But the communication of citizen preferences to evoke desired government behavior does not just happen. Appropriate structures to facilitate communication from citizens to their officials must exist. To borrow once again from Pitkin (1967, p. 232), "There [must] be machinery for the expression of the wishes of the represented. . . ." For the study reported here, such machinery was available—and apparently worked even in the absence of any meaningful accountability of public officials.

This is not to suggest that as long as opportunities exist for citizens to express themselves on salient policy questions, and they avail themselves of such opportunities, public officials will invariably respond. Participation that conveys mixed and conflicting cues about public policy preferences, for instance, may create a situation of "mandate uncertainty" for policy makers (Janda, 1961), and thereby render an "appropriate" response difficult (Verba and Brody, 1970; Karpis, 1978). A factor of particular theoretical importance that may condition the participation-responsiveness relationship, and which is not systematically taken into account in our analysis, is the nature of the issue. It is relatively easy for officials to respond to public preferences on an issue such as marijuana use, for whatever the distribution of those preferences, they do not pose a direct conflict with another democratic ideal, namely, the protection and expansion of civil rights and liberties. Opposing legalization of marijuana for personal use is not the same as opposing the admission of blacks into state universities or opposing the presence of homosexuals in public schools.

Given a situation where policy makers are confronted with a choice of *either* responding to prevailing public attitudes *or* upholding civil rights and liberties, two equally tenable response patterns can be hypothesized (below). Pattern A is similar to that identified in this article: response occurs across all levels of expressed public support for the issue. Officials attempt to carry out the public will, regardless

<sup>16</sup>In recognition of the fact that the public often does not hold opinions on issues, or at least does not convey them, Pitkin (1967) includes discretion by government officeholders as a component of political responsiveness. We are using a more restricted meaning of the term here.

of what that will may be. In contrast, pattern B implies differential policy behavior by government officeholders; the level of response varies with the level of support for the issue. Among officials representing publics who are relatively unsupportive of civil liberties issues, the response criterion is secondary to other democratic concerns.<sup>17</sup>

While the two patterns represent hypotheses to be tested empirically, they also remind us that, normatively, political participation perhaps should *not* always lead to government response. During the past 15 years, California voters, reasonably representative of the nation as a whole in their opinions, have supported environmental improvement efforts, the extension of voting rights, and bonds to aid poorly financed school districts. But they have also opposed open-housing legislation, supported reinstatement of the death penalty, and rejected efforts to achieve racial balance in the public schools. Not everyone will agree with

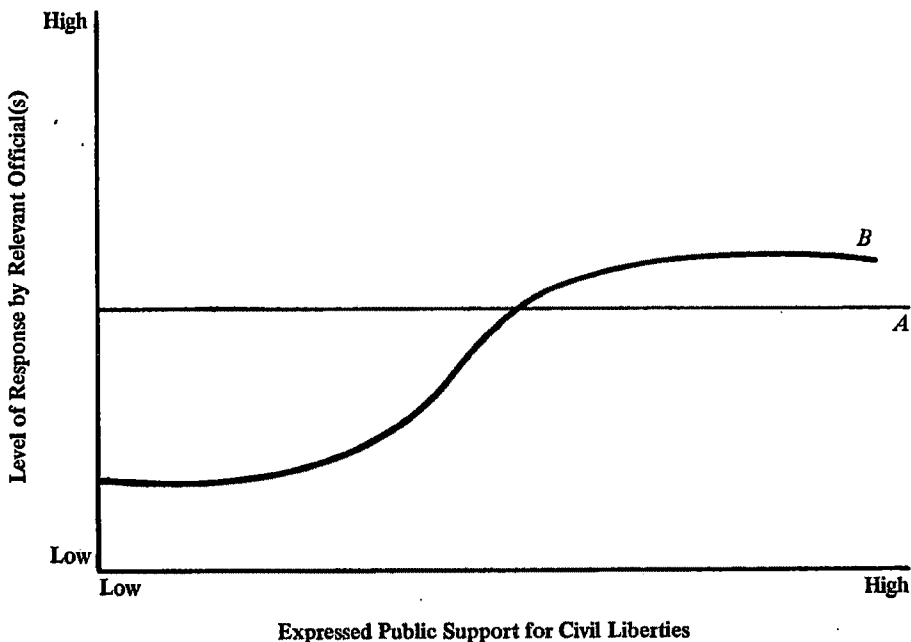
our assessment that the first set of voter actions is more desirable than the second. But the great disparity in these public opinions recommends that we at least restate the working premise of this study, as expressed in our opening sentence: "Responsiveness is what democracy is supposed to be about—*most of the time*." Even a quality of government as central to democratic thought as responsiveness must be tempered; the continuing questions are when and how.<sup>18</sup>

### Conclusion

The central concern of students of democratic politics long has been, and continues to be, the identification of ways by which the public can shape government decisions to its wishes. Because the voting act can be used to reward responsive officials and to punish errant,

<sup>17</sup>The frequency of these two patterns may (and probably *should*) vary across institutions. The expectation of responsiveness is much greater for legislators and executives than it is for judges.

<sup>18</sup>That this statement is more than rhetoric is supported by the fact that researchers have been reluctant to assess institutional performance in terms of responsibility. It is considerably more difficult to determine whether an institution (or official) is acting responsibly than to ascertain whether it is responsive.



Source: Compiled by the authors.

Figure 1. Alternative Response Patterns on Civil Liberties Issues

unresponsive officials, it is commonly seen as the key mechanism for maintaining a close relationship between government and the people it serves. Our analysis, while it does not dispute the importance of the electoral connection to responsive government, suggests that the direct communication of citizen preferences to public officials is another—perhaps the—essential component of the representational linkage.

Not surprisingly, social scientists and practitioners alike have looked to changes in electoral laws as the panacea for unresponsive government. The findings reported here, although admittedly based on data collected in one state over a short period of time, suggest that more attention be given to institutional arrangements that allow and—it is to be hoped—encourage citizens to communicate their wishes to government officials. Such arrangements should facilitate citizen communication that is direct, concise, and based on a large proportion of the electorate. Encouraging citizens' contributions to a government noted for an elaborate system of checks and balances that works to inhibit responsiveness should foster a close—but not precariously close—relationship between the people and their public servants.

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# American Legislative Decision Making and the Size Principle

FRED THOMPSON

*Economic Council of Canada and University of California, Los Angeles*

*This article explains why previous empirical tests of the size principle have failed to produce conclusive results. Particular stress is given to seldom considered aspects of constitutional choice; their implications are explored by means of a simple simulation. Then I propose a test of the size principle and report some initial empirical findings based upon this test.*

Despite K. A. Shepsle's observation that "much of the research on and criticism of the size principle is empirical in nature" (1974, p. 516), empirical analysis has so far failed to provide firm conclusions as to its relevance to American legislative behavior. As Hardin has observed (1976, p. 1211):

There is enormous disagreement over the empirical results. In positive theory, factual claims should be either true or false—not both.

A principal reason for disagreement among researchers has been a general failure to propose what constitutes a fair test.<sup>1</sup>

Special thanks are extended to two erstwhile students: Douglas Buchanan for writing the program for the simulation discussed herein and Make Gilson who collected the data described in the last section of the paper. I would also like to acknowledge the help and encouragement given me in the development of this paper by W. T. Stanbury, Ilan Vertinsky, Aiden Vining, and William Zumeta.

<sup>1</sup>Hardin also makes the point in this article that the complexity of the American legislative decision process—number of participants, diversity of their preferences, subjective uncertainty regarding those preferences, etc.—makes a determinate game-theoretical solution to the vote-minimization problem either unobtainable or trivial, but to say that the size principle cannot in this instance be derived by means of an axiomatic game-theoretic demonstration does not mean that it is false, or even that it is irrelevant to an understanding of the American legislative process. Of course, there must be some a priori justification of the size principle if it is to be taken seriously. In this case, however, a priori justification may be found in a somewhat more restrictive set of assumptions. In addition to the choice-theoretic, utility-maximizing assumptions of game theory, we must assume that the supply of support for legislative proposals is competitive. Then, given a set of legislative proposals, each with its proponents, the size principle may be derived via the method of partial equilibrium. See Koehler (1972 and 1975), Stigler (1971), Silberman and Durdan (1976), Peltzman (1976), Abrahms (1977), Fiorina and Noll (1978), and Thompson and Stanbury (1979).

The purpose of this article is to propose a "fair test" of the size principle. I shall try to explain why it is that previous tests have failed to produce conclusive results; I shall then propose a more satisfactory test and report some initial empirical findings based upon this test.

## What the Size Principle Implies

Given a competitive legislature, the size principle implies that the number of votes cast in support of a proposal is predictable. According to Koehler (1972, p. 154):

In constructing legislative coalitions, [proponents] will direct recruitment to the achievement of minimal winning size and no more. This follows from the assumption that the addition of new members is inherently costly and, therefore, once the coalition is apparently large enough to meet the known minimum requirement of the decision rule, further recruitment would have a negative marginal utility and consequently would be eschewed by rational actors.

In other words, utility-maximizing assumptions generate the prediction that, where other things are equal, the number of votes cast in support of a proposal *will be just sufficient to win and no more*. More generally stated, the size principle states that the number of votes cast in support of a proposal will be determined by the decision rule or rules in effect. Consequently, utility-maximizing assumptions also generate the prediction that the number of votes cast in support of legislative proposals will vary with changes in decision-making rules. Since votes are easily observed and counted, these predictions ought to be directly testable. In fact, they are not, as we shall seek to prove. However, the second proposition may be tested indirectly.

## Why Previous Tests of the Size Principle Are Inconclusive

Previous tests of the size principle have sought to test the proposition that the number

of votes cast in support of a proposal will be no more than sufficient to win. In theory, a direct test of this proposition should be a trivial matter—validation or falsification should require no more than the application of simple descriptive statistics and tests of significance to a set of easily obtainable observations. To falsify or support this proposition, one needs only to

- (1) select an appropriate base, or null model, for comparison
- (2) identify the minimum voted required given the decision rules in effect, and
- (3) determine whether or not the distribution of votes observed is significantly different from (1) in the direction predicted by (2).

As we have noted, counting votes is easy. Moreover, there is a highly satisfactory null model available for comparison—the case in which all possible voting combinations are equiprobable (Hardin, 1976, pp. 1211–12).

Why then is this test not possible? The answer is that one cannot unambiguously specify the minimum vote required given the decision rules and conditions found in real legislatures. Indeed, given the decision rules that actually obtain in American legislatures, it is not possible to make unambiguous quantitative predictions as to either the mean or the variance of the expected distribution of voting outcomes. To show that this is the case, we shall briefly examine the expected consequences of utility maximization under various conditions and decision rules.

Here, five kinds of rules seem relevant:

- (1) those which specify a choice rule based upon votes cast, i.e., unanimity, simple majority, etc.
- (2) those which specify the conditions under which (1) is satisfied, i.e., a simple majority of those present and voting, a simple majority of those eligible to vote, etc.
- (3) those which specify the number of times a legislative proposal may be revised and reconsidered
- (4) those which specify the order in which revisions may be considered
- (5) those which specify the number of decision sites and the order in which they must be encountered.

Given perfect certainty, supporters of a legislative proposal would put forward the combination of demands and payoffs that would maximize their utility. This, of course, implies that the number of votes cast in support of a legislative proposal is determinate and predictable.

In at least two cases, the expected outcome would be identical to that required by the choice rule. Under unanimity, all proposals voted upon would be given unanimous support; decision rules under (2) through (5) would be moot. Under all other majorities, proposals voted upon at a single site would receive the vote required by the choice rule in effect; rules under (3) and (4) would be moot.

However, we would propose that, where a necessary condition for the enactment of a legislative proposal is its approval by a majority vote at each of several decision sites, the expected outcome will be somewhat problematical. This is the case even under perfect certainty. As will become evident, this claim is particularly important to our argument. Consequently, we shall try to substantiate it.

To begin with, the relevance of this issue to real-world legislative decision making should be evident. At both the federal and the state levels of government, legislation must pass through a complex maze of committees in each of two separate houses of the legislature, must be passed by a majority vote in both houses, and must be accepted by an independent executive.

Furthermore, requiring approval of each of several distinct decision sites was intended by those who designed the American legislative process to constrain legislative decision making. Generally it is acknowledged these intentions have been realized. As Lindblom (1977, p. 151) has observed:

Separation of powers between legislative and executive, for example, sets each of two groups as watchdog on the other; . . . government is thus a collection of bodies all intentionally crippled because of their otherwise fearful strength. As a consequence all are to a degree . . . impotent where the citizenry asks that something be done.

Perhaps, more to the point, Silberman (1978, pp. 16–17) concludes:

Naturally, the more difficult the legislative process, the easier it is for a determined minority to block passage. Therefore, in the United States, as opposed to the parliamentary democracies, legislation has usually required a fairly broad national consensus.

That is, requiring approval at several distinct decision sites is similar in effect to increasing the majority required for approval. At a mini-

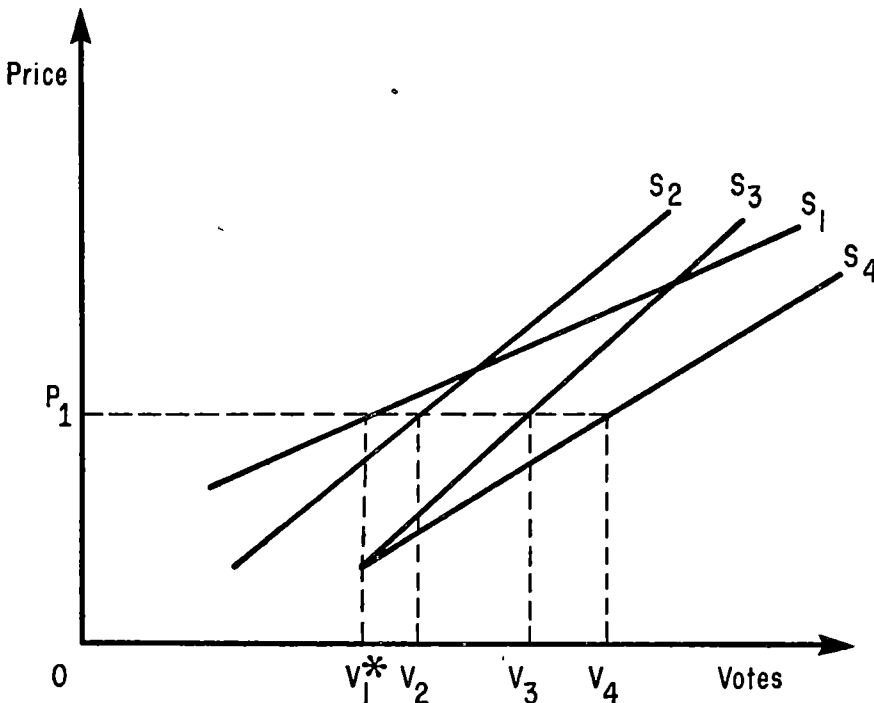
num, given a competitive, price-taking legislature, it would raise the price that proponents of a proposal must offer to ensure the enactment of their legislation. The price offered would be determined by the decision site with the highest minimum price required to assure approval of the proposal. As a consequence, even under certainty, the actual vote at any given decision site may not be identical to that specified by the choice rule in effect. (Indeed, except for the case in which supply schedules are identical across all decision sites, it *will* not be at a majority of sites.)

This effect is illustrated by Figure 1. Here price is shown on the vertical axis and is measured in terms of homogeneous units (e.g., disutility to legislative proponents). The number of votes obtained is shown on the horizontal axis. Support supply schedules are shown as  $S_1$  through  $S_4$ .<sup>2</sup> Each of these is assumed to be

identified with a given decision site, 1 through 4.

The minimum number of votes required for approval at each of the sites is indicated by an asterisk (\*). Consequently, the highest minimum price is determined to be  $P_1$ , the price required to obtain  $V_1$  on  $S_1$ . Note, that at  $P_1$ ,  $V_2$ ,  $V_3$  and  $V_4$  are considerably larger than required to obtain approval at sites 2 through 4. For our purposes this is the most important conclusion that can be drawn from this exercise. A second conclusion is that, except where the supply schedules faced by all proposals are identical, the vote actually observed at any given decision site will vary from proposal to proposal. This is so despite the fact that it is the minimum vote required to insure enactment of the legislative proposal.

To investigate the implications of this conclusion further, we devised a very simple computer simulation of a decision-making system with multiple decision sites. This simulation was designed to reflect the relationships between the number and size of decision sites on the one hand, and support prices and observed voting majorities on the other. It was composed of  $n$  decision sites or "committees," each with  $m$  "members." The members of each



\*

Figure 1. Supply Schedules at Four Voting Sites

<sup>2</sup>Here, the set of supply schedules shown is assumed to represent the set with the minimum highest, minimum price; subject to this constraint, each supply schedule is, of course, assumed to represent the least costly combination of sacrificed legislation and payoffs required to obtain a given level of voting support.



committee were randomly drawn from a population of 99 "legislators." Committees were approached seriatim. In order for a proposal to "pass out" of committee, it had to receive the support of a simple majority of the members. Members supported the proposal if the price offered equalled or exceeded the member's supply price. If the proposal failed to obtain a simple majority in a committee, the offer price was increased and the cycle repeated until a simple majority was obtained. Legislators' supply prices were normally distributed with the mean supply price set equal to the initial offer price. Here it is reasonable to think of the variance of the distribution as one measure of consensus.<sup>3</sup> The simulation was structured to permit the standard deviation of the distribution to be varied and to allow up to 600 iterations of any run. Here, it is reasonable to think of each iteration as representing a distinct proposal.

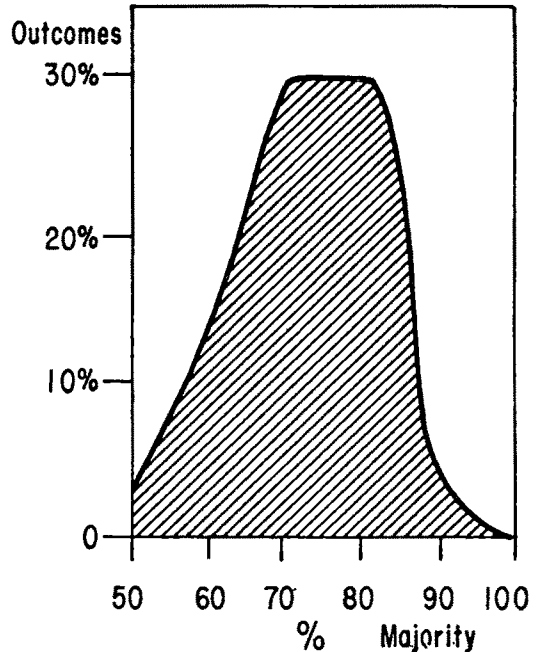
In summary, the input variables for the simulation were:

- (1) the number of committees ( $n$ )
- (2) the number of members on each committee ( $m$ )
- (3) the minimum price required to insure acceptance of the "proposal" by the full "voting population" (equal to population mean)
- (4) the standard deviation of the distribution, and
- (5) the number of iterations.

The outputs of the simulation were distributions of observed "majorities" and supply prices. Each unique set of input variables produced unique distributions of observed majorities and prices.

The key point of this exercise is that the

Table 1. Sample Simulation Results



simulations consistently generated offer prices that were acceptable to large proportions of the 99 legislators. For example, where the number of committees was set at 6, with memberships of 1, 3, 7, 13, 19 and 25 respectively, the initial offer price was set at 50, and the standard deviation of the distribution was set at 5, 400 iterations of the simulation produced the distribution of observed majorities shown in Table 1.

Systematic variation of the input variables also indicated that where the number and size of committees were held constant the mean observed majority increased, but mean price decreased when the standard deviation of the distribution was decreased. One might interpret this to mean that the effect of multiple decision sites is to increase observed majorities, where the degree of consensus in the legislature as to the merits of a proposal is high, and to increase the size of the payoff required to secure its enactment when consensus is low.

Finally, when the standard deviation of the distribution was held constant, both the observed mean majority and observed mean price increased (at a decreasing rate) with an increase in the number of committees and decreased as committee size was increased.

Under certainty, it would be unnecessary to

<sup>3</sup>The shape of the distribution might be another. It should be noted here that there is reason to assume that valid subjective estimates of probability distributions for specific legislative proposals may be made. See Adrian and Press (1968), Hinkley (1972), Matthews and Stimson (1971), and Cherryholmes and Shapiro (1969). Furthermore, it is likely that Bayesian techniques might be employed to revise such a priori probability estimates. In this instance survey or quasi-survey techniques could be used to obtain the required a posteriori probability distributions.

cycle the proposal through various decision sites until the minimum price needed to achieve enactment of the proposal was reached. Proponents of a policy would know the minimum price required. Consequently, all proposals would be winners and, given the conditions assumed in the above simulation, the prices and majorities produced by the simulation would, in fact, occur. Again, rules under (2) through (4), above, are moot.

This is a determinate outcome. Furthermore, the mean and the distribution of expected majorities are predictable. However, accurate prediction requires prior specification of:

- (1) the number of decision sites
- (2) the number of participants at each decision site, and
- (3) the standard deviation of the legislative supply function.

Only the last of these presents problems and, if we can assume certainty on the part of the participants in the decision-making process, we can assume certainty on the part of the analyst.

The problem here is that certainty is a wholly unrealistic assumption. Of course, if there were no costs associated with delay, one might imagine a system in which proposals were recycled at successively higher prices until approval were given, as in the simulation, in which case, the size principle would be binding only on the final, successful vote. Therefore, so long as the analyst attended only to winning votes, the size principle could be directly tested.

But delay is costly (Buchanan and Tullock, 1962, pp. 63–91; Breton, 1974). For this reason legislatures establish rules governing the number of times a proposal can be redefined and reconsidered and the order in which redefinitions are considered. This means that both the proponents of a proposal and legislators must face considerable uncertainty. Furthermore, owing to the absence of readily understandable measures of price and quantity, it is not possible to assume a Walrasian process of *tâtonnement* that would produce a perfect equilibrium despite such uncertainty. Under uncertainty,

each action that may be chosen is identified with a distribution of potential outcomes, not with a unique outcome. Implicit in uncertainty is the consequence that these distributions of potential outcomes are overlapping. It is worth emphasis that each possible action has a distri-

bution of potential outcomes, only one of which will materialize if the action is taken, and that one outcome cannot be foreseen. Essentially, the task is converted into . . . selecting an action whose potential outcomes distribution is preferable; . . . there is no such thing as a maximizing distribution (Alchian, 1950, p. 215).

That is to say, one can talk about minimizing the vote required to win, subject to a given probability of winning, or of maximizing the probability of winning, at a given price, but one cannot simultaneously minimize the vote required to win and maximize the probability of winning.

This point may be illustrated by the simulation described above. If each iteration of the simulation is taken to be an alternative reformulation of a single proposal, given a known minimum price and degree of consensus, the simulation tells us that the probability that any given combination of possible winning combinations of sacrificed legislation and payoffs is the minimum winning combination is less than 2 percent. Alternatively, we might say that for a given combination of sacrificed legislation and payoffs, the probability that it is a winning combination is equal to or less than some specified percent. In the example shown above, a price of "51" would have less than 2 percent chance of success. (See, however, footnote 3.)

The conclusion that where foresight is uncertain vote minimization is meaningless does not imply that reasonable probabilistic predictions of voting distributions are impossible. In order to make such a prediction it would be necessary to know:

- (1) The number of decision sites at which approval must be given. This information should be relatively unambiguous.
- (2) The number of votes required at each decision site to secure approval of the proposal. This also should be unambiguous where the choice rule specifies a number, more ambiguous where number depends upon the number of participants present and voting.
- (3) The standard deviation of the supply schedules. It is reasonable to assume some knowledge of the degree of consensus in the legislature on certain issues; this knowledge can be increased by survey techniques. Furthermore, there is no reason to assume

that in real legislatures, committee assignments are random.<sup>4</sup>

- (4) The risk preferences of proponents of the policies considered. Here unfortunately a number of equally plausible assumptions may be made, e.g., that the consequences of winning and losing are asymmetrical, perhaps, because payoffs have to be made regardless of the outcome. In that case, proponents would err on the side of caution. Alternatively, it might be noted that cognitive theorists tell us that people tend to treat uncertain outcomes as if they were certain (Fischhoff, Slovik and Lichtenstein, 1976; Fishburn, 1973; Kogan and Wallich, 1964). In that case, pessimists would err on the side of caution; optimists on the side of imprudence.<sup>5</sup>

To this list should perhaps be added the degree of monopoly power exercised by participants. For example, power to set rules as to the order in which proposals are considered, etc., might be interpreted as a unit veto. This factor should also be accounted for in making predictions of expected vote distributions.

However, it is not possible that a direct test of the size principle based upon such a prediction would be conclusive. We could be no more sure of the test than of the product of the certainty of our knowledge of 1 through 4. Since we cannot be certain of this knowledge, direct tests of applications of the size principle to American legislative decision making must necessarily be inconclusive.

<sup>4</sup>Most legislators receive the committee assignments they request and their requests are correlated with the interests of their constituents. See Rohde and Shepsle (1973), Shepsle (1975), and Cohen (1974).

<sup>5</sup>If people ignore uncertainty and are pessimistic (i.e., they assume the worst will happen), they will behave *as if* they were minimizing their maximum possible loss (mini-max regret). If they are optimistic, they will behave *as if* they were maximizing their maximum possible gain (maxi-max). Either decision rule has been shown to be consistent with observed political behavior. See Ferejohn and Fiorina (1974 and 1975). Unfortunately, one cannot simply assume optimistic or pessimistic expectations; Samuel Bodily, for example, has shown (1978) that for one class of "risky" decisions about 50 percent of the population are consistently optimistic in their expectations, 25 percent are consistently pessimistic, and 25 percent are consistently neutral.

### An Alternative Test of the Size Principle

Owing to the complexity of the relationships influencing roll-call voting behavior, I have argued that a direct test of the size principle cannot be conclusive. However, social scientists have long recognized the utility of the method of partial equilibrium as a means of decomposition of complex systemic relationships. Its utility rests on the fact that

in cases where the equilibrium values of our variables can be regarded as solutions to an extremum . . . problem, it is often quite possible regardless of the number of variables involved to determine unambiguously the qualitative behavior of our solutions in respect to changes of parameters (Samuelson, 1947, p. 21).

Since the size principle is clearly based upon an extremum formulation, it is reasonable to think that an alternative test might be obtained via the method of partial equilibrium.

Indeed, this is the approach that Koehler took in his test of the size principle, albeit informally. Koehler (1972, p. 155) hypothesized that "changes in party agreement would not produce changes in the corresponding voting agreement which instead will remain fixed and minimal." That is, he hypothesized that

$$\frac{\partial V}{\partial P} = 0$$

$$\left| \begin{array}{l} R = r^o \\ U = u^o \end{array} \right. \quad (1)$$

where

$V$  = size of the majority cast on roll-call vote

$P$  = party majority

$R$  = decision rules

$U$  = uncertainty as to support supply schedules.

This hypothesis may clearly be derived from a formal statement of the size principle. Furthermore, since it is reasonable to assume that  $U$  is random over time and not correlated with  $P$ , and since it is known that  $R$  has remained stable over time, time series analysis of congressional roll-call votes should provide a satisfactory empirical test of Koehler's prediction. Basing his conclusion upon an analysis of 11 time periods, Koehler demonstrated that  $\frac{\partial V}{\partial P}$  is not  $\neq 0$ .

This outcome is clearly consistent with what the size principle would lead us to expect. Nevertheless, while a contrary result would

have cast strong doubt on the size principle, Koehler's findings do not provide positive evidence of its validity. As Hardin (1976, p. 1212) quite correctly has noted: "What [Koehler] actually demonstrates is that the average size of winning coalitions on contested roll-call votes does not correlate very well with the size of the dominant party's majority."

To test the size principle proper, we should want to determine whether or not a change in a key parameter results in a predicted change in the solution value. In this case, we should ask whether or not decision rule changes produce expected changes in observed majorities. For example, it follows from the size principle that

$$\left. \begin{array}{l} \frac{\partial V}{\partial M} \\ R = r^o \\ U = u^o \end{array} \right\} > 0 \quad (2)$$

where

$M$  = the required majority  
 $R$  = other decision rules.

Furthermore, it follows from the utility maximization assumptions that underlie the size principle that if  $M$  is increased, proponents of a policy will wish to reduce  $U$ ; hence:

$$\left. \begin{array}{l} \frac{\partial^2 V}{\partial M^2} \\ R = r^o \end{array} \right\} > 0. \quad (3)$$

Unfortunately, evidence that  $\frac{\partial V}{\partial M} > 0$  would not be very persuasive evidence of the size principle. Confirmation of a counter-intuitive prediction provides for stronger support than does a demonstration of the obvious. Common sense leads us to expect that observed majorities will increase if the required majority is increased.

Demonstration that  $\frac{\partial^2 V}{\partial M^2} < 0$  would appear to constitute a far stronger proof of the size principle, since it is not at all clear that this is what common sense would lead us to expect.

Regrettably,  $\frac{\partial V}{\partial M} > 0$  may be subjected to an empirical test, but  $\frac{\partial^2 V}{\partial M^2} < 0$  cannot. Here, the problem is the lack of appropriate data. To test the first of the propositions, we need only to identify a change in  $M$ . For example, California's Proposition 13 requires a two-thirds majority to pass state tax bills, whereas before, simple majority vote in each house of the

California legislature was sufficient. After a few years, time-series analysis will tell us whether or not this change results in an increase in observed majorities on tax bills. But a similar test of the second proposition is not possible. Additional parameter value data are needed and they are not available. Unfortunately, required majorities are not systematically varied for the convenience of political scientists. Cross-sectional analysis might provide the needed variation in parameter values, but unless other systematic variations in parameter values are held constant, the resulting coefficient estimates likely will be biased. Data gathered from within a single legislature would certainly be unsatisfactory, owing to the fact that required majorities are covariant with types of legislation. Cross-jurisdictional data might prove satisfactory, but it seems likely that the results of such a test might be confounded by variations in the size of legislatures, sophistication of policy advocates (which could be correlated with the size of the state), and so forth.<sup>6</sup>

Similar problems are encountered when other decision rules are considered directly. This does not mean, however, that a satisfactory test of the size principle is infeasible. I argue that it is not necessary to address changes in decision rules directly. I have claimed that an increase in the number of decision sites is equivalent to an increase in the size of the majority required. The test I propose is based upon the assertion that prior approval at alternative decision sites is the equivalent of a reduction in the number of decision sites. That is, if the number of decision sites determines the price that must be paid to ensure enactment of a policy proposal and, therefore, determines the size of the observed majority, we would expect that prior approval at other decision sites would have the effect of reducing the price offered by proponents of a policy and, therefore, of reducing the size of the majority observed in a roll-call vote. For example, legislation initiated by the executive should produce smaller majorities than legislation initiated in a substantive committee of a particular house of a legislature, since executive approval is assured. The same logic should apply to votes on actions which do not require the concurrence of the other house or to situations where the other house has already passed the legislation.<sup>7</sup> Hence, it is proposed that

<sup>6</sup>See, however, Koehler (1975).

<sup>7</sup>See Strom and Rundquist (1977); see also DiPalma (1976).

$$\frac{\partial V}{\partial A} < 0 \quad \left| \begin{array}{l} R = r^o \\ U = u^o \end{array} \right. \quad (4)$$

where

$A$  = prior approval of the executive.

#### A Test of this Proposition

To test this proposition, it was first necessary to hold constant the type of legislation. This was accomplished by focusing upon a set of spending bills (authorizing legislation) enacted in California during August and September of the second half of the 1974–1975 legislative session. Legislative proposals which failed on any of the following criteria were excluded from the sample:

- (1) the proposal had to be contested, trailer and supplementary legislation was excluded from considerations;
- (2) the proposed expenditure had to exceed \$1 million;
- (3) the governor had to take a position on the proposal prior to first reading before Assembly Ways and Means Committee.

Twenty-seven enacted proposals met these three criteria.

The next step was to determine whether or not one could reject with any degree of confidence the hypothesis of no difference in  $V$  between proposals supported and opposed by the governor. Furthermore, it should be noted that the predicted direction of the change, i.e., prior approval produces a decrease in the size of the observed majority, appears somewhat counter-intuitive. One might suppose that the support of a popular Democratic governor would increase the majority given a proposal by an overwhelmingly Democratic legislature.

The statistical test used was quite simple. I combined the observed roll-call vote in each of the two houses and executive position data in a single regression according to the following model:

$$Y_i = \hat{\alpha} + \hat{\beta}X_i$$

where

$Y_i$  = the observed majority on proposal  $i$ ;  
 $X_i$  = the position of the governor (0 if opposed, 1 if supported) on proposal  $i$ .

If it is assumed that the error terms for different observations are normally distributed and uncorrelated, one can formulate an  $F$  test to tell us whether the coefficient ( $\beta$ ) of the independent variable is different from zero. This test was performed on both sets of data. The parameters were estimated using ordinary least squares. The resulting estimated equation for the Assembly data is:

$$\begin{aligned} Y_i &= 66.5 - 3.87X_i \\ r^2 &= .27 \\ F_{1,26} &= 3.97, \text{ sig. } > .90. \end{aligned}$$

The resulting estimated equation for the Senate data is:

$$\begin{aligned} Y_i &= 29.54 - 2.04X_i \\ r^2 &= .12 \\ F_{1,26} &= 4.49, \text{ sig. } > .95. \end{aligned}$$

That is, we can be 90 percent certain the  $\beta \neq 0$ . Note also that  $\hat{\beta}$  has the predicted sign.

Despite the fact that the average observed majority greatly exceeds the minimum required vote in both houses (41 in the Assembly and 21 in the Senate), I suggest that these results provide positive support for the size principle. Of course, the data used in this test may be somehow biased or I may have simply selected an idiosyncratic sample. Strong positive support of the size principle must await further empirical confirmation of these results. However, testing the size principle is not the primary purpose of this article. My primary purpose was/is the design of a "fair test" of this notion. It is my belief that I have achieved this purpose.

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# COMMUNICATIONS

## ON ARTICLES

### Comment on the Holmes and Nichols Exchange (Vol. 73, March, 1979, pp. 113–38)

#### TO THE EDITOR:

I would like to comment on the dialogue in the March 1979 *Review* between Stephen Taylor Holmes' "Aristippus in and out of Athens" and James H. Nichols' reply, "On the Proper Use of Ancient Political Philosophy."

Holmes argues in his article that the Greek ideal of polis, as both society and government, is one of an undifferentiated political whole made of various parts of society, and that such a political ideal no longer applies, and should not be applied, to the modern state, in which life is differentiated into spheres of competing concerns: church versus state, bourgeois capitalism versus government. Holmes then argues that, a dichotomized society being the case, any application of Greek "holism" to the modern state will result in something anachronistic, retrogressive and, it is clearly implied, morally undesirable. Further, he posits, this is exactly the error of totalitarian states and hence, in his view, Marxist and neo-Marxist thought.

Though Nichols responds to some of these points, when answering the totalitarian issue, he restricts himself to saying that totalitarian governments are not guilty of anachronistic Aristotelianism because they in fact violate its premises, and Marxism, too, rejects the sober moderation and political ends of Aristotle's teaching.

What Holmes fails to manifest, and Nichols fails to answer directly, is the charge that Marxism or neo-Marxism is synonymous with totalitarianism. It is obviously not, and yet wording in both articles in various places suggests that it is. Surely no one as seemingly astute as Holmes or Nichols could believe that Marxism must inevitably trace the path of Stalinism, that "hope for a radically different future" must necessarily "justify brutal and murderous means" (Nichols, p. 130). Yet both writers are explicit in their bias toward these conclusions. It is enough to suspect that either ignorance of Western-socialist, neo-Marxist volumes or capitalist ideological myopia is at work here. In either event, it is unsettling to think that these two scholars were incapable of even imagining a distinction between Marxism and coercive government. (For readers with similar tunnel-vision, this distinction has been recog-

nized by individuals as diverse as Ralph Miliband, Charles Bettelheim, Michael Harrington, Michael Best, William Connolly, Leon Baradat, Roy Medvedev, Santiago Carillo, Franz Marek, Giorgio Napolitano, Jean Ellenstein.)

A second point to affirm is a consideration Nichols makes and, in my reading, Holmes does not substantively refute. This is the fact that even the most "differentiated," most modern states are componentially integrated and in that sense holistic. The modern state is, indeed, a "whole encapsulating its subordinate parts," and "a command center which orders society and makes it cohere" (Holmes, p. 135). The notion that it is only fascist or "communist" governments which are totalitarian is the truly anachronistic idea in Holmes' essay—not the Greek model. Nichols defends the "holism or totality" of state culture when he argues, "Apolitical thinking in contemporary liberal society suffers from the same defect of superficiality; it fails to recognize the ultimate dependency of individual freedoms (and of the quasi-autonomy of social interaction contexts) on the political regime of liberal democracy" (Nichols, p. 132).

It is a fallacy for Holmes to reason that because some parts of the modern state may be differentiated they are not also coercively coordinated and manipulated. The distinction between capitalist power and law is viewed by many as illusory. Though religious values and forces may be divided from legal rights and legislative bodies/parties, the rupture is more theoretical than empirical. Too, *where, in theory*, religious virtue may seem a struggling underdog to material pragmatism, it is only because the modern state has in many respects succeeded in transforming secularism and empiricism into a holistic civic culture of its own, with priority over, and eliminating the need for, a religious alternative. Psychologists and sociologists have long documented the manipulative socialization that occurs in schools, fraternal organizations, clubs, workplaces, and through intra-class relationships, and the media. These factors indicate the modern state is far more holistic and undifferentiated and, in *that* sense, totalitarian, than it might pretend. (Recall discussions of economic/social/political holism in the works of, for example, C. Wright Mills, G. William Domhoff, Herbert Marcuse, Louis Althusser, Paul Sweezy, Michael Parenti, Ira Katznelson and Mark Kesselman, G. David Garson.)

To answer Holmes directly, there is very

little which is "beyond politics." To assume there are many areas where democratic government does not need to be is to assume that those areas do not affect us unless we choose them to. This is laissez-faire nonsense. If one of the normative functions of politics is to pursue and guarantee justice, then politics must often define as its territory those public concerns controlled privately.

Democratic neo-Marxists may believe politics, economics, religion, and socialization are all holistically and undifferentiatedly related in any society, and they may believe that there is a more just economic solution than capitalism available, requiring comprehensive political planning. But that prospect is not, by any stretch of the imagination, a more totalitarian solution than the modern Western states have already found.

ROBIN GASS

*Chaffey Community College*

### Reply

#### TO THE EDITOR:

Ideologies, of course, may bolster political regimes, but they cannot be "synonymous" with them. As Marx said in the *18th Brumaire*, the rhetoric of classical republicanism lent an aura of legitimacy to the activities of the Committee of Public Safety, but it was only one factor among many. In my original article, I had very little to say about Marxism. Nothing I did say, moreover, implied the fashionable Parisian insight that "Marxism must inevitably trace the path of Stalinism." In fact, the most I would want to claim is that Marxism—like various extreme right-wing ideologies—has historically had a high potential for being (mis)used to *legitimate* non-democratic regimes. Several of the illustrious souls Robin Gass parades before us have written something similar.

But Gass is not simply wrong about the implications of my article. Because of her lack of clarity about the connotations of the old organicist metaphor, she cannot even present her own position in a coherent way. For example, it is conceptually absurd (leaving aside the empirical issue) to say both that there is *no distinction* between capitalist power and law, and that the capitalist class coercively *manipulates* the law. Likewise, it is unintelligible to assert (without further qualification) both that politics *should* take over crucial social sectors now controlled by "private" enterprise, and that politics and the economy are *already*

"holistically and undifferentiatedly related."

Without presuming to unravel Gass' numerous inconsistencies, I would like to speculate on their source: even though she is vaguely aware that a "more just" society would require *more differentiation* (for instance, between law and capitalist power), her conceptual pre-commitment to diffuse social "wholeness" forces her to deny explicitly what she implicitly affirms.

S. T. HOLMES

*Harvard University*

### ON BOOK REVIEWS

Comment on Wertheimer's Review  
(Vol. 73, March 1979, pp. 226–27)

#### TO THE EDITOR:

In his review of my *Punishing Criminals*, Alan Wertheimer suggests that I am "not aware of some of the difficulties in [my] argument" and "make some glaring errors." He may be right. His generosity, perhaps excessive, must have prevented him from substantiating the "glaring errors" I made. Instead, he charges me with errors I did not make.

Quoting my "conduct prohibited by law is wrong" (p. 9), Wertheimer comments: "This is either tautologically true (if he means . . . legally wrong) or false." But I continued: "To engage in it is to commit an offense. Legal guilt is incurred. . .," i.e., the "tautological" interpretation was intended in this section, which explicitly pointed out that acts "not intrinsically wicked . . . whatever their moral quality" may be offenses. However, even if my "wrong" is misread to mean "morally wrong," it still does not become "false." Unless he contends that conduct prohibited by law is never morally wrong, Wertheimer must have meant—but did not write—"sometimes false (and often irrelevant)."

Wertheimer quotes (p. 16), "Since we all benefit from even the worst social order . . . we all owe society some allegiance in exchange" and comments that I have not shown either the benefit or that it creates an obligation. I didn't, because I attributed this view (which has been around since Socrates) to others, adding: "The idea loses what explanatory value it has . . . when treated as an actual contract rather than an . . . obligation which many people reject. . . ." I do not believe the obligation can be shown.

Wertheimer writes that I "argue that society



has an obligation to punish criminals because it has promised to do so," and adds: "A person to whom a promise is made normally can cancel the obligation. . . ." I explicitly repudiated the view Wertheimer attributes to me and pointed out that the threat to punish criminals is a promise not to them but to the law-abiding who indeed can "cancel" it. Criminals cannot. They receive no promise but a threat.

Wertheimer quotes, "Utility must prevail in making laws, whereas the distribution of punishments must be retributive" (p. 25), and charges that I do not tell "just why retributivism would be wrong" in making laws. I didn't tell because I don't think it is. I merely explain why it should not prevail: "Acts are prohibited on pain of punishment because the prohibition reinforced by the threat is meant to keep people from doing what is prohibited," i.e., the threat of punishment has a utilitarian purpose—which does not make a retributive purpose "wrong." I did explain, however, why the distribution of punishments to specific persons (whether or not useful) must be based on their past crimes and not on future behavior: "No amount of utility then could make the incarceration of an innocent person just, nor is utility required for the punishment of guilty persons" (p. 26). I cannot here reproduce the argument leading to the conclusion. Wertheimer is entitled to ignore it, but I cannot find any basis for his misreading.

Wertheimer writes, "[van den Haag] errs again when he argues that juveniles should be held responsible for their crimes because 'the victim of a 15-year-old mugger is as much mugged as the victim of a twenty-year-old mugger'" (p. 174) without mentioning my next sentence: "The need for social defense or protection is the same." I meant to indicate that the need for social defense is imperative enough to discontinue the immunity given juveniles. I may have misled Wertheimer into attributing the belief to me that the harm done suffices to make the doer responsible for it. I do apologize and assure Wertheimer that I am acquainted with the notion of *mens rea*. So is every careful reader of *Punishing Criminals*.

ERNEST VAN DEN HAAG

New York Law School

## Reply

### TO THE EDITOR:

Ernest van den Haag denies that *Punishing Criminals* contains the "glaring errors" I attributed to it. After reading van den Haag's response and after reexamining the text, I stand by the claim that the argument does, in fact, contain several errors. The reader will have to determine whether or not they are "glaring."

Van den Haag does argue that acts which are prohibited by law are *legal* offenses, whatever their intrinsic moral quality. He distinguishes between acts which are intrinsically evil (*malum in se*) and "non-intrinsic evils" or "bad because prohibited" (*malum prohibita*) (p. 9). He claims that I have misread his argument in thinking that he means to say that prohibited acts are *morally* (as opposed to legally) wrong. If so, then what is the point? To say that acts which are prohibited by law are legal offenses is tautologically and trivially true. A definitional claim cannot establish the state's moral right to punish law violators. Moreover, since the words "bad," "wrong," and "offense" normally carry at least some moral force, I do not think I have misread the argument. I believe that van den Haag means to argue that because the state is morally entitled to make laws and because citizens have an obligation to obey the law, acts which are otherwise not immoral become so by virtue of being prohibited. Perhaps so. But then van den Haag needs a moral argument which has not been supplied. He correctly notes that I should have written that his claim "conduct prohibited by law is wrong" is *sometimes* false. I hasten to point out, however, that if it is sometimes false, then it also is not true that "conduct prohibited by law is wrong."

*Punishing Criminals* states: "True, since we all benefit from even the worst social order . . . we all owe society some allegiance in exchange" (p. 16). According to the book, the idea which loses "explanatory value" is the idea of an implied promise or "contract," particularly an implied promise by the criminal. But, contrary to the impression given by van den Haag's letter, my review did not state that he held a "contract" or "consent" theory of obligation. I argued that he held a "receipt of benefits" theory (which I believe is quite different) and that on this theory he has not shown that all do benefit from the worst social order. More importantly, even if all receive some benefits, he has not shown that those who are systematically oppressed receive benefits which are greater than the costs imposed on them. I find it surprising that van den Haag should now

state, "I do not believe the obligation can be shown." Surely he must believe that some obligation can be shown, or it would seem that the moral legitimacy of punishment is in deep difficulty.

Van den Haag also now denies that he believes society has an obligation to punish criminals in virtue of its promise to them. I am pleased that he no longer wishes to defend this view. But *Punishing Criminals* does:

Debts must be paid in the first place because they are owed, because one has promised to pay them. Retribution must be paid because it is owed, because it has been threatened, and a threat is a (negative) promise. The payment of debts (or of retribution) fulfills an obligation undertaken in the past (p. 14).

The book goes on to say: "Society has obligated itself by threatening. It owes the carrying out of its threats" (p. 15). Now it is true that van den Haag correctly argues against the view that the criminal exonerates himself by being punished, by "paying his debt." But the whole point of his discussion is to say that it is *society's* debt (not the criminal's debt) which is at issue. Indeed, he argues that society has an obligation to the criminal and "law-abiding citizens as well" (p. 16, emphasis added). Thus, I restate my point: society cannot create an obligation to punish the criminal simply by threatening (promising) to do so, unless it has a pre-existing right to make the promise. Perhaps it does. But van den Haag has not shown that it does.

As my review suggests, van den Haag adopts a popular rule-utilitarian position in holding that "utility must prevail" at the legislative phase and that "justice must prevail in the judicial phase. . ." (p. 25). I suggested that he does not show why legislative retributivism would be wrong. He now says it would not be wrong—"I merely explain why it should not prevail." Let me rephrase the point. Why must utility *prevail* at the legislative phase? Why would a thoroughgoing retributivism be false? *Punishing Criminals* contains no answers to these questions. Moreover, if utility is the proper underlying objective of punishment, one needs a complex *argument* (not mere assertions) which shows why punishment should not be distributed on a (sophisticated) act-utilitarian basis. Assertions to the effect that "justice must prevail in the judicial phase" are not sufficient. The incompatibility of retributivism and utilitarianism cannot be so easily defused.

*Punishing Criminals* states: "Children surely should not be held responsible for their conduct to the extent adults are" (p. 173). The book goes on to ask: "But should we regard

sixteen-year-olds or even fourteen-year-olds as children?" (p. 173) and then points out that the "need for social defense or protection [against juveniles] is the same" (p. 174). Now if there are good reasons for treating children as less responsible than adults, the fact that the need for social protection from juveniles is the same (it may be greater) as the need for social protection from adults does not and cannot establish the appropriate degree of juvenile responsibility. Nowhere did I suggest that van den Haag is unacquainted with the notion of *mens rea*. It simply has not been adequately analyzed nor invoked in this context.

ALAN WERTHEIMER

University of Vermont

#### Comment on Rosenthal's Review (Vol. 73, March 1979, pp. 321-23)

##### TO THE EDITOR:

In a recent review of my book, *The French Popular Front: A Legislative Analysis*, Howard Rosenthal raises a very important criticism that merits a response. He questions the statistical methodology that I employ in analyzing roll-call votes, and suggests that roll call analysts such as myself must "shoulder the burden of proof" that their techniques are appropriate to the underlying reality.

The "proof" that I have developed was presented in two articles, "A Reevaluation of Alternative Methodologies in Legislative Voting Analysis" and "The Definition and Measurement of Similarity among Roll-Call Votes" which were published in *Social Science Research* (1975) two years before my book appeared. This material was not repeated fully in the book because it seemed to me that a brief presentation of the technique and a greater concentration on the subject of the study itself was more appropriate in that case. Incidentally, Rosenthal will be interested to find that in those articles are developed: (a) the underlying assumptions about voting behavior, including a treatment of abstentions which is different from the assumption he made in criticizing me, and (b) a precise analysis of the relationship of Guttman scaling to principal components and an indication of when each is to be preferred. Needless to say, on this basis I reject the criticisms that Rosenthal makes of my methodology, which were based on his reading of one article which he admits does "not pertain directly to roll call-by-roll call analyses." Whether one accepts my approach to

legislative analysis or not, I should not be accused of not having thought through the issue of technique.

PAUL WARWICK

*Simon Fraser University*

### Reply

#### TO THE EDITOR:

I am delighted that Paul Warwick has introduced his 1975 *Social Science Research* articles as additional evidence. These articles beautifully illustrate the central point of my review—that mindless application of a statistical black box (e.g., factor analysis, Guttman scaling) will generally fail to recover the underlying behavioral space of a legislature.

Warwick (1975, pp. 363–65) establishes a hypothetical two-dimensional legislature where legislators are points and roll calls cutting lines. “Yeas” and “Nays” lie on opposite sides of a line. Those exactly on the roll call’s line abstain.

What do his computations show?

(1) *Principal components factor analysis can readily give the wrong dimensionality.* Although the true dimensionality is two, Warwick generally finds three or four dimensions! (Similarly, unidimensional data [p. 255] needs three components.) The excess dimensionality immediately implies that the behavioral space is not recovered correctly.

(2) *Even when the dimensionality is correct, the space may not be recovered.* For example, two distinct roll calls, with legislators in all four quadrants formed by their intersection have identical (to the two decimal places reported) factor loadings. (See roll calls 2 and 13 in the last columns of Table 3 [p. 370].) Also, even within the same “scale,” roll calls that are relatively distant may have more similar factor scores than roll calls that are relatively close. (Compare roll calls 11, 13, and 15 in the D2 + D3 columns of Table 3 [p. 370].)

(3) *If a number of roll calls have small angles of intersections, certainly likely in applications to real data, Warwick acknowledges the inappropriateness of principal components* (p. 371) and as well, both the Q and gamma Guttman methods (p. 378).

Warwick does suggest (p. 263) that the unwarranted dimensions of principal components are useful in identifying blocs of legislators. But, in the case of one dimension, blocs fall out just as readily from a simple scalogram, such as Table 4 (p. 255). As to

higher dimensions, Warwick presents no direct evidence that factor scores usefully cluster legislators as they appear in the original two-dimensional space.

Finally, as to my “reading of one article,” that seminal piece by Richard J. Morrison was enough to suggest the impoverished foundations of most roll-call analyses, including Warwick’s. Not surprisingly, Warwick’s own computations for roll call-by-roll call matrices confirm the lessons of Morrison’s analytical work on legislator-by-legislator arrays. If statistical techniques are not grounded in explicit models of political behavior, the black box is likely to be a garbage truck.

HOWARD ROSENTHAL

*Carnegie-Mellon University*

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#### Comment on Hoole’s Review (Vol. 72, December 1978, pp. 1509–10)

#### TO THE EDITOR:

In his recent review of Stuart A. Bremer’s *Simulated Worlds* (1977), Francis Hoole issues a number of criticisms which are pertinent not only to the book, but also to the enterprise within which the volume must be placed: socio-political global modeling. Most specific points in the review are well taken. Yet the thrust of Hoole’s conclusions is, if misinterpreted, at cross purposes with Hoole’s avowed support of the “goals and approach” involved in developing computerized models of global social, political, and economic relations.

Hoole is concerned about the adequacy of the various verbal, mathematical, and flow-diagram expositions of the model in the volume. The difficulty of reconciling these presentations was an impediment to developing an “understanding” of the various meta-components of the model, including: (1) Inter-Nation Simulation (INS) theory (Guetzkow, 1963) and (2) the adaptive and self-monitoring decision-making algorithms Bremer developed, such as the international trade mechanism, diplomatic conflict, budgetary activity, and the formulation and monitoring of national goals vis-à-vis strategic military positions, domestic political sta-

bility, and economic growth. Bremer's presentations sought to highlight the substantive aspects of the theory without obscuring essential details of the model or bogging down the interested reader in complicated technical materials. This was accomplished with elan and most limited resources.

Hoole's dissatisfaction reflects the likelihood that, in general, presentation of simulation materials cannot stand alone. It is necessary to document all phases of such projects in great detail at all levels, from the theoretical design to the computer code used to implement it. The verbal material and the flow diagrams should be combined with a listing of the program (as appeared in Bremer's dissertation). Particularly for someone as familiar with simulation methodology as Hoole, or someone who wishes to utilize the program for research or teaching, access to the program is essential. Participants at the recent International Institute of Applied Systems Analysis' Sixth Global Modeling Conference (Vienna, 1978) stressed the need for adequate documentation in the modeling field (Donella Meadows' estimate was that adequate documentation required half of the hours and money in global modeling projects). The substantively updated program (SIPER4.0) has been thoroughly documented in the listing with statements which tie the functional program statements to both a computerized cross-referenced glossary and to Bremer's various written discussions of the model (available upon request).

Although tracking down subsequent uses of scholarly material is customarily outside the obligations of a reviewer, one wishes Hoole had sought some of the additional materials available. World 3 was similarly critiqued for omitting technical details in early publications. Yet the excellent documentation of the Meadows' team was precisely what permitted other scholars to examine and probe their model so carefully (see, among others, Bremer, 1978, p. 52). In the long run, Hoole's objections will be similarly undercut by the availability of extensive documentation for SIPER4.0.

The validity of the simulation model is tied to the initial development of INS. When INS was created, the data movement in international relations was an infant, if not but a twinkle. This led Guetzkow to use prototypic variables (and nations) in constructing the model. Bremer (1976, p. 309-15) has discussed extensively how using a prototypic strategy has hindered efforts to validate INS and SIPER. Yet in spite of the unresolved nature of the validity issue, INS-based theory and SIPER as well, by virtue of their prototypic constructions, have made

possible considerable rigorous and systematic scholarship in international relations. Nor should we forget that appreciable scholarship has been devoted to validating the INS model on which *Simulated Worlds* is founded (Guetzkow, 1968).

As to the question of "deductions . . . of interest" (Hoole, 1978, p. 1510), the simulation itself is a systematic, rigorous, and internally consistent concatenation of a large number of hypotheses drawn from the scientific literature of international relations (see Wallace, 1977, pp. 241-42). Both INS and SIPER have aimed at cumulative theory construction. Such work has been facilitated, in part, by the use of prototypic variables. Much of the rigor of the construction has been self-consciously shaped by applying the mathematical and logical demands of the simulation format to the hypotheses drawn from the literature. The operation of the simulation yields a series of consequences—which are testable. With respect to how interesting these deductions are, I should note that a major concern underlying the operation of the model deals with the joint, interacting influences of political, economic, and strategic considerations upon the allocation of resources by national political entities. Basic research on these issues will interest scholars for some time to come, as they encompass the interrelationship of "high" and "low" politics.

Hoole does not ask the critical question regarding the Bremer volume and the model it expounds: where does it fit in the effort to construct systematic and rigorous mechanisms with which to better "understand" global relations? It is worth noting that the Bremer effort preceded the widely popularized Club of Rome world-modeling projects. More importantly, until recently these latter efforts have asserted the primacy of economic formulations in building models to aid global understanding of salient phenomena. By exclusion, they indirectly argued for the triviality of political and strategic concerns. It is only in the late 1970s that global modelers are coming to the position advanced much earlier, fundamental in Bremer's contribution: that economics is by itself an inadequate framework for global modeling efforts, and somehow sociological, political, and strategic variables and relationships must be systematically and rigorously included in global models (Ward and Guetzkow, 1978). That is the significance of the Bremer work, and the tradition from which it emerges: it successfully builds an operational model of global affairs which explicitly incorporates the politico-economic-military aspects of global relations.

Even by comparison to the more widely

known models, Bremer's work fares remarkably well in its formulation and in its reportage. To be sure, it must be substantively updated—and work has been under way for some time. What is crucial, however, is that the global simulation/modeling thrust beyond the boundaries of economics not be lost in a barrage of criticism which fails to place Bremer's achievement within the larger context.

The irony is that had *Simulated Worlds* been published sooner and received the attention it deserves, perhaps the global modeling field would have been forced to confront the sociological and political vacuum of their models sooner. Might then the world modelers have incorporated crucial facets in their work at earlier stages, yielding quite different insights into our collective futures?

MICHAEL DON WARD

Northwestern University

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#### Reply

TO THE EDITOR:

I accept your invitation to respond to

Michael D. Ward's quarrel with my review of Stuart A. Bremer's *Simulated Worlds*. And I shall reply with brevity.

Ward and I will just have to agree to disagree respectfully on the contribution of *Simulated Worlds*. There is nothing in Ward's communication which makes me want to change anything in my review of Bremer's book. I do want to wish Ward well in the work in which he has been engaged in recent years in building on Bremer's model. As I noted in my review, "I agree with the goals and approach, but not with the execution of the research [reported in *Simulated Worlds*]."

FRANCIS W. HOOLE

Indiana University

#### Comment on Vaillancourt's Review (Vol. 72, December 1978, pp. 1491–92)

TO THE EDITOR:

Pauline Vaillancourt's review of *Must Canada Fail?* which appeared recently raises serious questions both about the limits of fair comment in a review, and about the editorial responsibilities of the book review editor.

There is, of course, much that I disagree with in Vaillancourt's assessment of the book, most of which can be reasonably debated. My concern, however, is with one passage which reads: "Some of the articles manifest a certain degree of sensitivity to Quebec's present position on independence and sovereignty-association. Others are so narrow and prejudiced as to border on racism."

This allegation is in no way substantiated in the review; nor does the reviewer identify which of the 17 authors fit into her two categories. As a result, every one of the contributors stands accused, in the eyes of your readers, of prejudice and racism. Those are not terms to be used lightly; they constitute very serious allegations which are deeply disturbing to me and my colleagues. They are, we believe, totally without foundation.

They are also charges which are difficult to refute, since they are so general and unspecified. It is, however, significant to note that in the many reviews, in both French and English which have appeared previously, not one reviewer has made a similar statement. Indeed, the majority of reviewers have underlined the depth of understanding of a sympathy with political developments in Quebec in recent years.

The duty of the book review editor, surely,

is to ensure that reviews meet minimal standards of fair comment, and that such serious criticisms which impugn the integrity of the authors not be allowed to appear in print unless fully supported.

Since the allegation in Vaillancourt's review is so erroneous, I believe that both reviewer and editor have seriously damaged the personal reputations of the authors, and that an apology is required.

RICHARD SIMEON

*Queen's University*

### Reply

#### TO THE EDITOR:

The authors of *Must Canada Fail?* are clearly entitled to have criticisms substantiated. My remarks were not trivial or inconsequential. I therefore regret that I did not document them more completely both because of the imposed limits of length and because I did not wish to embarrass particular individuals personally. But Richard Simeon exaggerates when he charges that I have impugned "the integrity" and damaged "the personal reputations" of all those who contributed to this book. On the contrary, because of the manner in which the "objectionable" phrases are worded, none of the contributors is accused of racism.

Many of the articles in this book are sensitive and perceptive. Others are not. As a reviewer I have the responsibility of alerting the potential reader to what I consider to be unacceptable in some of these articles. I could not gloss over biased, preconceived ideas or partial analyses. Neither could I exonerate judgments based on the assumption that English Canadian institutions or culture are superior to those of the Quebecois. It is of course possible that the remarks to which I refer were unintentionally offensive. Nevertheless they must be judged on their objective content and not on the basis of the authors' motivations. I would like to emphasize that not all of the articles in the book are involved, as the following examples will indicate.

The provincial government of Quebec is portrayed as scheming and manipulative (p. 59), unreasonable (p. 68), lacking "good will" (p. 69), naive (p. 189), "beguiling" (p. 189), opportunistic (p. 297), guilty of publishing statistics which are "perhaps inaccurate" (p. 251) and of a problematic character (p. 257). These are personal opinions, not facts.

It is indirectly implied that Quebec national-

ism is xenophobic (p. 129) and socialist (p. 22), engaging radicals, political activists and the left wing (p. 23). Quebec nationalism could become "illiberal and intolerant" at any time, we are told (pp. 129-30). But throughout the book English Canadian nationalism is viewed as quite understandable, a normal phenomenon.

A distinctly paternalistic attitude permeates certain articles. If Quebec nationalism is intolerant, it is contended, this is a "consequence of economic inferiority and uncertain capacity to shape major social institutions" (p. 130). If Quebec were to achieve independence, it would end up weaker than it was within confederation (p. 242). "The best security Quebec can have is to remain within Canada" (p. 83).

We are told that other Canadian provinces, rich in resources, would have no trouble functioning as independent countries (British Columbia, p. 70; Alberta, pp. 82-83; the western provinces as a group, p. 104). But Quebec's project for sovereignty-association is unfeasible (pp. 103, 257). Foreboding and veiled threats characterize some articles. There is no support for Quebec independence, we are told (p. 103). The other provinces will oppose it (p. 69). In some cases warnings are explicit: violence would be unleashed (p. 103), rapid polarization could result (p. 189), civil war such as that in Northern Ireland is possible (p. 189). Armed intervention (p. 205) from the United States, perhaps even France, is contemplated (pp. 189-90). It will not be easy to avoid "large-scale violence or repression" (p. 203). Questions and innuendo create an impression of impending disaster should the Quebecois people vote for sovereignty-association (p. 26). This kind of analysis lends credence to the most unthinkable scenarios (pp. 199-201, 219-21) such as the expulsion of anglophones from Quebec (p. 129) or a coup d'état by leftists replacing the democratically elected Parti Quebecois (p. 200).

The plausibility of these scenarios is called into question by the public opinion pool data presented in another article in the book. While not willing to grant Quebec independence, Canadians are willing to make accommodations (p. 30). There is no fear for the future expressed by the majority of the Canadian population (p. 30), and certainly no support for the idea of forcing Quebec to stay in confederation (pp. 32-33). One author maintains that English Canadians would not accept economic association with a sovereign Quebec (p. 297) but the data indicate (p. 34) that economic union would be approved by the majority of the Canadian population!

The historical development and the prob-

lems of various regions in Canada are presented in part 2, but information about Quebec and the centuries of oppression experienced by the Quebec people, essential for a complete understanding of events in Canada today, is absent. On the contrary, one author suggests (p. 76) that Quebec has received too much from the rest of the provinces in the past! Another article outlines strategy for the federal government. Written in Machiavellian language, it makes no effort to present a balanced perspective (pp. 177-85).

I believe it is clear from these examples that none of the articles in this book is racist. Whether or not those cited are "so narrow and prejudiced as to border on racism" must be decided individually by each reader. I hope that these examples serve, in some way, to clarify my own evaluation for *Review* readers.

PAULINE VAILLANCOURT

*University of Quebec, Montreal*

#### Editor's Note

The *APSR* apologizes to the editor and authors of *Must Canada Fail?* for inattentive editing of the review of the book appearing in Vol. 72, December, 1978, pp. 1491-92. We refer specifically to the statement by the reviewer that certain articles in the book "are so narrow and prejudiced as to border on racism" (p. 1491). It should be noted that *APSR* policy does not permit rejoinders by authors of communications.

#### Comment on Nau's Review (Vol. 73, March 1979, pp. 329-30)

#### TO THE EDITOR:

Henry R. Nau's review of my *Technology, World Politics and American Policy* (1977) contains inaccuracies which significantly distort the point of view of my book. I am writing this letter to set the record straight, and I would appreciate your publishing it.

Nau alleges that I do not understand that pluralism is not only a consequence of the political process, but also "a basic value ensuring individual choice and safeguarding against narrow and inflexible national choice." This allegation is not true.

I recognize and emphasize the value of pluralism in a number of places in the book. My

point of view is perhaps best represented by the following statement: "The American system of government is based on the separation of powers and concomitant pluralism, whose purpose is to safeguard freedom. The value of pluralism built into our system has been proved time and again, Watergate being only one important example. However, on top of the pluralistic characteristics of our governmental system, the impact of technology has overpluralized Washington and the entire nation. Excessive pluralism in the U.S. government presents a serious problem inasmuch as it impedes the government's ability to control the impact of technology and thus create conditions necessary for the enjoyment of freedom" (pp. 310-11).

Nau states that, for the United States, I allegedly favor "a more rational method of decision making in which 'the interests of various groups would be given due consideration. . .'" but 'in the last analysis, the decision would be based objectively on what is good for the nation as a whole and not on the power, vociferousness, or similar attributes of a particular constituency.'" Henry Nau accurately describes my definition of the "rational" (as distinguished from the "constituency" or purely pluralistic) method of decision making, but its acceptance for the United States is not what I am in favor of. I believe there should be a *balance* between the rational and the constituency decision making. Again, to quote from my book: "The task, then, is not to choose one or the other, but to strike the most suitable balance between the two for a particular period of history. Moreover, since each method has its own advantages and disadvantages, a qualitative distinction is appropriate: For a given balance the best elements of each method must receive emphasis" (p. 199).

Because of excesses of pluralism in our society, I argued in favor of moving the balance in the direction of the rational method. However, I emphasized that "this new balance need not, and should not, reduce the pluralistic nature of our system to the point that its ability to safeguard freedom is jeopardized. But it would be expected to eliminate those excesses of pluralism, largely induced by technological impact, that seriously impede the ability of the government to function effectively and to introduce desirable changes" (p. 311).

Nau uses my argument for a U.S. national purpose of a high civilization (or "a virtuous, enlightened society") in which all the people would share as a point of departure for his discovery of "the obvious shortcomings" of my book. Using the aforementioned inaccuracies to

support his contention, Henry Nau implies that my argument is authoritarian in approach. This is not true. As I pointed out on pp. 297 and 312, there are indications of grass-root support in American society for objectives of a high civilization. If my analysis of the future is correct, in time an increasing number of constituencies are likely to emerge in support of this objective. However, an effective presidential leadership, fully compatible with the democratic process, would be required.

VICTOR BASIUK

Arlington, Virginia

**Comment on Cannizzo's Review  
(Vol. 72, December 1978, pp. 1533-34)**

**TO THE EDITOR:**

Cynthia A. Cannizzo's review dismisses my *Field Theory Evolving* as "an affront to philosophers of science, an outrage to statisticians [which] will be irrelevant to nearly all international relations scholars and political leaders." This invites response.

Regarding the book's contents, Cannizzo only notes (incorrectly) that it is "an attempt to answer past critiques and show how field theory has grown and changed." The omission of a summary is critical, for it would reveal weaknesses in her subsequent technical criticisms.

*Field Theory Evolving* attempts to develop, elaborate and test a social field theory of international relations. The theory is that international behavior occurs in a dynamic field (like a free market) of diverse force potentials involving national interests, expectations, perceptions, and dispositions; these forces are imbedded in international meanings, values and norms; which forces influence what behavior depends on the location of nations in a socio-cultural space-time defined by their economic, political, demographic, and cultural attributes, among others; and these forces lie along the distance vectors between nations in this space-time. That is, international differences and similarities (such as rich-poor gap, power disparity, ideological distance) explain much of international relations.

In *Field Theory Evolving* I try to define this theory mathematically and subject it to critical, empirical tests. This raises complex theoretical and operational problems for which widely useful solutions are developed. My tests included numerous factor and canonical analyses, and varied, widely useful empirical results.

Indeed, the theory aside, using about a million pieces of data, *Field Theory Evolving* probably presents most comprehensive general empirical findings on national attributes, behavior, and their linkages. As for field theory, the book shows that it has face validity and internal consistency, and is important, operationalizable, testable, and well confirmed by the empirical data.

Now, for Cannizzo's technical points:

1. *Field Theory Evolving ignores "mathematical implications."* No specifics. A questionable point: chapters do explicitly derive the theory's mathematical implications, including the component and canonical models, status theorems, the appropriate empirical forecasts, and the relationship between field and attribute theories.

2. *It uses "regression analysis while denying the regression model."* No specifics. Since *Field Theory Evolving* uses the canonical model to test the theory, perhaps the reviewer confuses canonical and regression analysis (a *subcase* of canonical analysis for one dependent variable).

3. *It confuses "statistical with substantive significance."* No specifics. I am sensitive to this distinction, which I elaborated in my *Applied Factor Analysis* (1970), and I maintained it throughout *Field Theory Evolving*. Indeed, the concluding chapter explicitly evaluated the theory's substantive versus statistical significance.

4. *The theory "is fundamentally unrealistic."* This is supported by four criticisms:

4a. *Operationalization.* Noting that factor analysis operationalizes the international socio-cultural space-time, the reviewer asks, "Why don't the resulting factors resemble theoretical dimensions laid out by Wright?" (Quincy Wright posited an "analytic field" in his 1955 *A Study of International Relations*.) The answer is that my theory is not Wright's (and his factors were intuitively posited), although there are *some* conceptual similarities. The reviewer confuses similarity in name with similarity in theory.

Cannizzo also asks uncomprehendingly why if factor analysis is proper it delineates many odd combinations of variables. True, for some smaller factors. But the largest attribute-factors clearly define wealth, power, size, politics, stability, density, and culture—hardly odd. Anyway, this is operationally irrelevant: the question is whether factor analysis is mathematically appropriate to the theory.

In any case, Cannizzo notes negatively that factor analysis is linear and offers a "quick glance" through the scientific journals which reveals: "Most of the important relationships in



international relations are curvilinear and multiplicative." First, she incorrectly views linearity as only a first-degree equation, unaware that it also covers the combination of terms in an equation. E.g.,  $y = a + bx + cx^2 + dx^3$  is a curvilinear equation, but a linear combination of terms. Now, field theory assumes a linear space of equations whose terms are linearly combined, as does factor analysis (making it appropriate for operationalizing this space). That is, field theory and factor analysis span many multiplicative (e.g.,  $y = a + bx + cxz$ ) or curvilinear ( $y = a + bx + cx^3$ ) relationships.

Second, Cannizzo's point is irrelevant anyway. Linearity in *Field Theory Evolving* is theoretical, not operational. Field theory assumes linearity (in the space), and its appropriateness depends on the empirical tests. The book presents 51 tests on different nations, years and data. These show that field theory explains about half the bilateral behavior of, for example, Burma, Brazil, China, Egypt, the U.S., or USSR. By contrast, the reviewer submits her "glance" at the journals, leaving implicit her alternative, a priori theory of nonlinearity.

4b. *Time*. *Field Theory Evolving* entails social time, which the reviewer asserts creates unpredictability. This claim is also a priori, and ignores the book's many positive, empirical tests.

Additionally, Cannizzo argues that attributes change more slowly than behavior. "Hence, attributes cannot possibly account for short-term fluctuation in national behavior." A surprising point, given, say, the effects on foreign behavior of the Iranian revolution or China's new leadership. Regardless, she misunderstands the theory and methodology. By theory behavior and attributes are standardized, thus equating behavior and attribute variance.

Even were variables unstandardized, however, she would still be incorrect. Even though attributes change slowly, their combined linear effect may produce surges in behavior (e.g., small cycles may in combination produce high peaks or a flat trend). Anyway, attributes produce behavior only through individual perceptions, interests, and expectations: a slight change in attributes can, through its perception and relevance to interests, trigger sharp change in behavior.

4c. *Feedback*. After then inconsistently noting "that many of the rapid, large, and/or discontinuous changes in attributes result from behavior," Cannizzo argues that ignoring such feedback distorts the results. But in *Field Theory Evolving* one-way causation is a theoretical assumption—an ordering framework (as are cause and effect in Kant's view) for compre-

hending the hidden complex of international forces. Its appropriateness (as with linearity) depends on the theory's efficacy and empirical confirmation, which the book shows are good. The reviewer provides no explication or evidence for her contrary theory.

4d. *Variables*. "Predictor variables are another major source of distortion in field theory." To wit: "the factor analysis" of 236 variables. First, this was done in my *Dimensions of Nations* (1972), not in *Field Theory Evolving* (two chapters use indicators from it). Second, Cannizzo mistakenly claims the analysis is on the dyadic level. Third, the book presents not one, but many factor analyses, such as of bilateral behavior for the U.S. in 1955, 1960, 1963, and 1965, for China during 1950–1965, and for the USSR in 1960 and 1965.

Cannizzo also criticizes the omission of system- or individual-level variables and, finally mentioning an actual fit to data, asks, "What good is a theory that can statistically account for 80 percent of the variance in national behavior when it does not specify the causal mechanism?" Apparently, she dislikes the theory's generality. Fine. She wants to know the intervening causal mechanisms. Also understandable. But *Field Theory Evolving* is only one book. As the book points out, volumes 1 and 2 of my *Understanding Conflict and War* (1976–) provide what she asks and could have been consulted.

In total, her points 4a–4c supporting field theory's alleged "unrealistic" nature are technically uninformed or intuitive, ignoring the book's many positive, empirical results.

Points 1–4 above constitutes the review. To return to the resulting condemnation:

5. *Field Theory Evolving* "is an affront to philosophers of science." The above points are methodological or theoretical; Cannizzo mentions no philosophical blunders or even errors. She simply denounces the book's character. Now, if developing a formal theory, deriving operationalizations and methods from it, attempting to falsify it empirically over 50 times, and assessing its elegance, richness, operationalizability, importance, testability, and confirmation is an affront to philosophers of science, so be it. However, *Field Theory Evolving* is consistent with this philosophy (e.g., Popper, Nagel, Hempel), which originally oriented me towards social science. My debt is footnoted throughout the book.

6. *Field Theory Evolving* is "an outrage to statisticians." The reviewer's points concern approach and research design and cannot support this destructive censure. No gross statis-

tical errors or flagrant mistakes are mentioned. I can only say that *this* statistician is not outraged.

7. It "will be irrelevant to nearly all international relations scholars and political leaders." *Field Theory Evolving* is not for political leaders, nor traditional scholars. It is a theoretical-empirical work, to be judged as a contribution to the science of international relations.

To summarize, Cannizzo's points are incorrect, improper, or irrelevant. They show unfamiliarity with the book's contents, with theory and theory testing and with the relevant mathematics and methods. Her condemnation of the whole work must therefore rest on ignorance.

R. J. RUMMEL

University of Hawaii, Manoa

### Reply

#### TO THE EDITOR:

This reply to Rummel's response is, of necessity, short, as was my original review of *Field Theory Evolving*. With space at a premium one can never document and substantiate arguments in the manner Rummel would like. I have only two points to make.

First, if my review is as irrelevant and as full of errors as Rummel alleges, why did he bother to respond? Does he not trust our colleagues to see through it as he did? I think he does not. Such a belief on my part is substantiated by the number of people who commented to me that they felt my review was sound and right on target. One of those people added, however, that he thought my review was perhaps a bit too harsh. That criticism I will accept.

Second, it seems to me we need to put this argument in perspective. This is not a case of Rummel versus Cannizzo, but a part of the fundamental and raging debate that divides scholars in macro-quantitative research on conflict and international relations. This debate has apparently "progressed" so far that emotionalism and rhetoric outweigh scholarly interchange and that neither side can correctly understand the work of the other. If I misunderstood parts of *Field Theory Evolving*, Rummel also misconstrued what I was saying in my review. I strongly advise readers of these exchanges to take a look at *Field Theory Evolving* through their own peculiar epistemological lenses and come to their own conclusions regarding the relevance of this book.

CYNTHIA CANNIZZO

Ohio State University

## EDITORIAL NOTE

Carl Beck passed away on October 24, 1979. The profession lost a leading scholar, the University of Pittsburgh lost a dynamic and forceful director of its Center for International Studies, and the *APSR* lost a friend and supporting member of the Editorial Board. Carl Beck lived at least two lives in one. He had unbounded enthusiasm for the academic enterprise in all its forms. For Carl, there were no insuperable barriers. "Let's do it" was, perhaps, his most common reaction to any proposal. He launched careers, he stimulated and supported research and writing, he made connections. What was started by Carl will not pass with his leaving but it will be done with less joy.

## BOOK REVIEWS

### Political Theory and Methodology

**A Little White Lie: Institutional Divisions of Labor and Life.** By Robert E. Agger. (New York: Elsevier North-Holland, 1978. Pp. xiv + 193. \$14.95.)

Robert Agger's book exemplifies a barely nascent tendency that may be understood as humanistic political science. "Humanistic" here, as in psychology, would indicate roughly a focus on human beings as autonomously shaping, and assigning significance to, their experience, through their capacity for integrative self-awareness. The "little white lie" that concerns Agger is the modern model of people as aggregates of discrete, objectivizable needs that are met through corresponding discrete institutions. He calls this model "institutional consciousness."

Agger identifies the problem in thinking that needs are discrete as caused by our treating values as equivalent to needs, and consequently as also discrete. However, he asserts, values are actually holistic "human experiences toward which . . . people are oriented" (p. 16), so that conflating them with discrete needs is an error. He concludes that needs may be discrete after all, but now his key point is that values are not.

Similarly, Agger objects to conceiving of "everyday life" as separate from, and residual to, institutional contexts. He asserts that everyday life is actually the total experience of holistic persons, so that "despite what people think, all life is everyday life" (p. 96). Therefore what we think are discrete institutions are really imbedded in everyday life; "discrete institutions do not exist" (p. 13).

These conclusions rest on Agger's assertions that values and everyday life are "actually" holistic. He conceives these to be ontological points, but formulates them as definitions. Since such formulations can be refuted simply by stipulating other senses for the terms, Agger's propositions appear only as attempts to "demonstrate" that discrete needs and institutions "do not exist" by making the conclusions definitionally true.

The exposition is persistently weakened by such confusions about the relations between concepts and phenomena. For instance, surely it is the orientation to experience, and not the experience itself, that constitutes value. In particular, Agger badly wants a concept of the relation between ideas as theoretical and as

constitutive of social reality; he too often resorts to locutions of the form, "what's real today isn't really what's real."

Agger also tends to value conceptual over material realities. His main attack on the idea of discrete needs is in fact directed only against the specific distinction of material from mental needs; his implicit intent emerges as countering the latter's relegation to a "higher but lesser" place in human life. Thus he praises Marx for understanding "production" to include activity satisfying any human need, but criticizes him for sometimes restricting "production" to economic activity.

There is, however, a cogent intention behind Agger's exposition. His implicit thesis is that there is a level of human experience not encompassed by reductionist or objectivizing analysis, and that we must take this level into account if we are to understand institutions and human values adequately. His argument should thus focus not on how institutions and values are defined, but on what account we give of them: that what we do designate as values and institutions are not the kinds of things that institutional consciousness supposes. He could then concentrate on showing that his alternative accounts illuminate just those points that institutional consciousness mystifies. As it is, one remains unconvinced that conceiving institutions and values as discrete is really the problem, rather than, say, conceiving them as suprapersonal or hierarchical, aspects which he treats as secondary.

Agger's intention therefore ultimately emerges as a critique of ideology, specifically of institutional consciousness as the ideology of technocratic elitism, and as a challenge to the corresponding practice. His analysis suffices to show that people in institutional settings are not reduced to roles, but remain "total persons" who experience the settings in terms of their holistic values. Institutions are thus in fact shaped by the purposes and actions even of those whom institutional consciousness defines as "passive" clients and operatives.

Agger therefore concludes by proposing, not that specialization and structure be abolished, but that existing roles be reconceptualized to provide explicitly for, and to foster, this participation. He briefly develops some examples of, and strategies for, doing so. This concept of "opening institutions" goes a step beyond

previous formulations and makes a useful contribution.

RICHARD S. BETH

*Southwest Missouri State University*

**Democracy, Consensus and Social Contract.**

Edited by Pierre Birnbaum, Jack Lively and Geraint Parry. (Beverly Hills, Calif.: Sage Publications, 1978. Pp. 361. \$15.00, cloth; \$7.50, paper.)

At worst, these articles offer perfectly competent surveys: Hannu Nurmi of public choice models of the state; Percy Lehning of property rights in Rawls and Buchanan; Christine Mironesco of game theory. And, with the forgivable exception of John Gray's broodings on Rawls, the pieces are refreshingly unpretentious. In all, the collection would be as useful as any for introducing advanced undergraduates to modern political theory.

In professional terms, these articles are less significant than the enterprise producing them. This volume, together with Brian Barry's collection *Power and Political Theory* (London: Wiley, 1976), announces the formation of a new circle of political theorists meeting annually under the aegis of the European Consortium for Political Research. What may eventually come of it is still uncertain, but this collection offers some clues and much hope.

Theirs is political theory in the broadly analytical mode. The continentals involved speak the language of Anglo-American philosophy, but with interesting accents. They, and the circle as a whole, are wonderfully free of American preoccupations. Only Pierre Birnbaum could be accused of flogging an utterly dead American horse, the "functions of apathy" literature. And of the 16 essays only three address the Rawls-Nozick debate dominating the American scene at the time. Of these, only John Gray's attack on Rawlsian moral epistemology (and especially on the "reflexive equilibrium") counts as a real contribution. Hillel Steiner's essay fails to live up to the promise of its marvelous title, "Can a Social Contract Be Signed by an Invisible Hand?"

Inevitably, the British serve as the initial mainstay of the group. Their contributions to the present volume serve as useful ballast. David Miller explores tensions between liberal conceptions of democracy and justice, showing that whereas early liberals preferred to resolve the tensions by forsaking democracy (restricting political equality), modern liberals have instead

resigned themselves to building considerations of needs into the original meritocratic notion of justice. Jack Lively exposes the consensual presuppositions of pluralistic conflict models: if the state is merely an impartial arbiter, then it embodies shared community values; if political entrepreneurs are instead engaged in bargaining, then everyone must at least acquiesce in the initial distribution of bargaining power and concede the neutrality of the political market. Geraint Parry surveys the views of the standard Great Men on the problem of how much and what sort of things a good citizen needs to know, concluding that even the passive role of being governed demands considerable political sophistication. Even Graeme Duncan's rendering of the tired old individualist-communitarian refrain offers some interesting commentary on the problem of fitting small participatory communities into a "total society" organized, by necessity, around very different principles. All these are utterly solid, if rather uninspired, pieces.

Being more polished, all the facets and flaws of the British essays are pretty apparent. The continental contributions are often more fascinating because, as uncut gems, we are unsure of their ultimate cash value. Often the vignettes are far better than the overarching theories, as in Uli Windisch's discussion of the interplay between class and collective consciousness in Swiss xenophobic movements protesting migrant workers or in Valais, an isolated mountain community managing to combine clan-based local politics with class-based national politics. In other essays, the theory is still tantalizingly tentative. Herman van Gunsteren, for example, wants to recast the notion of citizenship so as to satisfy both Althusser and volunteeristic Enlightenment liberals simultaneously by enquiring into what stands behind what liberals must regard as arbitrary exercises of individual wills. It is an improbable task, to be sure, but in light of van Gunsteren's earlier and equally improbable success (in framing a Wittgensteinian public administration), perhaps it is best to reserve judgment. Far the most impressive is Elias Berg's terse analytic treatment of the notion of self-determination, decomposing it into the two reflexive notions of "self-choice" (meaningful participation) and "self-relevance" (issues at stake must be relevant to one's projects and concerns). Threats to self-determination are treated under the general rubric of "perceptual distortion," either of the essence of the self or of causal relations determining outcomes; and the threats stem either from willful manipulation or from systemic mystification. This is the sort of high-quality, original

theory we might hope to see continuing among this group.

ROBERT E. GOODIN

*University of Essex*

**Democracy in Deficit: The Political Legacy of Lord Keynes.** By James M. Buchanan and Richard E. Wagner. (New York: Academic Press, 1977. Pp. xii + 195. \$14.50.)

Economists Buchanan and Wagner are concerned with the interaction between economic ideas (specifically those attributable to John Maynard Keynes) and political institutions. Their basic thesis in *Democracy in Deficit* is that the Keynesian theory of economic policy has inherent biases toward inflation and a growing public sector when applied within the institutions of political democracy. Part of this bias is owing to problems in the Keynesian model as perceived and implemented by academics and politicians; the authors argue that Keynes' disciples, if not Keynes himself, paid little attention to monetary policy and did not recognize public debt as a burden on present or future taxpayers.

But the more serious difficulty involves the goals and strategies of elected officials and bureaucrats. It is usually politically profitable to foster economic expansion through government intervention in the economy. But few votes are to be garnered by applying the brakes to reduce inflation or to decrease government spending. The result, over time, is a tendency toward continued inflation, soaring deficits, and increased size of government. Even these are not sufficient to eliminate unemployment; as inflationary finance comes to be anticipated, private capital investment is diverted to government spending, transfer payments, and investments which depend on continued inflation for their profitability. In their phrase, "Ploughs, generating plants, and fertilizer would be sacrificed for TV dinners purchased by food stamps" (p. 67).

The basis for this disheartening conclusion (and the plan of the book) is threefold. Buchanan and Wagner first explore the intellectual history of the gospel according to Keynes—a fascinating discussion of Keynes' elitist assumptions concerning public policy, the unique circumstances of the Great Depression, and the ways in which this new paradigm worked its way through the academic mainstream into the media and the consciousness of politicians. The second section of the book briefly surveys economic policy making in the United States

from 1932 to 1976, including an insightful discussion of stagflation, the demise of the Phillips Curve, and current economic issues.

In the third section, Buchanan and Wagner build on their previous writings to present a public-choice model of Keynesian political economy in practice. They argue that under conditions of political competition, the goals of elected politicians, bureaucrats, and monetary authorities converge to produce easy budgets and tight money, whereas the Keynesian prescription for inflation would call for tight budgets and easy money. Voters' rational ignorance about taxes, spending, and the true causes of inflation perpetuate this system, with disastrous results. Since voters are unlikely to learn enough economics to evict the politicians responsible for such deficit financing, Buchanan and Wagner favor passage of a constitutional balanced-budget amendment. Although they admit this may not be effective in containing government growth, they are highly critical of alternative remedies for inflation (central planning, full-employment budgets, or budget balance over the business cycle) for both political and economic reasons. But they neglect to subject their balanced-budget proposal to similar political analysis; cannot reasonably clever politicians devise means to get around such an amendment? Currency inflation is certainly one such solution.

The authors make many excellent debaters' points, but their analysis is marred by flaws of omission and logic. Political scientists would probably find most serious their lack of attention to real-world political institutions and differences among democratic systems. They discuss "institutions of fiscal choice" only in a limited sense referring to methods of financing government. This is well done, in particular their account of the politics of monetary policy, but little mention is made of political parties, Congress, class conflict, or differences in interests or ideology among voters or politicians. The American federal system is relegated to a footnote (p. 58), where Buchanan and Wagner note that the states and cities have accounted for most of the growth in federal spending since 1960. Contrary to the book's major thesis, however, state officials have managed to raise taxes, and state budgetary limitations have not constrained growth in spending. Since these nonfederal units cannot create money, and face legal constraints on indebtedness, the authors blame the stimulus of federal spending. Yet has not the demand for federal revenue come from the states as much (or perhaps more) than from Washington? Could it be that popular support exists for an expanded

government sector, even if increases in highly visible sales, proportional income, and property taxes are involved? The authors are unwilling to consider such alternative explanations because of their concern for deficits and government growth. Nevertheless, the Council of Economic Advisers supported a large federal deficit in the mid-1970s at least partially to offset the deflationary effects of state surpluses. Certainly a broader consideration of the politics of fiscal federalism is needed.

A second difficulty is the absence of quantitative analysis. The book's theoretical structure is buttressed by many numerical examples, but these often appear to have been chosen selectively. Thus the only table in the text (p. 111) purports to show a relationship between deficit spending and expansion in the money supply from 1947 to 1974. But the federal deficit is shown in current dollars; figures in constant dollars, or debt expressed as a proportion of GNP, would show a considerable *decline* in the federal deficit over this period. The recent increase could be attributable to OPEC, the balance of trade, and the 1974-75 recession as much as to the political application of Keynesian economics; but again, alternative explanations are given short shrift.

A third difficulty concerns the basic logic of their argument. Applied Keynesian economics may, under certain (non-Depression) conditions, produce inflation, soaring deficits, or a growth in government spending, but the relationships among these outcomes merit clarification before one can conclude that democracies inexorably tend toward deficits. Here, quantitative analysis or a comparative perspective might help. A comparison of democratic systems suggests many variations, even contradictions. Germany, for example, has very low inflation despite a public sector considerably larger than that of the U.S. The American states (with few exceptions, such as New York) have expanded spending without incurring deficits. U.S. federal spending has been quite stable in relation to GNP (19-21 percent over the 1955-1978 period, according to figures from the Congressional Budget Office and Brookings), although inflation has become a serious problem. Buchanan and Wagner (with John Burton) have written a parallel book, *The Consequences of Mr. Keynes* (London: Institute of Economic Affairs, 1978), on the British experience; a truly comparative treatment of the themes they raise in *Democracy in Deficit* would go far in clarifying their theory.

Persons of a less conservative bent than Buchanan and Wagner may disagree with their conclusions, but *Democracy in Deficit* is a

well-written and thoughtful book, sure to provoke debate on a serious issue of policy and theory. Paying attention to politics and political institutions is a commendable trend in economics. Although political scientists can learn much from this discussion of Keynesian theory and current economic trends, *Democracy in Deficit* still falls short of a political theory of fiscal choice, especially when viewed in comparative perspective.

SUSAN BLACKALL HANSEN

University of Michigan

**From Contract to Community: Political Theory at the Crossroads.** Edited by Fred R. Dallmayr. (New York: Marcel Dreker, 1978. Pp. xii + 172. \$19.50.)

Fred Dallmayr contends that the articles here assembled are united in their concern with the conflict between individualism and community. In apparent counterpoint with Sir Henry Maine's celebrated "from status to contract," the editor implicitly characterizes his book as an exploration of means of bringing Maine's progression to its term; of finding devices for integrating the individuals who had been "liberated" by that progression into a genuine community.

An individualism-communitarianism dichotomy does play a role in most of the eight essays. Moreover, in the articles by C. B. Macpherson and Christian Bay it presents itself in something akin to the normative form just mentioned. Western societies are said to be afflicted by a fragmenting individualism destructive not only of community and its morality but also of individuality itself. In this circumstance a major task of the theorist is to identify a conception of society that transcends the atomistic egocentrism of "bourgeois liberalism" and "liberal democracy" while avoiding both the archaic, status-dominated notions of the pre-liberal period and the recent but barbaric formulations of totalitarianism.

In mentioning the ideologies discussed by Macpherson and Bay we in fact get somewhat closer to the common concern of the several contributors and to the substantial interest of the best of these essays. When Macpherson and Bay attack liberalism as the wellspring of present discontents, they implicitly present the rudiments of a conceptual scheme in which to identify and analyze the "individual," the "society," and hence the possible relationships between them. The schema advanced is dom-

inated by ideological or—more broadly—ideational categories such as “individualism,” “liberalism,” “communitarianism,” and the like, that is, by ideas in terms of which individuals and their interactions are identified and understood. Individuals and societies are not self-subsistent, invariant or even pellucid entities; nor can their character be ascertained by gathering “objective” information about material or other non- or extra-ideational realities. Individuals and societies, and hence relations among them, are what they are by virtue of the ideologies that prevail in the times and places in which they are encountered. As ideologies change, so do the individuals and their relationships.

The question implicit here, the question of how to conceptualize the individual and the society, is the explicit, self-consciously pursued concern of several of the essayists, most notably Jürgen Habermas and Robert Paul Wolff. Habermas, influenced by Marx, seeks a theory of social evolution which gives due weight to Marx’s concept of the mode of production. But this concept is “not sufficiently abstract to encompass the universals of developmental levels” (p. 62) as they have in fact unfolded in human history. Attention must be given to “learning processes” and the “symbolic structures” in which they take place. Habermas does allow that Marx’s concept of the mode of production directs attention to these considerations. But he contends that its application is restricted to “the domain of technical knowledge,” a domain which is “decisive for the growth of productive forces” (p. 55), but which tells only a small part of the story of social evolution and form. Human beings also learn in “the domain of moral-practical awareness that governs the development of structures of interaction.” Rather than simply reflecting changes in the mode of production, these processes “unfold in accordance with their own internal logic” (p. 55).

To understand that logic, Habermas sets aside Marx and Marxist-sounding discussions of bourgeois ideology and looks to theories of kinship from social anthropology and especially to Piaget’s theory of the developmental stages of moral consciousness. The resulting construct appears to me to be at a considerable remove from any recognizable version of the “historical materialism” that Habermas claims to be “reconstructing”; indeed, some will think that it moves so far as to have become a species of idealism. However that may be, it projects an account of the individual-society relationship which, while indeed emphasizing ideational factors, is vastly more complex than the picture presented by Bay and Macpherson. Political

theory will move out of this among its crossroads only when it has achieved an encompassing historical, social and psychological theory, that is, a full-blown “critical” philosophy.

Karl-Otto Apel and Svetozar Stojanovic pursue related themes in different contexts, the former in respect to the possibility of intersubjective standing for moral judgments and the latter in a critique of Stalinism. As with Habermas’ article, familiarity with the other writings of these authors will contribute importantly to understanding the essays here published. Finally, Robert Paul Wolff, in the most self-contained of these essays, considers the claim that societies can and should be held responsible for at least some of the injustices suffered by their members.

Granting that large numbers of white American males have committed unjust acts against blacks and women, what exactly does it mean to say that these injustices are “social” in character? Despite admitted philosophical sympathy for methodological individualism, Wolff draws on Durkheim for a view of the society-individual relationship strongly analogous to Habermas’ doctrine concerning learning processes occurring in symbolic structures. As a consequence of such processes and structures, white males have adopted “beliefs and attitudes” according to which blacks and women constitute social groups with a “real, rather than [a] merely nominal experience” (p. 73). The unjust acts that result from these beliefs and attitudes are therefore social both in origin and effect. The social elements that produce them “may indeed be unreal in the Hegelian sense that they are irrational. But they are nevertheless quite actual, and positive social measures are required to eliminate them.” Wolff concludes that “individualism is not a description of our society; it is a goal toward which we can strive” (p. 79). Readers are likely to share, about the argument of this and other papers, something like the residual ambivalence that this sentence seems to reflect on Wolff’s part. Several of the essays nevertheless repay attention.

RICHARD E. FLATHMAN

*Johns Hopkins University*

**The American as Anarchist: Reflections on Indigenous Radicalism.** By David DeLeon. (Baltimore: Johns Hopkins University Press, 1979. Pp. xiii + 242. \$14.00.)

The radicalism of the 1960s provoked a renewed concern for the American radical past. Within the past few years, an increasingly heavy stream of studies analyzing and dissecting the roots of American radicalism has appeared. While contemporary concerns often do and should encourage us to reexamine the past, there is a particular danger that present political needs will lead to distortion or alteration of that past. David DeLeon's book, while often illuminating and insightful, unfortunately does not escape this problem.

DeLeon has not written a history of anarchism, nor has he set out to do so. His book is a series of reflections, with the strengths and weaknesses of that genre. He argues that there are two varieties of American radicalism—an indigenous type with an anti-statist or anarchist orientation, and a statist, primarily imported brand largely identified with European Marxism. The former is allegedly far more culturally significant even though its extent and influence have been vastly underestimated. Thus, for DeLeon there is a widespread indigenous American radical tradition which can be used and built upon by contemporary radicals if they would only learn to distinguish it from its European competitor.

The factors in American life which DeLeon believes have made our society particularly open to anti-statism include the dominance of a Protestant tradition emphasizing personal conscience, the persistence of a frontier spirit inculcating a sense of self-reliance and constant change, and a capitalist economic system with its ideal an Adam Smithian market. These factors have produced a variety of indigenous radical traditions: liberalism, right libertarianism and left libertarianism.

Liberalism is the most moderate of the anti-authoritarian strains, arising from the American Revolution and our aversion to big government. DeLeon seems to suggest that liberalism's turn towards regulation in this century indicates its intellectual bankruptcy. He spends far more time discussing right libertarians—such as Emerson, Thoreau, and Benjamin Tucker—who insisted on a linkage between freedom and private property and such left libertarians as J. H. Noyes, Edward Bellamy and Emma Goldman who yearned for a communal anarchism.

After criticizing the statist radical doctrines of Daniel DeLeon, the Communist party and

the Socialist party, DeLeon turns to the 1960s revival of radicalism and discerns elements in both the Students for a Democratic Society and the Young Americans for Freedom that harken back, respectively, to left and right libertarianism.

The dangers of such a simplistic and unidimensional categorization of American intellectual history are readily apparent. Individuals and organizations are crammed into slots which they fit badly. To label George Wallace and Ronald Reagan representatives of the right libertarian tradition because they attack big government or pointy-headed Washington bureaucrats is to misunderstand seriously American political life and libertarianism. For DeLeon, however, American hostility to the state and government is essentially anarchistic, a sweeping generalization which ignores such other sources of anti-statism as contract theory. DeLeon to the contrary, contract theory does not self-evidently arise from the same sources or tradition as anarchism. Indeed, the European origins of liberalism—which DeLeon briefly acknowledges—are a primary example of how indebted “indigenous radicalism” is to imported doctrines. American anarchism itself was strongly influenced by continental writers and figures.

DeLeon is concerned not only with demonstrating the strength of native radicalism, but also he insists that it is left libertarianism which is more humane. He charges, for example, that hostility to the state, when not connected to a concern for the common good, is merely a “bourgeois ego trip” (p. 131). Many of the left libertarians DeLeon admires, such as Bellamy and Noyes, substituted hierarchical or authoritarian communal structures for state authority. He argues, however, that “the drugs, sex, literature, and dress of the left represented a more thoroughly humanist and utopian socialist vision than that of the right” (p. 130). While DeLeon prefers the cooperative to the competitive mode of anarchism, he forgets that American hostility to the state has come far more often from a selfish desire for self-aggrandizement and privacy than from a desire for a communal world. The latter vision has been articulated far more often by Marxists and other statists. Left anarchism has never created roots in America—numerous failed communities attest to that—but capitalism has been quite successful.

HARVEY KLEHR

*Emory University*



**Negotiations: Social-Psychological Perspectives.**

Edited by Daniel Druckman. (Beverly Hills, Calif.: Sage, 1977. Pp. 416. \$25.00.)

**The Negotiation Process: Theories and Applications.**

Edited by I. William Zartman. (Beverly Hills, Calif.: Sage, 1978. Pp. 240. \$16.95, cloth; \$6.95, paper.)

These books on negotiation reflect different conceptions of how to go about trying to understand human conduct. Daniel Druckman's approach is that of experimental social psychology, a discipline organized to discover empirical regularities in the interactions of a few individuals by exploring, through controlled experiments, the relationship among a small number of variables. Because negotiations are often small group affairs which take place in a relatively structured environment (in contrast to, say, revolutions), the approach would seem to be relevant. But it also tends to become narrow and ritualistic. And the result of programmatic rigor is usually not a new way of seeing things but rather the perpetuation of old ways. Druckman's book illustrates the conception of science as the patient and optimistic accumulation of empirical findings, and of theory as an ordered body of such findings. Although interdisciplinary in the sense that its contributors come from several fields, the collection displays considerable unity of purpose and method.

Thirteen chapters devoted to reviewing and integrating previous research on a number of topics are arranged by the editor in four categories: (1) the character of negotiation conflicts, (2) the negotiation process, and in particular the relationship between strategies and outcomes, (3) the effects of background conditions such as the personalities of the negotiators, and (4) the effects of factors ordinarily found in negotiations but often missing from the contrived situations of the social psychological experiment. The introduction attempts to suggest how the various "influences" thus categorized might be woven into an integrated theory. The result, however, is very far from being a theory of any sort. On the contrary, it is a jumble of considerations that require rather than provide explanation. The individual chapters, while narrower in scope and thus more coherent, do not give the impression that much progress has been made in understanding negotiations as a result of social psychological research. Most of the new ideas presented here seem to have had their origin outside experimental social psychology, and are novel only in the sense of having been imported from a larger intellectual world in

which they have long been common coin. A good example is provided by several chapters in the first part which focus on the recent discovery by social psychologists that the conflicts in which negotiations occur cannot be understood wholly as conflicts of interest, but must also be understood as "*cognitive* conflicts or conflicts caused by different ideas, values, ideologies, and policies" (p. 106).

The final section, which attempts to come to terms with the complexity of "real world" settings, illustrates the opportunities missed by experimental social psychology as a consequence of its methods and its conception of scientific research. The chapters in this part concern the effects of factors such as conference size and mediation on the negotiation process and its outcome. We learn that an increase in the number of parties creates difficulties for negotiation strategy, makes it harder to reach agreement, and leads negotiators to seek ways of simplifying the issues. Mediation on the other hand can help bring the parties together. This is a slim harvest. These chapters do not begin to tap the richness of even those sorts of negotiations, in labor relations and international diplomacy, that have been closely studied for years. And no attention whatsoever is paid to negotiation in such contexts as legislation or litigation, although the phenomena of legislative agenda battles, plea bargaining, or jury decision making are no less worthy of study than those of international or labor relations.

Of course there is much useful and interesting material in these chapters that is necessarily overlooked in a review of the "skim and scold" variety. My suggestion is that there might be more such material if the experimentalists could overcome the self-imposed limitations of their craft, and especially their tendency to dismiss with disdain mere "case studies" and the "speculations" of the "wisdom literature." Such attitudes have done much to keep the social psychological tradition in a groove along which the present volume moves negotiation research hardly more than a few steps.

In contrast with Druckman's approach, which is "cumulative," that of William Zartman's collection purports to be "dialectical." Zartman has set out to create a "confrontation of competitive paradigms" (p. 9) by soliciting contributions from a sociologist, an economist, a social psychologist, and several political scientists. The second half of the book contains studies of the diplomacy of the nuclear test ban negotiations, the Vietnam peace talks, and several other international negotiations. All of this makes for more interesting reading, al-

though whether Zartman has started a fight or merely orchestrated a chorus is debatable. On the one hand, the reader is indeed treated to a wider variety of theories and methodologies. Several of the essays pay particular attention to Zartman's own interesting conception of negotiation as a two-stage process in which bargainers seek to arrive at a generally acceptable formula before hammering out the details, in contrast to the usual assumption that negotiation involves a uniform process of concession. Zartman also causes us to pay more attention to negotiations as enduring relationships rather than temporary transactions. On the other hand, the Zartman book is actually narrower than Druckman's because the studies in it are largely focused on the relationship of strategies to outcomes, whereas Druckman and his collaborators cover a broader range of questions.

Both books tend to proceed on the assumption that negotiations in which rights as well as interests are at stake are not essentially different from those concerned with purely economic issues. And the studies in the Zartman volume, although free of many of the idiosyncrasies of the social psychological tradition, remain committed to the search for empirical generalizations that are valid under many different circumstances. Negotiation research guided by this conception of science attempts to push to a new level of understanding by generalizing across institutional settings. But it is possible that such research is at the same time obscuring important distinctions between different kinds of relationships that are being indiscriminately lumped together under the label of "negotiation."

TERRY NARDIN

*State University of New York, Buffalo*

**Public and Private Morality.** Edited by Stuart Hampshire. (New York and London: Cambridge University Press, 1978. Pp. x + 143. \$15.95, cloth; \$3.95, paper.)

This is a collection of essays by five distinguished British and American philosophers: Stuart Hampshire, Bernard Williams, Thomas Nagel, Thomas Scanlon and Ronald Dworkin. If there were a truth-in-labeling law for books, Cambridge University Press would be liable for prosecution. The dust jacket and the foreword encourage the expectation that the essays will address "the problem of 'dirty hands' in politics . . . posed most strikingly by Machiavelli." In fact, only three of the six essays in the book address that problem at all directly. The other

three treat issues which will be familiar to readers of books such as Rawls' *A Theory of Justice* and Nozick's *Anarchy, State and Utopia*, but which Machiavelli apparently never dreamed of and probably wouldn't have cared about if he had.

Of the three "dirty hands" essays, Nagel's deals with the version of the problem likeliest to be in the minds of the *Review's* readers. That is, he tries to explain what it is about public roles that prompts us to accept conduct by public officials that we would regard as morally outrageous if engaged in by private individuals. Nagel does not deny that public officials may (indeed sometimes must) do things that are not required of or even permitted to private individuals. But he argues that a correct understanding of the discontinuity between the public and private spheres does not justify the view that a public role frees its occupant of moral restraints to the extent that many have supposed.

The other two "dirty hands" essays are further removed from the issues that one normally associates with Machiavelli. Williams asks, given that morally discreditable conduct is an inherent feature of everyday political life, how readily disposed to such conduct do we want our politicians to be? And answers: not too readily disposed, since "only those who are reluctant or disinclined to do the morally disagreeable when it is really necessary have much chance of not doing it when it is not necessary" (p. 64). It is not clear why philosophical analysis was required to reach that conclusion. In "Public and Private Morality," Hampshire contends that "the moral infamies of the American intervention in Vietnam" (p. 51) resulted from our policy makers having been captured by an over-simple model of rationality according to which actions are rational only if the reasons for them can be made explicit, where only consequences count as reasons. In this and another essay in the book, Hampshire flails away at consequentialism in moral thinking from the vantage point of an intuitionist position which recognizes a plurality of absolute (and possibly conflicting) moral principles not derivable from any single overarching principle to the effect that conduct is morally justified as long as it has certain consequences. Neither of Hampshire's contributions to the collection told me as much about the role of consequences in justifying the actions of public officials as Nagel's did with much greater economy and clarity.

Of the essays by Dworkin and Scanlon, readers of the *Review* will probably find the former of greater interest. Against the widely held view that the term "liberalism" no longer

identifies "an authentic and coherent political morality" (p. 115), Dworkin argues, in a clear, persuasive essay, that if one understands the distinctive ingredient of liberalism to be a certain conception of equality, a case can be made that liberalism is not merely an unstable, middle-of-the-road compromise between more robust conservative and radical positions on either side, but a distinct, self-contained political philosophy in its own right. Perhaps there are some, but I do not know of another brief defense of that view as nearly successful as Dworkin's. Scanlon's essay begins from the observation that classical utilitarianism has been embarrassed by values such as equality, fairness and individual rights, and argues that a more adequate account of those values can be given within a theory that avoids the embarrassment but is, like utilitarianism, still consequentialist.

The best of these essays (to my taste, Nagel's, Scanlon's and Dworkin's) repay careful reading by anyone who does not identify political philosophy with Straussian divination. I do not believe that the remainder will be particularly informative for anyone with even a passing acquaintance with recent Anglo-American moral and social philosophy.

LEON GALIS

*Franklin and Marshall College*

**Lenin's Political Thought: Vol. 1, Theory and Practice in the Democratic Revolution.** By Neil Harding. (New York: St. Martin's Press, 1978. Pp. 360. \$25.00.)

In the study of Bolshevik ideology one question that has faced everyone is the one concerning the relation of Lenin's ideas to original Marxism as well as to the Russian revolutionary traditions of the nineteenth century. Neil Harding seeks to show that it is a mistake to derive Lenin's political thought from *Narodnichestvo* or Jacobinism. Instead, he seeks to show Lenin to have been the most consistent spokesman of a Marxist orthodoxy adapted to Russian conditions by Plekhanov, and accepted as valid by Kautsky. He credits Lenin with having complemented and justified Plekhanov's political orthodoxies by thorough and original economic analyses which proved conclusively that Marxism indeed was applicable in Russia.

Harding convincingly brings out the peculiar features of Russian Marxist orthodoxy and shows Plekhanov as a genuine pioneer of Leninism, who later recoiled from what he had

begotten; he is less convincing in his refusal to acknowledge any of these peculiarities as derived from the *narodnik* heritage. He makes a contribution also by stressing the economic arguments Lenin developed in supporting the political positions he took, though he cannot in fact prove that the latter inevitably were deduced from the former; instead, the economic analyses may often have been rationalizations of political positions taken beforehand. He also provides confirmation of the intensity with which Lenin felt himself to be a modernizer in a retarded society; this is brought out particularly by Lenin's dismissal of *narodnik* theories as *kustarnichestvo*, i.e., the outlook of the pre-capitalist craftsman. It is a pity that Harding links Lenin's own views only to the production methods of the machine shop and not to the managerial rationality pervading all complex organization. Having missed this Weberian glimpse at Lenin, he does not deal at all with Lenin's greatest contribution to revolutionary Marxism, which is his attempt to carry out the revolution with the methods of rational bureaucratic management.

In his effort to prove his thesis, Harding ignores or 'slides over much evidence to the contrary. For instances, he dismisses all information which points to the lateness of Lenin's conversion to Marxism, and pays no attention whatever to discussions of his personality. Weaknesses in Lenin's own arguments are not stressed sufficiently. For instance, in his attempt to prove that Russia had already embarked on the road to capitalism, Lenin adduced the growth of usury, interest bondage, commodity production, and related *pre-capitalist* formations, and then mechanically transferred Marx's developmental scheme to Russia—all this, as Harding rightly points out, in tune with the views of Plekhanov, Struve, and others. Lenin concludes from all this that the peasantry was breaking down into two classes—rural bourgeoisie and rural proletariat. Harding neither stresses the foolishness of these views, in the light of which Vorontsov and other *Narodniks* appear to have been vindicated, nor does he point out that after 1905 Lenin changed his tune, treating the peasantry as a class in its own right, with its own interests. Indeed, Harding's failure to recognize Lenin's post-1907 policy toward the peasantry as a fundamental change, involving the abandonment of his previous economic analysis and political position, appears to be the principal failing of this book—this and the absence of any treatment of the heated discussions which Lenin's new policy engendered within the RS-DRP. Altogether, after stressing the ortho-

dox heritage within which Lenin developed his views, Harding does not sufficiently acknowledge the many retreats from orthodoxy which he staged.

In general, the treatment is not critical enough; and although Harding seeks to dissociate himself from the worst inanities of Soviet hagiographers, the tone of his book approaches theirs. Lenin is treated with admiration and sympathy as one who was right most of the time. His antagonists are described with the same invective that Lenin used, or at least with condescension. The occasional mild criticism of Lenin is voiced so timidly that it has a slightly dishonest tone. Harding is lamely apologetic about Lenin's frequent crudeness, rashness, and misjudgments. In discussing *Materialism and Empirio-Criticism*, that exercise in philosophic philistinism, Harding timidly acknowledges the purely political motives for writing this book and appears aware of its philosophic inadequacies, but glides over them. In many other cases, he mentions contradictions, simplifications, and other failings, but never develops them. In the end, Lenin always seems right, and his opponents puny. His total ideology is given a coherence and sophisticated structure which in fact it did not possess.

While Lenin's rivals are diminished in stature, Harding pits his own interpretation of this material against antagonists who inevitably remain unnamed and uncited. Often the views criticized are caricatures of what previous scholars have written. Many important works dealing with Lenin do not even appear in the bibliography and seem not to have been consulted, including relevant works by Dan, Haimson, Meyer, Arthur Rosenberg, and Aleksinski, to name only a few. Kautsky's *Agrarfrage*, which had a major influence on Lenin, seems not to have been read either.

Many key words in this book should have been defined with some care, including "democracy," "democratic centralism," and the philosophic terms used in *Materialism and Empirio-Criticism*. Harding's phrase, "a straight dialectical either/or" (p. 280) and other passages suggest to me he needs a course in the Marxian dialectic.

The English is atrocious. The editing is sloppy. There are numerous typographical errors, especially in the transliteration of Russian names and titles. In his own translations, Harding obscures the vital distinction between *robachie* and *trudishchiesia*, i.e., between industrial proletarians and the broad masses of all those who work with their hands. Not bringing out the difference means obscuring the vital issue, for Lenin, of the relationship between

these two social classes in the democratic revolution. It symbolized the inadequacy of this work.

ALFRED G. MEYER

*University of Michigan*

**The Political Theory of John Taylor of Caroline.** By C. William Hill, Jr. (Cranbury, N.J.: Associated University Presses, 1977. Pp. 343. \$18.50.)

John Taylor of Caroline County, Virginia, was born in 1753 and died in 1824. He wrote five books and twelve pamphlets, delivered numerous legislative speeches at both the national and the state level, and left an extensive political correspondence. He was engaged in more crucial arguments over a longer period of time than anyone else in early American politics, yet his work has failed to receive careful and thorough attention.

William Hill has attempted to remedy this defect by writing the first comprehensive treatment of the political theory of John Taylor of Caroline. He rejects previous commentaries which cast Taylor either as an apologist for a particular issue, such as agrarianism or states' rights, or as a bridge to important thinkers, such as Thomas Jefferson or John Calhoun. He completely demolishes the Beardian economic determinism approach by carefully examining Taylor's formative years, and he severs Taylor from his contemporaries and successors by demonstrating how far their views diverged. Hill's Taylor is neither an ideologist nor a supporting actor but a political theorist in his own right.

A major political theorist, according to Hill, must have "a comprehensive system of thought that cover[s] a wide range of topics in an integrated fashion" (p. 282). Hill's thesis is that Taylor's concept of "the division of power" is the "unifying principle," or "central doctrine," which makes the wide range of his works internally consistent and systematically presentable. He argues that Taylor's views on such apparently varied and unrelated topics as political parties, aristocracy, separation of powers, federalism, disunion, and agriculture are actually united and integrated because they are applications of the doctrine of the division of power.

Hill is correct; there is a remarkable unity and coherence to Taylor's work. Unfortunately, Hill is more concerned with testing Taylor by the abstract standards of contemporary political theory than he is with taking the con-

temporary reader into Taylor's world and providing a critical guide to the issues which concerned Taylor. Thus we learn that with respect to political parties Taylor was a weak political theorist because he changed his mind concerning their role in popular government. With respect to all the other issues, however, Taylor was a major political theorist because we can find the concept of the division of powers present in Taylor's treatment of these issues, and therefore we can declare Taylor to be consistent.

Hill's Taylor is an important political theorist because a unifying concept can be found in his varied works and he applied this concept consistently. But what is important about the division of power concept? Was Taylor the first or the best interpreter of this concept, and how does it differ from other competing concepts? On these, and other important theoretical questions, Hill's treatment is sketchy and inadequate. Taylor's central concern was to demonstrate that republican liberty was in constant danger and that the only solution was a firm attachment to moral principles. The "division of power" was a moral principle which Taylor introduced in 1814, and developed in the 1820s, to counteract the persistent threat of political and commercial despotism engineered by a privileged minority. Taylor is important because he presents a teaching on republicanism which differs from that of *The Federalist* and John Adams. His intellectual and social origins are within the Antifederalist political tradition.

There are several irritating qualities to the book. Despite the fact that all of Taylor's leading works have been reprinted within the last ten years, Hill persists in quoting from out-of-print Taylor source materials which have a different pagination from the more accessible editions. Is it "Lockian" or "Lockean," "state rights" or "states' rights," "division of power" or "division of powers," "president" or "President"? There are also at least 40 typographical errors. Nevertheless, Hill's work on Taylor is important because it is the first comprehensive account of Taylor's thought.

GORDON LLOYD

University of Redlands

**How to Think About the American Revolution: A Bicentennial Celebration.** By Harry V. Jaffa. (Durham, N.C.: Carolina Academic Press, 1978. Pp. xii + 183. \$12.75, cloth; \$5.00, paper.)

*How To Think About the American Revolu-*

*tion* is a "milestone" in Jaffa's attempt "to understand the American political tradition as it has understood itself" (p. ix), and worthwhile reading for those who pursue that goal.

The introduction (the first of five loosely connected essays) contends that the principles of the American Revolution are hardly understood today, citing as an example Hofstadter's *The American Political Tradition*. The second essay criticizes Willmore Kendall's thesis that Lincoln's emphasis on equality in the Declaration of Independence "de-railed" the American political tradition. Jaffa rightly criticizes Kendall for trying to read Locke out of the American political tradition, and for ignoring the difference between the Declaration's equality principle and more radical notions of equality. Jaffa makes many of the same points in the fourth essay, a response to M. E. Bradford's criticism of the earlier essay. He defends the connection of equality and justice, relying especially on Aristotle's (somewhat equivocal) concept of "proportionate equality."

The third essay, the central and most important part, is a sharp critique of Bicentennial lectures by Irving Kristol and Martin Diamond. Kristol is criticized for playing down the radical character of the Revolution and overemphasizing its "success" and the inarticulateness of the American political tradition thereafter. Diamond is taken to task for arguing that "the Declaration . . . offers no guidance for the construction of free government," since there is nothing in it "which goes beyond the sentiment of liberty" (p. 76).

Jaffa's analysis of Kristol and Diamond provides a useful antidote to their (largely rhetorical) excesses. Kristol does omit the Civil War and its relation to the "successful" revolution. Diamond does argue that the Declaration is neutral on forms of government, although the Declaration's "sentiment of liberty" clearly precludes despotism and would sit rather uneasily with non-constitutional monarchy or aristocracy.

On the other hand, Jaffa consistently and improperly relegates the post-Declaration problems (and thus the Constitution, the *Federalist*, and such questions as "republican virtue") to a secondary place. This is partly because he strains to see so much in the Declaration. He argues, for example, that the Declaration of Independence demands democratic government, relying on Jefferson's argument *elsewhere* that there is a natural right to consent to taxation. He also sees the principle of constitutionalism in the "clear distinction" between consent to the form of government (among the self-evident truths) and consent within the

operation of government (to taxes, in the listing of British abuses). At best, this distinction seems none too clear in the Declaration. Apparently Jefferson did not see the implications of his clear distinction as clearly as Madison did (or as Jaffa does), since he was rightly criticized by Madison for advocating too-frequent constitutional revision. Jaffa can only weakly account for such criticism by saying that it is "founded in Jefferson's own thought." But that raises the question of whether the Declaration leaves more unresolved than Jaffa admits.

Jaffa's "natural theology" of the Declaration involves a Creator who is the source of natural rights (and corresponds to the people acting in its constitutional capacity), and a three-personed God who is legislator, judge, and executive (and corresponds to the people acting in its governmental capacity). However interesting on its own merits, this is a reading into the text rather than a reading of it. Neither it nor vague references to legislatures and judges justify Jaffa's assertion that separation of powers is a part of the Declaration's teaching.

The fifth essay, criticizing the establishment of the APSA Leo Strauss Award for the best dissertation in political philosophy, seems detached from the rest of the book. Yet it does help to explain the otherwise puzzling fact that Jaffa, a devoted student of Strauss (a critic of modernity), is an uncompromising, sometimes virulent defender of one form of modernity, the Lockean American polity. The explanation appears in his remark that Strauss' *practical* teaching took the form of "a celebration of the virtues, above all, of Anglo-American constitutionalism at its best" (p. 171). Thus, Jaffa's book can be understood as a celebration of what he regards as the best form (practically) of a defective politics (theoretically).

In light of that purpose, one understands why Jaffa adopts a "deliberately polemical" style (p. 9). On the other hand, one can ask whether such polemics (often insightful, but frequently unfair) are the appropriate manner in which to deal with scholars like Kendall, Diamond, and Kristol who pursue his own enterprise (albeit in a different way) of celebrating the virtues of Anglo-American constitutionalism at its best.

CHRISTOPHER WOLFE

Marquette University

**Ideology and Myth in American Politics: A Critique of a National Political Mind.** By H. Mark Roelofs. (Boston and Toronto: Little, Brown, 1976. Pp. vii + 262. \$5.95, paper.)

Mark Roelofs' book offers the reader a "general theory" of American government" (p. 7) that is still attentive to "the particularity of the American experience" (p. 244). Its intellectual roots are in "a debate that developed . . . in the field of American Studies in the 1950s" (p. 243), yet it recognizes the need for and tries to work out "a methodology which will unite ideology and behavior as jointly constituting the world of politics" (p. 249). The broad scope of the book's ambition is highly commendable, particularly in light of the narrow specialization, with little effort at synthesis and interpretation, still characteristic of writing by students of American politics and government. Occasional lapses in awareness of related specialized scholarship are surely pardonable, and need not detract from Roelofs' accomplishment. (One example only: Roelofs refers to Locke's "defense of the English constitution yielded by the Revolution of 1688" (p. 89) despite his reliance on Peter Laslett's edition of Locke's *Two Treatises*, in which it is convincingly shown that Locke wrote well before 1688.) His central preoccupation is not with novel or esoteric materials but with "the plain and obvious" (p. v): major historical events, documents, figures, problems. "We all know what the evidence is; the question is, what now, in the 1970's, are we to make of it?" (p. vi).

What Roelofs' book makes of the evidence is most depressing. His lead question is, "Why has America failed itself, so often, so disastrously?" (p. 254). And his answer is "that in America the discrepancy, the tension, between myth and ideology is extreme, so extreme as to be self-crippling" (p. 4). "Myth" and "ideology" are stipulated in somewhat unusual ways. Both terms refer to shared frameworks of political consciousness, with "myth" used to distinguish national identification and aspiration at a fairly lofty level—"a people's legend and its hope" (p. 4)—and "ideology" reserved for more narrowly practical patterns of operative behavior. America's myth, according to Roelofs, is Protestant, its ideology bourgeois; the myth is humanistic, egalitarian, and generally virtuous while the ideology is avaricious transactionism and rather sordid. Each is characterized as "a remarkably self-consistent whole" (pp. 38, 42), mainly because "for purposes of analysis we have separated ideology from all the mythic elements of the American political mind" (p. 38).

The book uses the tension between what America aspires to be and what it is and does to analyze a great range of topics, often with insight and interest: the Constitution, the Civil War, electoral politics, alienation, the "great" presidents, Watergate and the impeachment process, and much more. Its conclusions are almost invariably critical. So it bears stressing that the distressing picture of American political life is not the artifact of some "ought" imposed by Roelofs; rather, "the American political system is a failure *in its own terms*. We are not what we want ourselves to be" (p. 240, emphasis added). But not only is the past distressing; the prospect is more of the same. The prospects of or for revolution are nil, and so is the chance for change within the system. Roelofs' only advice is to have "a period of sober reflection" (p. 242). But he has already argued that the interplay between myth and ideology creates a cyclical pattern of despair, violence, and return to tense normality that is remarkably stable.

In analytic terms, Roelofs' conclusion appears to be that our national political mind is so sick, so stuck in repetition-compulsion, and so paranoid that the national political equivalent of (psychological, not economic) depression would be a step towards health. One very much hopes that the brave bleakness offered by Roelofs and his owl is a product of the reactionary 1970s. But one wonders.

JOHN R. CHAMPLIN

Ohio State University

**The Foundations of Modern Political Thought:** Vol. 1, *The Renaissance*; Vol. 2, *The Age of Reformation*. By Quentin Skinner. (Cambridge: Cambridge University Press, 1978. Pp. xxiv + 305; vi + 405. \$29.50, cloth; \$9.50, paper, each.)

Underlying this magisterial survey of the transition from medieval to modern political thought is the provocative claim that the history of political philosophy can be successfully rewritten as "the history of ideologies" (p. xi). Because Skinner is exquisitely scrupulous in rendering the views of so many diverse thinkers (including Dante, Marsiglio of Padua, Machiavelli, Guicciardini, Erasmus, More, Luther, Calvin, Vitoria, Suarez, Beza, Hotman, Mornay and Bodin), his concept of "ideology" accumulates a wealth of different connotations. To appreciate the truly significant achievement of these volumes, in fact, it is important to distinguish between at least two separate ways

in which Skinner actually uses the term: ideology as the general *mentalité* of a culture or age, and ideology as skillful (sometimes quite cynical) propaganda.

As is well known, Skinner advocates a "contextualist" approach to the study of the history of ideas in contrast to context-indifferent "textualism." But "contextualism" too is a polysignificant word, and (as it happens) there is an implicit reference to *two distinct contexts* attached to Skinner's double concept of ideology.

On the one hand, there is the background intellectual context, for example, the "normative vocabulary" or "the prevailing assumptions and conventions of political debate" reigning in early modern Europe (p. xiii). Although he sometimes claims to be interested in routinized or standardized patterns of thought for their intrinsic historical importance (p. xi), Skinner elsewhere admits that, so far as his present work is concerned, the "main reason" (p. xiii) for tracing out the *mentalité* or commonplace ideologies of, say, Machiavelli's and More's lesser predecessors is to provide a "benchmark" (pp. 180, 255) against which he can accurately gauge Machiavelli's or More's originality as political theorists. Stated in Skinner's own favorite Austinian terms, placing a work within this sort of context allows us to appreciate its "illocutionary force," since we cannot understand the precise impact of what someone is saying until we know what he is denying. The thrust or explosiveness of an utterance cannot be discerned acontextually. This is true since what makes an utterance so surprising is the fact that it was spoken or written against the odds, that its context made it such an unlikely or improbable innovation. By leading us on a long tour through a multitude of fourteenth- and fifteenth-century homilies about the necessity for princes to be loved rather than feared, always faithfully to keep their word, to combine *virtù* with the Christian virtues and so forth, Skinner prepares us to comprehend the power of Machiavelli's denial of these conventional pieties. By first setting forth the typical humanist tenet that a virtuous commonwealth must include "degree, priority and place," he can subsequently convey the extraordinarily radical nature of More's attack on social inequality and private property. It is worth stressing that Skinner's "benchmark contextualism" (my phrase) has an extremely orthodox end in view. It allows the historian "to return to the classic texts themselves with a clearer prospect of understanding them" (p. xiii).

Alongside this notion of ideology as the conventional intellectual milieu environing a



"classic text," Skinner introduces the quite separate concept of ideology as deliberate political propaganda. This second sort of ideology, as already mentioned, corresponds to a second kind of milieu within which Skinner wants to situate theories: the political context within which doctrines are "applied" in order to "legitimate" certain courses of action. Skinner is addressing himself here, as he has done elsewhere, to Lewis Namier's tough-minded suggestion that political historians can safely neglect the history of ideas, since political principles are largely "flapdoodle" or *post festum* rationalizations for actions which people would perform in any case and for extra-theoretical reasons. Skinner's counterargument is simple and convincing. Most political agents need cooperation in order to reach the goals they set themselves. To mobilize others into cooperating, they must present their own goals in a way that makes them appear compatible with the goals of partners being wooed. But once agents "legitimate" their behavior by describing it in widely accepted evaluative terms (once German princes describe their expropriation of monastic lands as part of the evangelical Reformation, thus mobilizing support for their initially greedy cause), they are no longer sovereignly free agents, but are instead bound at least to *appear* to act consistently with the terms of their "legitimizing ideology," lest they lose the support and cooperation they need.

Now, what makes this book a brilliant advance on (rather than merely an application of) Skinner's previous methodological writings is his success at simultaneously employing these two distinct forms of contextualism, each affiliated with a special use of the term "ideology."

There is a closely related methodological premise implicit in the major historical claims Skinner puts forward. Each volume is organized as a series of polemical refutations of misleading contextualist accounts of Renaissance and Reformation political thought. One of the main targets in the first volume, for instance, is Hans Baron's *Crisis of the Early Italian Renaissance*. According to Baron (and this is a thesis which Pocock too accepts), there exists a sharp "cleavage" between the traditional convictions of Italian political theorists prior to the fifteenth century and the new outlook attained by the Florentine "civic humanists" at the outset of the *quattrocento*. The rupture is said to have occurred in 1402, under the impact of the threat to Florentine liberties posed by the expansionist Milanese tyrant, Giangaleazzo Visconti. As Skinner shows, however, a strong

republican or civic ideology can be discovered in Italy as early as the 1260s. It is embodied in an illustrious tradition, including the works of Brunetto Latini, Mussato, Marsiglio of Padua, Bartolus and Ptolemy of Lucca. Thus, says Skinner, it is misleading of Baron and Pocock to suggest that republicanism "began" in the immediate political context of threatened Florentine liberties.

Analogously, one of the chief targets of the second volume is Michael Walzer's *The Revolution of the Saints*, where it is claimed that there is something radically innovative and unprecedented about the revolutionary ideology of sixteenth-century Calvinism. Here again, Skinner looks backwards and finds that the purportedly novel "Calvinist revolutionary ideology" is actually composed of theoretical components which were developed prior to the upsurge of Calvinism, to some extent by Luther, but even more importantly, by the Catholic advocates of conciliar rights against papal "absolutism."

On the face of it, these two revisionist claims seem like products of traditional philology, of "influence studies" or "source exhumation." It is an age-old problem in geography: where does the river really begin? And Skinner has shown that Baron and Walzer misidentify the "origins" of the theories with which they are concerned. But to interpret Skinner's arguments as a mere expression of bibliographical curiosity (who said it first?) is to miss his underlying methodological point. The prime duty of a contextualist, Skinner obviously believes, is to avoid the vulgar assumption that there is only *one* (homogeneous) context within which political theories must be "situated." By focusing exclusively on the political and military context of 1402, Baron neglected the crucial *intellectual context* furnished by the republicanism of Bartolus, Marsiglio of Padua and others. By concentrating on the political and religious context of the Puritan Reformation, Walzer slighted the specific *intellectual context* of anti-absolutist arguments developed by the Catholic conciliarists. The theoretical point is obvious: contextualism must not be confused with reductionism of the Marxist or Namierite sort.

Following the tradition of Figgis, Mesnard and others, Skinner wants to view the entire development of early modern (especially sixteenth-century) political thought as the gradual unfolding of the modern concept of the state. Thus, woven through the first volume we find the story of how anti-imperial and anti-papal arguments contributed to the modern notion of a plurality of territorially "sovereign" political units. To this Renaissance component of the



modern concept of the state, Skinner adds (in the second volume) two typically Reformation components. He tells us first how the privatization of religion led to the idea (and the reality) of an increasing concentration of power in the hands of secular rulers, and then how the religious wars in France led to the *politique* assertion that the state no longer need furnish theoretical solutions to theological conflicts, but must be satisfied with providing a *modus vivendi* among discordant religious groups.

Skinner is obviously fascinated by the problem of "meaning change," and he devotes several pages to the standard topic of how the word "state" arose from its medieval ancestor *status*. Nonetheless, he is too good an historian to wrench his material into a preconceived pattern. Instead of aggressively stamping each part of his study with the fully articulated Weberian concept of the impersonal, bureaucratic state, Skinner tends to give a great deal of leeway to each thinker and problem-constellation. He never claims, for example, that Marsiglio of Padua was trying (but failing) to come up with Bodin's juristic theory of the modern state. More than in the Whiggish works of Figgis and Mesnard, as a consequence, there exists a slack relationship between the story of the emergence of the modern concept of the state and the account Skinner gives of the twistings and turnings of early modern political thought. Rather than being a defect, however, this is a virtue—another sign that methodological scruples pay off in the application.

S. T. HOLMES

Harvard University

**The Developmental Logic of Social Systems.** By Henry Teune and Zdravko Mlinar. (Beverly Hills, Calif.: Sage Publications, 1978. Pp. 174. \$14.00, cloth; \$6.95, paper.)

**The Social Ecology of Change: From Equilibrium to Development.** Edited by Zdravko Mlinar and Henry Teune. (Beverly Hills, Calif.: Sage Publications, 1978. Pp. 296. \$14.00.)

Zdravko Mlinar and Henry Teune have been the driving force behind the Research Committee for Social Ecology of the International Sociological Association. They have constructed an ambitious research program and have stimulated others to delve more deeply into the approach of social ecology and the study of development. These two volumes are part of the product of their labors.

In *The Developmental Logic of Social Systems*, Teune and Mlinar claim to address "the most perplexing theoretical problem in the social sciences: the explanation of social change." In fact, they deal only with developmental change, admitting (p. 45) that this "may only be a small domain of the social change taking place." Nevertheless, they do have interesting, closely argued and provocative ideas concerning developmental change. Their opening sections, detailing their general stance and systems-theoretic approach, are excellently constructed, luring the reader to read on with genuine interest.

By contrasting three kinds of systems (mechanical, ecological, and developmental), Teune and Mlinar call for an analysis of the latter in their own terms rather than in the imagery of mechanical or ecological systems. Developmental systems are defined by "their capacity to generate new properties and integrate them which results in changing the nature of the components and their relationship." Systems have components, subsystems, and levels. They can vary in their structural configurations and they have properties which are distributed across components. The major focus of Teune's and Mlinar's analysis is the set of components, never adequately delineated, and the changing distributions of properties across these components. The sharing and not sharing of properties by components is pivotal in their theory. Variety is the number of subsets of identical properties. Diversity is the extent to which the sets of properties of the components are not identical. Integration is the probability that a change in one component will lead to changes in other components. Finally, development of a system is its level of integrated diversity. It is a subtle and troublesome concept to deal with.

The dynamics of structural development are articulated in a variety of "laws." Given a count of components, properties, and their distribution, there is ample scope for simple combinatorial exercises. Many of the "laws" are the (tautological) outcomes of such exercises and, as such, do not constitute social laws at all. The use of their "laws" leads to statements like this one:

The core of the structural dynamics of developmental change is that the greater the total number of properties available for distribution, the greater the variety of those properties; and the greater the number of properties distributed to each component, the greater the diversity of the total system. To the extent that this greater diversity is integrated, it will yield greater variety. The probability that diversity will become integrated increases to the extent that

greater diversity is accompanied by greater similarity. And the more diversity there is, the more rapidly variety increases. An increase in one accelerates the other . . . and the integration of diversity increases. Thus, the level of development predicts the rate of development (p. 53).

Laid over this is a distinction between coaction systems (which are not developmental), interaction systems and transaction systems. Each behaves differently, each has different dominant activities and each has different developmental dynamics. Teune and Mlinar articulate a cross-level dynamic whereby variety, diversity and integration can be moved between levels.

The ultimate point of development appears to be transaction systems whose only components are individuals. Equality abounds. Indeed, "widespread cooperation would further the transformation of interaction systems into transaction ones where properties proper [skills, knowledge, values] are not scarce and are not located in unique places in time and space. Whatever remnants of competition and conflict [that] remain would recede into irrelevancy" (p. 171). This has all the hallmarks of a naive utopian vision—only there is a theory behind it. Whether one regards the theory as viable will depend on one's views concerning clarity of expression, relevant concepts, evidence and scholarship.

Power is ignored and the idea that different components may have different interests is not really dealt with. Resources are exogenous to the theory, as is control of resources and, implicitly, resource abundance is assumed for transaction systems. Underlying the analysis is the unrealistic assumption that developed social systems are free from ecological restraint. Technology, while seen as crucial to development, is very briefly considered and inadequately incorporated into the argument.

Evidence and scholarship are virtually absent from this volume, a consequence of the authors' decision to write a theoretical treatise exclusively in terms of "their" ideas. They are concerned with testability, correctly observing that "testability is a question of measurement systems rather than the generality of the theory." But unless the components and properties are carefully specified (a mammoth and complex task), variety, diversity and integration can take any value. Teune and Mlinar sketch some of the characteristics of an appropriate measurement system but they do not provide such a system. The epilogue, claiming that they have (selectively) accumulated evidence in support of the theory, without revealing that evidence, provides cold comfort. Moreover, the

fragments that are supported (e.g., "patterns of population concentration in countries becoming developed or industrialized") could be obtained from other theories.

The absence of scholarship is, if anything, more worrisome. Initially, Teune and Mlinar do assemble, rather persuasively, their ideas on the dynamics of development. In the latter part of their volume, they then deal with the production, consumption, and use of system properties, exchange, value systems and manifestations of developmental change (like urbanization). Here many of the ideas are readily apparent in many areas of social science literature and are simply being repackaged. Relevant evidence is ignored or only casually appealed to. While the authors legitimately dismiss much of the "development literature," their work suffers from their practice of ignoring the broader literature or surreptitiously smuggling fragments of it into their argument. To use their concepts as images, they ignore the variety and diversity of the literature and thereby fail to integrate it into a truly coherent theory. There is none of the richness and interpretive detail of, say, the theoretical argument of a work like Lenski's *Power and Privilege*. Instead, there is an underdeveloped and largely unsubstantiated thesis, which is unfortunate, given the rich potential of the authors' initial ideas.

*The Social Ecology of Change* issues a clarion call for the reorientation of human ecology and ecological thinking in the social sciences. Social ecology has been moribund for several decades and the editors counsel us to take the high road of examining social systems that develop and to deal with the spatial and temporal dynamics of development within a social ecological framework. The call should be heeded.

The book is divided into four sections: (1) Social Ecology at the Crossroads; (2) Socioeconomic Processes in Space and Time; (3) Human Action and Ecological Structure; and (4) Methodological Approaches to the Study of Socio-Ecological Change. It is the product of a workshop on human ecology held in Ljubljana, Yugoslavia, in 1976. Those papers most consistent with the organizers' objectives (see *The Developmental Logic of Developmental Systems*) were collected in this volume, together with an introduction and the distillation of some of the participants' deliberations. In the main, it is a useful collection.

Mlinar's thoughtful discussion of the transformation of social ecology provides a fine foundation for the volume. The editors' distillation is uneven as it is permeated with their ideas and other ideas that do not fit well with

them. Further, as a workshop participant I see that, in the methodological segment, the editorial work strips all of the references cited by the contributors and quotes published material without acknowledgement. At the very least, this is unhelpful to the reader; this chapter should not have been included.

The remaining articles are uneven in quality and space limitations preclude individual examination of them. However, I would particularly recommend the papers by Mlinar, Trampuž and Ferligoj on the social ecology of developmental change; by Janson on the factorial study of socio-ecological change; and by Sicherl on the dynamic measurement of disparities. The workshop could have a dramatic impact upon social ecology, but whether that impact will be within the conceptual framework advanced by Teune and Mlinar remains to be seen.

PATRICK DOREIAN

*University of Pittsburgh*

**Psychoanalytic Politics: Freud's French Revolution.** By Sherry Turkle. (New York: Basic Books, 1978. Pp. x + 278. \$12.50.)

This is a first-rate book. Sherry Turkle handles a vast sweep of ideas with assurance. Her reconstruction of the arcane thought of Jacques Lacan is fair and subtle and, above all, lucid. The book is fascinating to read, placing the theoretical ideas within the drama of their historical and cultural setting.

Since the May 1968 student uprising, psychoanalysis has taken a major place in the theory and practice of the French Left—and in French popular culture as well. How did this come to pass in a society which was resolutely anti-psychoanalytic? How was a radical current in psychoanalysis discovered when the mainstream of that discipline is so conservative?

These are some of the questions Turkle poses. In providing the answer, she gives us a panoramic view of French society and intellectual life today and places them in historical context. Focusing more narrowly, Turkle reconstructs the role of psychoanalysts themselves, and of their ideas, during the May 1968 events. She follows these beginnings through the subsequent development of a psychoanalytically influenced radical ideology. She traces the impact of this ideology in the university, the mental hospitals and the psychoanalytic societies, basing these institutional studies on interviews which enliven the text. Enough detail is provided for us to grasp the labyrinthine quality of political life in these settings.

At the center of all of these developments stands the figure of Jacques Lacan, whose charismatic leadership, as well as his novel formulations, have given psychoanalysis such a prominent place on the French Left. American readers may have already encountered Lacan as a representative of French structuralism, as an influence on the Marxism of Louis Althusser, and as one of a number of major innovators in contemporary psychoanalytic thought.

Turkle's approach of examining the thought in the context of Lacan's life and times is particularly apt in this case. Lacan's ideas—and their influence—cannot be appreciated through the published text alone. His public life as a trainer of analysts, a founder of institutions, and a lecturer whose persona is itself part of his message must also be understood.

Turkle shows respect and understanding for Lacan's cryptic mode of communication through which he attempts to speak directly from and to the unconscious. Yet she speaks in her own straightforward and lucid fashion. The result is easily the best I have seen among a number of expositions of Lacan's ideas in English. Her epilogue, "Lacan in America," is a particularly helpful interpretation and completes some of the ideas presented in chapter 2.

Turkle sees Lacan's insights into the "presence of social forces within the individual" as having genuinely radical political import—as restoring to psychoanalysis its original significance as a "subversive science." When she sees the profound difficulties and paradoxes which have developed in the Lacanian movement, she interprets these as universal dilemmas for any psychoanalytic politics. Since she has joined the Lacanians in viewing American psychoanalysis as totally preoccupied with adaptation, she sees no alternatives from that quarter. (Nor does she make any mention of the British "object relations" school.)

It is my belief that there are profound new currents (and old ones being rediscovered) in psychoanalysis and humanistic psychology which escape some of the paradoxes inherent within Lacan's system. The political implications of these psychoanalytic views have not yet been fully articulated, but efforts in that direction are beginning to appear. These efforts involve an appreciation of: recent American work in ego psychology such as that of Mahler, Kohut, and Kernberg, or of Loevinger and Kohlberg; the work of the British "object relations" school; and the insights of Reich (and his student Lowen) on character analysis and muscular armoring. The combination of these psychoanalytic perspectives with insights from feminism and Marxist humanism offers

the promise of a psychoanalytic radicalism that is a genuine alternative to that of Lacan.

WILLIAM R. CASPARY

*Washington University*

**Commitment and Change: Georges Sorel and the Idea of Revolution.** Essay and translations by Richard Vernon. (Toronto: University of Toronto Press, 1978. Pp. viii + 148. \$15.00.)

Historians of political theory have not done well by Georges Sorel (1847–1922), who has been more talked about than read, labeled more often than understood. In part this springs from the bewildering range of political positions he espoused during his lifetime. Additionally, few of his writings have been easily available and, until quite recently, the only work available in English was his *Reflections on Violence*. Within the past few years, however, a serious attempt to understand Sorel in depth has begun to take hold. Richard Vernon's book is an important, if incomplete, addition to this literature. The main topics he discusses are Sorel's conception of revolutionary commitment and change, his reflections on the relationship between thought and action, his critique of Marx, the influence of Bergson on Sorel, and Sorel's location in the history of political thought.

This is, however, a curious book, exciting and disappointing at the same time. In large part, this is because of its form—a 70-page essay followed by 50 pages of extracts from Sorel's untranslated essays, prefaces, and books. Neither separately nor together, however, do they provide a thorough examination of Sorel's conception of revolution. Nevertheless, the essay is one of the most sophisticated and illuminating discussions of Sorel that has appeared in print, and it is a shame that Vernon did not see fit to develop it more fully. He suggests, for instance, that the basis of much of Sorel's thought is a form of conservatism, with emphasis on order, decentralization, and morality, an "ethics of association embodied in a concrete way of life" (p. 61), and it is this

that provides an underlying unity to much of Sorel's thought. Yet the form and content of Sorel's morality is hardly explored; indeed, the terms Sorel uses—"the heroic," "the sublime," "regeneration"—are not discussed at all. To take another example, Vernon argues that Sorel's conception of the disjunction between knowing and acting is central to his understanding of the problematic nature of revolutionary commitment and change. Yet he never attempts to assess the validity of Sorel's contentions.

Sorel is important, Vernon thinks, for several reasons. He anticipates some of the most effective critiques of Marxism, his writings are a sustained meditation on the relationship between theory and practice, and he provides us with something that is missing from contemporary thought, "a chastened and sceptical view of revolution" (p. 71). Sorel, moreover, is a figure who has been much misunderstood, particularly by those who depict him as an "irrationalist." The term, Vernon argues, is ambiguous, and has foreclosed too many questions. Sorel was "much more than has generally been thought, a man of reflection, who denied, however, that the stable and unitary constructs of reflection had any secure place in the essentially multiple world of action" (p. 71). He emerges, too, in Vernon's account, as a "clerc of violence, preoccupied with the processes of contemporary change but denying that his own preoccupations could or should enter into the processes themselves" (p. 71). But Vernon is unwilling to leave it at that. If Sorel is to be located within the history of political thought, it can only be in a French tradition "with an essentially moral emphasis," one which is "acutely distrustful of the notion of sudden transformations leading to new and unknown orders" (p. 61). For Vernon, Proudhon was Sorel's precursor, Camus his successor.

The translations add little to the essay and are too brief to be helpful to anyone coming to Sorel for the first time. Indirectly, however, they point up the need for a complete critical edition of Sorel's works.

ROGER W. SMITH

*College of William and Mary*

## American Politics

**Television Network News: Issues in Content Research.** Edited by William Adams and Fay Schreibman. (Washington, D.C.: School of Public and International Affairs, George Washington University, 1978. Pp. 231. \$6.00, paper.)

*Television Network News: Issues in Content Research*, edited by William Adams and Fay Schreibman, takes one step toward informing political scientists of recent work about television news research and methodology. Five of the articles are written by political scientists, seven by communication specialists, two by historians, and one by an instructional media specialist. The essays reflect this diversity. The volume's explicit purpose is to "bring together into one forum discussion of some of the more important issues in the study of network news content" (p. 5).

The first section purports to discuss an overview of research about news. The initial essay summarizes studies about production, content, and effects research. The first involves work about the determinants of news content during production, while content research analyzes patterns of manifest content. Effects research investigates the social and political consequences of news. One may dispute some of the conclusions in this essay, such as the assertion that "agenda-setting has become the most theoretically-developed and empirically-tested distinct area of impact research" (p. 30), but Adams provides a credible and useful outline of an enormous body of interdisciplinary research.

Comstock and Cobbey argue that television has "heightened the urgency" of problems citizens face in evaluating news (p. 48). There are few concrete standards which the public can apply in evaluating the medium. Paletz and Pearson review six recent books about network and local news which concern either news organization, content, or impact. On the basis of a lucid, perceptive analysis of the studies, the authors argue that greater emphasis should be given to some largely ignored areas of research, including uses and gratifications associated with video news. Nowhere, however, is a rationale given for the selection of these particular works, all published between 1973 and 1976.

A second section concerns research methods and resources. Schreibman provides a particularly valuable survey of news collections housed in several television archives. Each is described

in terms of the range and scope of news holdings, access, procedures of use, costs, and organizational goals. The holdings of the oldest and richest source, the Vanderbilt Television News Archive, which includes the Television News Index and Abstracts, provides an example of how each is treated. Also discussed are special services, modes of access, fees, videotape format, and organizational aspirations. Lichty and Bailey present "practical advice" about content analysis. Having assessed some "wrong reasons" for conducting content analysis, such as trying to infer "effects" rather than manifest content and proving accusations rather than testing hypotheses, the authors discuss concerns of sampling, defining units of analysis, forming categories, executing coding procedures, and analysis and presentation of data. Points are illustrated by examples from published literature. While the essay is informative and useful in developing studies, research scholars will find little in it that is new.

Culbert discusses the value of idiographic approaches in contrast to nomothetic approaches employed by political scientists interested in building models. Understanding visual images presented in television news, he argues, is particularly important to the "fullest possible comprehension" of past events (p. 140). However, Culbert's apparent denial of selectivity in his quest for "complete comprehension" of visual images of the Loan execution in the broader context of Vietnam is puzzling. How can knowledge be drawn from any set of events, visual or otherwise, without selectivity and generalization?

Deploring the tendency to ignore visual characteristics in television news research, Adams describes examples of how visual images have been included in content analysis, and how image content may be determined by events, production, and the interaction between visual and audio aspects of programming. He raises some particularly interesting questions about the isomorphism between film and text in news coverage.

The final section discusses future directions of news research. Patterson urges that content analysis become more theoretical and that greater attention be devoted to scientific adequacy. Attention should be paid not only to reliability and validity in coding but also to research designs which emphasize comparisons among different forms of media at different times.

Gerbner and Signorielli assert that their "new, realistic, and more appropriate framework" will improve research about television news (p. 189). They argue that research about news should employ content and impact categories which are suitable to explicitly entertainment programming.

We are, however, left wondering whether most analysts are not aware of many of the problems cited.

Robinson also argues for a new direction, if a "reactionary" one, in television research. He lays out four interpretations viewed as alternatives to Epstein's organizational theory of news. Having reviewed the "newsworthy" interpretation, the "reality" interpretation, and the "collage" interpretation, Robinson elaborates what he calls the "attitudinal-political" interpretation, which assumes that the political attitudes of newswriters and reporters influence news content. Robinson argues for the value of analyzing news about less structured, less clearly "political" events.

Although of interest to social scientists concerned with television news research, these essays have a protean character, not unlike the alleged character of individual viewers of network news. Little binds them together except a concern with TV news. The preface serves little purpose, and no introductory material ties the contributions together. Moreover, the essays are of uneven difficulty, scope, and sophistication; still, the volume should be read by students of television news.

C. RICHARD HOFSTETTER

*San Diego State University*

**The William O. Douglas Inquiry into the State of Individual Freedom.** Edited by Harry S. Ashmore. (Boulder, Colo.: Westview Press, 1979. Pp. xvii + 250. \$15.00.)

This volume is the result of a project of the Center for the Study of Democratic Institutions to monitor concerns of individual freedom that marked the career of Justice William O. Douglas. It consists of 11 papers, ranging in length from 8 to 38 pages. Most, if not all, of the authors had served as law clerks to Justice Douglas.

Each contribution focuses on an aspect of one of four general organizing themes: (1) "Fundamental Freedoms: Areas of Human Behavior and Personality Immune from Governmental Interference"; (2) "Freedom and Law Enforcement"; (3) "Freedom and Governmental Expenditures to Advance the Common

Good"; and (4) "Freedom and the Duty of Government to Control Private Power."

Each essay typically attempts to survey the present situation. Problems and dilemmas are posed. An occasional alarm is sounded. Questions are raised, and sometimes guidelines are provided for a preferred approach to the issue of freedom under scrutiny. Court cases, governmental actions, laws, and current bills comprise the principal sources for the studies. Developments into 1978 are covered.

The nature of the topics may be gathered by an example from each of the four theme areas. Leonard B. Boudin contributes a brief paper on "Freedom of Inquiry in the Sciences," dealing with the tensions between the principle of unrestricted scientific inquiry, on the one hand, and the realities, on the other, of governmental regulation and increasing apprehensions raised by genetic and other controversial research. Boudin argues that "while the First Amendment precludes regulation of research, it permits regulation of the technological aspects (including the methodology) of the research" (p. 40).

In a concise chapter, Jerome B. Falk, Jr. explores the erosion in the law of searches, dealing with such recent cases as that of the *Stanford Daily* (in which he was counsel) and such situations as the screening of airline passengers and prospective thefts of plutonium.

In a lengthier, well-organized paper, William A. Reppy, Jr. thoughtfully summarizes and analyzes problems and issues of governmental funding as they bear on academic freedom. And, as a final example, Robert B. McKay raises stimulating questions "in the search for a proper balance between professional freedom and public control" (p. 203) as he examines issues of ethics and the law, medicine, the military, accounting, and journalism.

Other chapters examine the tension between the right to privacy and the right to know, control of the intelligence agencies, private discrimination, access to the media, governmental investigations, protecting persons from themselves, and the rights of those working for governments.

As intended, the volume is an "inquiry," a present inventory of aspects of individual freedom in a rapidly changing society bound by the Bill of Rights. It is addressed to the lay reader. A major purpose is to further public discussion. Portions will date quickly because some of the writers deal with current matters such as bills in committees. Documentation of some papers is elaborate; for a few it is scant.

Much of the material is familiar to political scientists and fairly elementary. For a political

scientist the book's usefulness lies mainly in the provocative questions raised, some of which suggest intriguing research possibilities.

HOLBERT N. CARROLL

*University of Pittsburgh*

**Leadership in America: Consensus, Corruption, and Charisma.** Edited by Peter Dennis Bathory. (New York and London: Longman, 1978. Pp. vi + 200. \$6.95, paper.)

This book draws on several independent studies. Bruce Miroff's chapter on JFK rests on a thesis; Dennis Hale's on Curley comes from a book on the Boston mayor. W. C. McWilliams' account of LBJ is the judgment of a mature political scientist conversant in detail with Johnson's politics. Rev. W. K. Smith is a thoughtful disciple of Martin Luther King, Jr. Yet while each contributor has his own bent, the book derives its unity from a meditation on Tocqueville. From him the authors have learned that one thing is needful: to create a form of political association that will educate the majority in the art of self-government. This task governs the choice of subjects for the nine essays in political biography that comprise the body of the volume. These amplify Bathory's contention that this task has been since 1896 the central problem of American politics. Tocqueville also provides a key to the book's thematic terms. Thus we may equate *corruption* with what he termed *individualism*. Bathory teaches that Mark Hanna's victory over Populism was the product of a comprehensive strategy to foster individualism. H. A. Plotkin argues that the managerial consultant P. Drucker has promoted individualism rather than Tocqueville's art of association, discouraging corporation managers from engaging in politics. By recovering Tocqueville's quest for associative forms to replace the New England township, the authors hope to bring political science back to Tocqueville's intention, to promote sober and reasonable deliberation in democratic politics.

One may doubt that the approach adopted is suited to this last task. For political science might well contribute more if its best recent efforts were properly interpreted. Bathory and his contributors fail to supply a comprehensive analysis of the predicament of our proper leaders, the duly elected ones. Such an account might start from recent controversies over reapportionment, the voting age, the electoral college, campaign financing, the rules governing party primaries, and the expansion of the judiciary's purview. What Tocqueville called the

*forms* of American democracy, the laws and customs in and through which the majority governs, have been under attack since he wrote. The egalitarian and individualistic movement he warned of has swept away one after another of these forms, always in the name of democratic reform. The authors are aware of this, although they do not trace the history of the unforming of American democracy in detail. But they do not remark how the pace of demolition has increased during the last 30 years; nor do they draw the consequences for political leadership.

That one should may be suggested by a glance at Marc Landy's fine essay on George Meany. He writes with more sympathy than the other authors can muster for Madison's political science. By adopting Madison's standard, Landy defends Meany by our most respectable criteria; moreover, our elected representatives fail while Meany passes. Landy argues persuasively, in two notable cases, when Meany endorsed the admission of refugees from Vietnam, and when he crushed local union resistance to busing, that Meany was just what Madison sought by means of representative institutions: a voice bringing liberality, tolerance, impartiality and reason into public deliberations. Yet Landy never weighs the effect on representative government of the part of Meany's strategy for building a national union which Madison would have thought most problematic. The drive to unionize public employees, combined with the use of labor voting strength to punish Congress members who opposed it, is not assessed in its effect upon the impartiality or rationality of elected officials. This failure to carry through Madison's science of representative government may point to a larger problem.

Bathory begins by comparing Tocqueville with Madison, for whom political science was to introduce impartiality into democratic politics. Tocqueville rejected Madison because "he feared a world that had lost a sense of the possibility of strong commitment to anything. . ." (p. 19). Because commitment rather than impartiality is for Bathory and most of his contributors the crux of democratic civility, they depart from Tocqueville's reliance upon judicial forms, upon jury duty as a means of political education, and upon the legal profession, to introduce respect for law, sobriety, and impartiality into democratic politics. This departure may reflect a deeper reservation about Tocqueville. Bathory's objection is one that led Weber to make liberal principles the object of a leap of faith. But Tocqueville still knew science as the ascent to first principles, to "the light by which we are guided," and thought he was teaching the secure principles of republican

government. If we could see our way clear to such principles, we could be liberals without blushing, without guilt, even without Rawls; but like the authors of this volume, we think we cannot. This is consequential, for principles are needed to bring liberal democracy to light as a whole one which we can pass judgment. Tocqueville had exhibited a distinctive regime in which the majority ruled through definite forms; what was missing in America but necessary to make democracy liberal and thus choiceworthy was supplied by him by deduction from principle. By contrast, *Leadership in America* is silent about liberal democracy as a whole. It confronts us with fractious parts, leaving us like delegates at a party convention to form them into a whole. If we strain our eyes, the most we might discern in these chapters is the possibility that with proper leaders the unions, cities, and a national party might perform Tocqueville's task of "transforming a faceless mass into a citizen-public" (p. 190). We may even glimpse a vision of how the diverse constituencies of the Democratic party might be formed into self-governing parts and combined into a working whole. But we cannot see the relation between this partisan whole and the liberal democratic whole which Tocqueville delineated. In their convention the Republicans too think that they are putting the country together.

*Leadership in America* seeks to bring political science and political leadership together in the quest for a democratic civility, robust enough to withstand the rough and tumble of American politics, yet noble and difficult enough to make liberal democracy choiceworthy once again. It is an exceptional introductory text; the authors convey their most serious reflections on what they regard as the most important issues, but with an experienced teacher's respect for what the newcomer can grasp. Without condescension or jargon, the book is dramatic, engaging, controversial and nicely brief. If it is widely taught, as it deserves to be, it should compel us to ask whether liberal democracy can indeed be choiceworthy, and whether we scholars can in good conscience assert our democratic liberalism. It compels me to ask because the authors do not, strange to say, although they appeal to Tocqueville, who taught us that we could.

ROBERT EDEN

Dalhousie University

Berger. (Cambridge, Mass.: Harvard University Press, 1977. Pp. x + 483. \$15.00.)

The thesis of this book is simple—under the guise of "interpretation" the Supreme Court has revised the original meaning of the Fourteenth Amendment, has substituted its will for law and thus has contributed to the destruction of constitutionalism and the rule of law in America. The meaning of the Fourteenth, which the Court has consistently misconstrued (and through which most modern constitutional adjudication flows), is not, to Raoul Berger, so difficult to discover. In fact, the intent behind the amendment was rather clear, even unequivocal: "The key" to the meaning of the constitutional text "is furnished by the immediately preceding Civil Rights Act of 1866, which, all agreed, it was the purpose of the amendment to embody and protect" (p. 20). Designed "to embody or incorporate the Civil Rights Act," the Fourteenth would secure that act against repeal by a subsequent Congress and make it safe from judicial repudiation.

But the Civil Rights Act of 1866 (and hence, in this view, the Fourteenth) had "patently limited objectives," among which were the right to inherit, to purchase and sell real and personal property, to make contracts, to sue and be sued and to give legal evidence (pp. 24–25). It did not reach nor desire to reach the protection of any political or social rights, and surely not the right to have all votes equally weighed or to attend integrated schools. Thus almost all of what passes these days as constitutional "interpretation" is nothing more than unlawful "amending" by the Court, the substitution of its views for that of the framers and of the sovereign popular will as expressed constitutionally—in brief, government by judiciary.

It would certainly not be fruitless to reexamine the historical data and review the accuracy of Berger's historical understanding. Yet the truly important issues raised by this book become clear if we grant, *arguendo*, the adequacy of the historical investigation and concede that the broad wording of the Fourteenth Amendment had as its direct and immediate intent simply the validation of that early Civil Rights Act. What, then, does it mean to hold that the life of constitutional law is neither logic nor experience but, rather, careful legal history? On one level it is probably beyond controversy that the framers of the Fourteenth did not narrowly "intend" the immediate desegregation of all public schooling, the ultimate destruction of all malapportioned legislatures or the full application of the Bill of Rights to the states when they wrote the amendment (any

**Government by Judiciary: The Transformation of the Fourteenth Amendment.** By Raoul



more than the Founders, when they gave broad powers and ample means to Congress were in favor of establishing a national bank; in fact, they might well have been against it). But, contrary to Berger, this does not overcome the fact that what the framers of the Fourteenth Amendment *did* (knowing full well, no doubt, the already rich history of the other general phrases in the Constitution) was to lay out in the Constitution not specific rules or objectives but broad *principles*: "privileges" and "immunities," "due process of law" and the expansive right to "the equal protection of the laws." Thus, that the framers did not openly contemplate granting blacks the right to serve on juries equally with whites (a right which the Court recognized in 1879 in the *Strauder* case and which Berger, looking at the debates of 1866, deems "wrongly decided," p. 412) is both true and irrelevant. For what the writers of the Fourteenth did—one might even say intended—was to lay out bold principles of justice and constitutional right and embody them in a document which, to use Marshall's words, was meant to endure for ages to come. Even Justice Harlan, whom Berger rather consistently praises, could say in *Duncan v. Louisiana*, "The very breadth and generality of the Amendment's provisions suggest that its authors did not suppose that the Nation would always be limited to mid-19th century conceptions of 'liberty' and 'due process of law' but that the increasing experience and evolving conscience of the American people would add new [meanings]" (391 U.S. 145, 175; cf. Berger, p. 259). For each generation to apply the great principles embodied in the Constitution to new and varied objects and situations is not, as Berger believes, to make shambles of constitutional law. Rather, it is to act in the only way appropriate—to use the principles and goals of our fundamental law as directive and binding guides to our ever-changing condition as a nation.

Not only may constitutional formulations develop in response to *new* situations but also the internal logic of general principles of law and justice often lead us quite appropriately to reconsider and reevaluate situations prevalent in the past. That demanding "equal protection of the laws" for all citizens was never "thought" by the framers (and therefore could not have been "intended" by the framers) to apply to the status of women can be no final bar to its application in that area once the principle is finally and fully thought out. That the liberal egalitarian principle of the Declaration of Independence was not applied to slaves in 1776 can still mean, as Lincoln noted, that as the

principle "broadens and deepens its influence" that it will prove to be incompatible with the very human servitude which gave Jefferson the time and opportunity to formulate the principle itself. That ideas do have consequences—and consequences which may go beyond or even against the immediate objects which gave birth to those ideas—is a fact having application well beyond the confines of constitutional adjudication. And while the Court can surely mistake the meaning of a broad constitutional principle (as in *Slaughterhouse*) or even subvert it (as in most of the racial cases of the 1880s), the Court's greatest function and perhaps its only real justification may be that it can help us, over time, elucidate our principles, apply them to new situations and give those fundamental principles the opportunity to unfold themselves in history. That the framers of the Fourteenth Amendment did not, in 1866, give to a branch of the central government the authority to overturn state malapportionment or to desegregate public facilities is obvious; that the principles written down in 1866 may never, therefore, demand those results is not so obvious.

Clearly, the line separating the proper application of constitutional principle and unwarranted constitutional revision is often narrow and the guideposts unclear. And, despite the basic thesis that the Court should be little more than a conclave of nine legal historians, Berger's book understands and depicts the real dangers involved in granting to the Court the unlimited right to fashion constitutional doctrine simply at will. The growing number of books and articles concerning the "imperial judiciary" attests to the fact that a reexamination of the proper role of the Court in a constitutional democracy is now both appropriate and necessary. The time surely has come for wide-ranging analyses of the Court in modern American life—investigations which will speak candidly about the ways in which the judiciary may have entered areas in which it has no experience, no ability or no legitimate business. The time has also come—and here Berger's instincts are fully correct—for a body of scholarship which goes deeper than the simple portrayal of the Constitution as a "living document" as if it were merely flexibility itself, without principle, meaning or solid purpose. But this analysis, which attempts to criticize the Court through an understanding which freezes the Constitution to certain historical particulars and no more, simply will not do, for it does not speak candidly about the Court as much as badly about the Constitution.

JOHN AGRESTO

National Humanities Center

**Big Story: How the American Press and Television Reported and Interpreted the Crisis of Tet, 1968 in Vietnam and Washington.** By Peter Braestrup. (2 vols. Boulder, Colo.: Westview Press, 1977. Pp. 740 + 706. \$45.00; Garden City, N.Y.: Anchor, Doubleday, 1978. Pp. xviii + 606. \$8.95, paper.)

*Big Story* argues that the American news media gave the public a basically false picture of the Tet offensive. Peter Braestrup attempts to show that the news media overreacted to the Tet crisis and presented it as a disaster for the U.S., when in fact it was a defeat for North Vietnam and the NLF.

Braestrup was himself a Vietnam correspondent for the *Washington Post* during Tet, and the strengths and weaknesses of *Big Story* are in many ways parallel to those of the news media he is writing about. As a gatherer of facts, Braestrup is thorough indeed. *Big Story* is based on extensive interviewing and a reading of almost the entire Tet coverage of the AP and UPI, the television networks, the *New York Times*, the *Washington Post*, *Time* and *Newsweek*. However, Braestrup's analysis has serious flaws.

Braestrup's basic observation about media content is correct: the news media did indeed present the Tet offensive as a failure of U.S. policy in Vietnam. But his style is so unremittingly contentious that Braestrup seriously overstates his points. With each example he lets his readers know he is scoring a point, and he often presents the "exceptions" with the same triumphant tone, as though they somehow proved his argument as well ("CBS managed . . . without . . ."). If one reads Braestrup's material carefully, one sees that it is often so mixed that his simple characterizations are clearly inadequate.

Braestrup's major criticism of the news media also contains a kernel of truth. The thesis that Tet was, at least militarily, a defeat for North Vietnam and the NLF is more plausible than most people realize, and the media did often make sweeping statements about the significance of Tet and presented dramatic incidents as though they somehow contained the "meaning" of Tet in microcosm, all with little solid analysis to back them up. But Braestrup himself treats complex questions of historical interpretation as though they were as simple as getting it straight whether the NLF did or did not actually enter the U.S. Embassy building on the first day of the offensive. "There is fairly broad agreement among historians today," Braestrup maintains, "that Hanoi suffered a military setback in the 1968 Tet

offensive" (p. 119). But historians have barely begun to scratch the surface of the Vietnam war, and Braestrup does not cite a single historian to this effect. When historians do begin to analyze the Vietnam war, it seems likely that there will be disagreement about whether the question of "who won" the Tet offensive is even an important one to ask. After all, the U.S. was always able to kill more of the "enemy," hold any territory it chose, and keep the initiative militarily, yet North Vietnam and the NLF always seemed capable, politically and militarily, of continuing the struggle. Braestrup criticizes the media for getting the "score" wrong; it never occurs to him to question whether "scorekeeping" (which was always a top priority for the media) could really give people an adequate understanding of the Vietnam war.

Braestrup is always quick to tell his readers what he does and does not consider good journalism, but he is not always willing to spell out his reasons. He disapproves, for instance, of the attention the media gave to civilian casualties, calling it "old-fashioned appeals to the emotions" (p. 209), and "compassionate sensationalism" (p. 217). But he offers no justification for this view, except in purely personal terms: "As a former Marine in Korea, I was perhaps less shocked by war's waste and destruction than were my colleagues who were experiencing these for the first time" (p. xvi).

Braestrup's work suffers from his lack of knowledge about other research on the media. He presents Tet coverage as an "extreme case," and offers a jumble of miscellaneous explanations for it. In fact, it is easy enough to see the routines of news coverage at work during Tet. To take one brief example, the news media normally focus on day-to-day events rather than underlying processes. This means that a crisis like Tet almost always comes across as a sudden reversal in the course of events.

DANIEL C. HALLIN

*University of California, Berkeley*

**The Presidential Experience: What the Office Does to the Man.** By Bruce Buchanan. (Englewood Cliffs, N.J.: Prentice-Hall, 1978. Pp. ix + 198. \$8.95, cloth; \$3.95, paper.)

Taking his title seriously, Bruce Buchanan sets about to fill gaps in our understanding of the presidency by providing a comprehensive and systematic description and assessment of the psychological environment in which the occupants of the presidential office live and

work. That milieu is pictured essentially as a constant one characterized by stress, deference, dissonance, and frustration. According to the author, these four acids of the presidential experience continually erode the moral habits and methods of truth-testing which incumbents bring to the office.

Drawing on the results of research in social psychology, Buchanan explores the behavioral consequences which may be expected in such an environment. Among those identified are tendencies to aggression, artificial inflation of one's own importance, weakening of truth norms, preoccupation with winning despite the cost, misinterpretation and misjudgment of people and events, reduction in physical and mental vigor, and delusion. Examples of each are found in the actions of twentieth-century presidents, leading Buchanan to conclude that the presidency as currently structured is a threat to political stability, democratic integrity, and effective administration.

The work concludes with two chapters which examine potential remedies. One investigates character-assessment as a device we might use to select presidents most likely to withstand the pressures of the environment. Drawing on self-concept theory and research, Buchanan provides valuable support for the notions developed by James David Barber in his paradigmatic book, *The Presidential Character*. Still, Buchanan will not confidently rely on character as a protector of his central values, for he is convinced that "on matters of profound personal importance to them . . . active-positives tend to behave like active-negatives" (p. 146). Thus he turns in the final chapter to institutional reform, but his conclusions are less than promising. The adoption of fundamental changes which might radically improve the environment, such as a plural presidency, is improbable. Those which might have some real political support, such as legislative question periods or censure motions, might reduce the effects of deference on the occupant of the Oval Office, but they would likely increase both stress and frustration.

*The Presidential Experience* is not without error. Using the models of staff organization identified by Richard Tanner Johnson in *Managing the White House* as his "Rorschach test" of a president's response to stress, Buchanan concludes that the formalistic model is a dangerous method of coping with stress and one which reveals a chief executive's inability to tolerate interpersonal conflict. While noting that an active-positive president might sometime "fall back on such an arrangement out of self-defense," the author contends that only

Nixon and Eisenhower among modern presidents have implemented such a model (p. 45). In fact, however, Johnson described the Truman administration as a formalistic one (p. 51 ff.).

The more serious problem with this work is one which seems characteristic of such studies. Buchanan cites behavior as evidence illustrating his notions, but this evidence is not the result of systematic examination of presidential actions. This is particularly disappointing because Buchanan's notions are richer in their implications than the use of the data suggests. For example, he repeatedly emphasizes that the continual press of the four elements in the psychological environment makes them likely to produce aberrant behavior (pp. 25, 46, 58, and passim). That would indicate that instances of such behavior should occur, or should occur with more frequency, later in a president's tenure. Buchanan cites Roosevelt's court-packing plan as a major instance of preoccupation with winning despite the cost (pp. 111-16). Such action at the outset of the second term invites attention to subsequent behavior. The invitation is not accepted in this instance or elsewhere.

Despite these criticisms, *The Presidential Experience* is an important book which will provoke controversy among scholars and students. Written with style and energy, it is stimulating in its contemporary relevance, in its use of research findings from another discipline, and in its relationship to ideas developed by classic works on the presidency. Most important, it continues a line of research which, despite what may well be inherent limitations, deserves further pursuit. Buchanan has done us a service.

CHARLES H. MCCALL

*California State College, Bakersfield*

**Decision for the Democrats: Reforming the Party Structure.** By William J. Crotty. (Baltimore and London: Johns Hopkins University Press, 1978. Pp. viii + 318. \$16.50.)

This book has two main ingredients: descriptions of the origins, activities, and products of the five "reform commissions" established by the national Democratic party between 1968 and 1975; and evaluations of those products.

The descriptions focus mainly on the McGovern-Fraser Commission (147 pages) and the O'Hara Commission (74 pages), with the other commissions—Mikulski, Sanford, and Winograd—mentioned only briefly (15 pages for

the lot). But the descriptive *methods* are the same for all five. Crotty's accounts and explanations of events are based almost entirely on the public and semi-public record—commission hearings, memoranda, and reports; convention proceedings; newspaper accounts and commentaries. He mentions a few interviews with commission members and staff, but makes almost no use of them. This is particularly surprising in view of the fact that Crotty, as a consultant to three of the commissions, had unusual opportunities for gathering less accessible, and usually more illuminating, information. Even more surprising is the fact that he makes no use whatever of the massive collection of data from interviews with the delegates to the 1972 Democratic convention conducted by the Russell Sage/Twentieth Century Fund project of which he was one of the five principal investigators. He also does not mention the major book by Jeane Kirkpatrick based on those data.

Crotty's approach makes his book useful mainly as a convenient chronology of events and a compendium of names, dates, and occasional tabular material. Because he relies so heavily on what he has read, his analysis of why the commissions did what they did floats on the surface of events and his explanations are generally unconvincing.

This failure is most apparent in the frequent evidentiary and analytical gaps in his account of the McGovern-Fraser Commission, which occupies nearly half of the book. This judgment is not based on any inside knowledge I acquired as a member of that formidable commission—indeed, with even greater access than Crotty's at the time, I saw only the same visible minor fraction of the iceberg. But I have recently been reading Byron Shafer's Berkeley doctoral dissertation on the formation and implementation of the commission's rules. Shafer's analysis rests mainly on in-depth interviews with over 150 of the principal actors involved, and, while it would be unfair to fault Crotty for not having read Shafer, I must note that their analyses diverge at many important points and that in each case Shafer's is more convincing.

For one example, Crotty describes the commission's activities almost entirely in terms of what various commissioners said and did; Shafer shows that the staff played a highly active and partisan role and powerfully influenced the outcomes. For another, Crotty cannot bring himself to believe that an old pol like Lawrence O'Brien really favored the reforms or helped them very much; Shafer demonstrates that O'Brien not only favored them but played a critical role in getting the state parties to

comply with them.

I have no comparable way of checking Crotty's accounts of the other four commissions, so I will note only that they all rest on similar kinds of evidence and analytical methods.

Crotty's evaluations of the commissions' products is highly favorable: they "managed to breathe new life into moribund party structures" (p. 272). This, of course, runs sharply counter to the view of some other political scientists, including me, that the new rules have contributed greatly to what Jeane Kirkpatrick aptly calls "the dismantling of the parties." Our differences with Crotty in this regard are probably of no great scholarly interest but the *grounds* for them may be instructive.

The critics of the new rules condemn them mainly because of their *perceived consequences*—for example, the proliferation of presidential primaries, the elimination of candidate-vetting by leaders and activists with a prime and continuing commitment to parties rather than to issues or candidates, and the consequent reduction of the parties to little more than passive arenas for conflict among entrepreneurial candidate organizations and special interest "caucuses."

Crotty does not defend the commission's "reforms" by attacking the veracity of such statements about their consequences—indeed, he barely mentions them. Instead, he praises the rules' *intrinsic fairness*, their increased commitment to what he tells us are his own prime values of openness, participation, and procedural fair play.

The differences come into sharpest focus in the two sides' very different evaluations of the 1972 national convention, the first major product of the McGovern-Fraser rules. The critics say it was extremely unrepresentative of ordinary Democrats; indeed, Kirkpatrick concludes from her interview data that the policy views of Democratic identifiers were much better represented by Republican delegates than by their Democratic counterparts. Crotty, as noted above, makes no mention of either the data or Kirkpatrick's findings. Instead, he presents a table (4.4) showing that the 1972 delegates included many more blacks, women, and youths than did the 1968 delegates.

The critics say that the convention's unrepresentative character led directly to its nomination of McGovern, whom they regard as the worst candidate and biggest loser in the party's history. Crotty agrees that McGovern ran a "hopelessly inadequate campaign," and that "many party regulars and state organizations never really took the national ticket seriously"

(p. 222); but in his view that was significant only because it gave the anti-reformers an excuse to launch a post-election campaign against the excellent new rules.

In sum, the rules' critics think that the 1972 convention was a disaster. Crotty concludes (p. 149) that "the delegates . . . were orderly, self-disciplined, and intensely concerned" and that "the mood, the sense of order, the dedication to work, and the outcomes represented a polar extreme from the unhappiness that marked the [1968 convention]."

Who, then, is right about whether the post-1968 "reforms" are good or bad? The answer, it seems, depends upon which of two venerable maxims one chooses as a guide: "By their fruits ye shall know them," or "Virtue is its own reward."

AUSTIN RANNEY

*American Enterprise Institute  
for Public Policy Research*

**The Powers That Be: Processes of Ruling-Class Domination in America.** By G. William Domhoff. (New York: Random House, 1978. Pp. xv + 206. \$10.00.)

For many years, G. William Domhoff's wit, insight, and engaging prose style have given radical scholarship in America what little attraction it possesses. In *The Powers That Be* Domhoff continues to develop his thesis of "ruling class hegemony"—a thesis which he first set forth in *Who Rules America* (1967) and *The Higher Circles* (1970) and elaborated on in *Fats Cats and Democrats* (1972), *The Bohemian Grove and Other Retreats* (1974) and *Who Really Rules* (1978). Of course, all of these works directly challenge the prevailing pluralist ideology of American political science. But Domhoff remains unrepentant: he continues to view American politics as a struggle between two social classes whose interests are rooted in the organization of production—a ruling class which owns and manages capitalist enterprise and a working class which does not.

Domhoff is unimpressed with the traditional pluralist argument that power can only be understood as a "process." He would be satisfied to infer power from "outcomes"—disproportionate wealth and income in the hands of the upper social class; their higher standing on a variety of measures of the quality of life; their control over major social and economic institutes of society; and their over-representation in government. But in *The Powers That Be* Domhoff explicitly accepts "the Challenge" presented by pluralists to explain the *processes*

"by which the ruling class in the United States dominates government and subordinates other social classes" (p. 9).

The processes of ruling class domination are classified as:

1. The special interest process, by which narrow short-run interests are advanced;
2. The policy formation process, by which general policies of the ruling class as a whole are developed;
3. The candidate selection process, by which the ruling class gains access to government;
4. The ideology process, by which ruling class beliefs are disseminated and reinforced.

Domhoff's description of the special interest process fails to rise above the standard anecdotal material in trickery, bribery, conflict-of-interest, and high-pressure lobbying that one finds in many muckraking accounts. Regrettably, his description of the candidate selection process is equally commonplace: electoral rules perpetuate a banal two-party system which avoids issues, discourages participation, and allows wealthy fatcats to manipulate candidates. It is not that Domhoff is wrong in his observations, but rather that others have described these processes more thoroughly and accurately.

Domhoff's best-developed model, and most intriguing supporting evidence, are found in his description of the elusive policy-formation process. The prestigious policy-planning groups are "at the center of things" (p. 64). Brief glimpses are provided of the composition and activities of the Council on Foreign Relations, the Committee on Economic Development, the Conference Board, the Business Council ("the Big Four of the policy network"), together with other "satellites and think tanks," including RAND, the Urban Institute, Resources for the Future, and the new Business Roundtable. Case studies of the activities of these groups provide both historical and contemporary evidence of their importance in overall policy development.

Domhoff's thinking about the ideological process is innovative, but his ideas on this topic are not as well developed as his ideas on policy formation. The ideological process is aimed at preventing the emergence of class consciousness among the masses and the development of serious alternative political programs. Individualism, free enterprise, competition, equality of opportunity, and related liberal notions are

disseminated at all levels of corporate, governmental, foundation, and organizational life. Unfortunately, Domhoff decides that "the ideology network is too big to describe completely" (p. 173). We are left with only a few interesting vignettes about the shaping of public opinion (although a short essay on the Advertising Council is a gem).

Pluralists will be enraged by this book (and so will "the powers that be," if they ever read it). It is more provocative than it is conclusive; it is anecdotal rather than systematic. But Domhoff's many fans—both Marxist and non-Marxist—will welcome its appearance.

THOMAS R. DYE

*Florida State University*

**Scholars, Dollars and Bureaucrats.** By Chester E. Finn, Jr. (Washington, D.C.: The Brookings Institution, 1978. Pp. xiii + 225. \$11.95, cloth; \$4.95, paper.)

The relationships between higher education and the national government which sank nearly \$14 billion into it in fiscal 1977 are the concern of this very good book by a former research associate in the Brookings Governmental Research program who is now senior legislative assistant to Senator Patrick Moynihan. It is recommended highly to those willing to look up from the disciplinary workbench to consider the health of the academic enterprise upon which their specialty depends and to those political scientists now investing heavily in the new concentration on public policy.

Certainly the public policy field examined here is important and complex enough. The federal government has no higher education policy; it has only "haphazard bits of policies." Federal support of higher education is almost always a mean to an end, not an end itself—to procure research useful to other programs, say, or to advance some other social end such as expanding opportunity for the poor or fighting discrimination. Nowhere in the federal government is there a general responsibility for policy toward colleges and universities. The academy responds to scraps of policy designed for a wide variety of purposes to which it is incidental.

Is this good or bad? Finn makes a case for both. The lack of concentrated federal authority and rationalized goals has given the institutions a chance to foster variety and proceed with great autonomy and freedom. But the attendant problems are many, and Finn proposes some modest structural changes he believes would help.

The principal value of Finn's book, it seems to me, is his sophisticated and knowledgeable explication of practices and problems. He has read widely, has understood what he read, and deploys his information judiciously. He is not dogmatic and he deals even-handedly with the possible consequences of various changes and the uncertainties which accompany any of them. He becomes controversial in that area where only angels perhaps could stand aloof, the questions about how the federal government should assist higher education.

In the public-private controversy over this policy, Finn's sympathies are clearly with the private interests. He views student assistance correctly as having two aspects, the question how best to expand access to higher education to categories of young people and the focus of a struggle among various kinds of institutions over which ones shall receive the benefits flowing from enrolling federally subsidized students. He recognizes the dilemma involved in all welfare programs: shall the poor get steak or make do with macaroni? Where higher education is concerned, Finn is strictly a "let them eat steak" man. The poor must have a "choice"—i.e., to go to a high-priced institution if that is what they desire. Finn advocates a two-tier schedule of assistance, the lower for those who will settle for a home-state public school, the higher for those who insist on an out-of-state public or a private college. This would equalize choice and balance the public-private competition.

What if the public should raise tuition too, to take advantage of the federal largesse? Finn says that would be bad, but later he reports sympathetically on suggestions that tuitions in all kinds of institutions should be full-cost pricing, with the differences supplied by increased student aid. He believes that "the objection to higher public tuition was doctrinal and not practical" (p. 130). Perhaps that is right, but so too is the special value attached to private schools by those who love them. Proving that some policy positions are bottomed on values and emotions simplifies nothing. Let the author repeat to himself ten times: "The privates could all go public" and see where that hits him!

Finn scorns the idea of institutional aid to public and private schools alike, to use as they please, justified on the ground that higher education *per se* is in the public interest. This is the only solution ever to have the unanimous support of the higher education community and its failure in Congress was certainly not strictly on the merits. Neither is he consoled by private acceptance of half-measures, such as

tuition offsets.

On the subject of the hated federal regulations, the book is judicious. Regulation of higher education as such has never even been considered. Laws aimed at other social goals affect higher education, and would without federal money. Institutions might try self-regulation or voluntary support of social reforms. Setting credibility for these efforts will be something else.

RALPH K. HUITT

*University of Wisconsin, Madison*

**Approaches to Controlling Air Pollution.** Edited by Ann F. Friedlaender. (Cambridge, Mass.: MIT Press, 1978. Pp. x + 465. \$24.95.)

Lawyers generally favor standards and sanctions to control pollution. Economists generally favor some form of economic incentives. Lawyers call for the direct approach, while economists call for indirect, market-like mechanisms. This book, a collection of seven articles from a conference held at MIT in 1976, is a major contribution to the controversy about how best to reduce pollution—whether to use directive or incentive methods.

Helen Ingram, Richard B. Stewart, and Marc J. Roberts and Susan O. Farrell assess aspects of the existing standards and sanctions approach; A. Michael Spence and Martin L. Weitzman compare the use of standards and sanctions with the use of incentives; Daniel L. Rubinfeld and Donald N. Dewees comment on the problem of assessing the benefits and costs of pollution control, a problem that confronts both pollution reduction methods; and Edwin S. Mills and Lawrence J. White argue that major benefits would accrue if effluent fees replaced direct standards for the control of auto emissions.

The editor, Ann F. Friedlaender, maintains that there is agreement that uniting the two approaches—combining standards with the use of economic incentives—is the best alternative. Disagreement exists, however, about how economic incentives should be determined and what purpose they should serve. Economists like Spence and Weitzman argue that incentives should be determined by estimates of how much damage pollution causes. According to economists, the purpose of incentives are to internalize externalities and to restore market-like efficiency. On the other hand, William Drayton, a lawyer and EPA administrator, argues that economic incentives should be determined by how much a firm saves from not

complying. The purpose of the incentives would be to bring violators into compliance, not to maximize market-like efficiency.

According to Drayton, incentives are an enforcement method. According to Spence and Weitzman they are means to increase overall economic efficiency. Friedlaender comments:

Unless the marginal damages are equal to the marginal costs of cleanup, the two systems will lead to different results. In particular, if marginal costs of cleanup rise rapidly as zero discharge is reached and if marginal damages rise slowly with movements away from zero discharge levels, it is likely that the Connecticut plan (advocated by Drayton) will lead to more cleanup than the standard-fee plan because the penalties for noncompliance will be greater under the former than the latter.

In 1977, the EPA, under new amendments to the Clean Air Act, was granted authority to introduce a version of the Connecticut Plan, which uses incentives as an enforcement tool, not as an efficiency-maximizing method. This book provides interesting insights into the reasons why the Connecticut Plan was adopted and what it is likely to accomplish.

This volume on alternative approaches to air pollution control is useful not only for the insights it provides about the use of incentives. Its conclusion—in the form of comments by J. Clarence Davies, James E. Krier, Allen V. Kneese, and Roger Strelow—provides an interesting catalogue of alternative reform proposals. Davies, for instance, argues that the scientific basis for all standards is inadequate and the EPA's Office of Research and Development should be reorganized so that it is more responsive to the needs of EPA's line programs. Krier maintains that because the 1970 Clean Air Act imposed vast economic and social costs for relatively small improvements, a management-based standards system that would permit regional variations is appropriate. For anyone interested in alternative means to achieve regulatory ends, this volume provides serious and sophisticated analysis.

ALFRED A. MARCUS

*University of Pittsburgh*

**Protest at Selma: Martin Luther King, Jr. and the Voting Rights Act of 1965.** By David J. Garrow. (New Haven: Yale University Press, 1978. Pp. xiii + 346. \$15.00.)

This book focuses on the 1965 Voting Rights Act, which David Garrow characterizes as "a legislative enactment that was to stimulate

as great a change in American politics as any one law ever has" (p. 236). As background, Garrow details the increases in southern black voter registration from 1940 until the eve of enactment. Later chapters trace the impact of the legislation in terms of voters listed by federal referees appointed pursuant to the act, blacks who registered with local registrars, the election of black office holders and the quest for black votes by white candidates.

The bulk of the book describes the events which led up to passage of the 1965 Act and analyzes the importance of the Selma protests in mobilizing congressional support for President Johnson's emerging voting rights legislation. The pivotal event in the months of civil rights activity was, of course, the March 7 attack by law enforcement officers on peaceful marchers. The marchers were setting out from Selma to Montgomery to dramatize demands for federal legislation which would remove discriminatory obstacles to black voting.

If *Protest at Selma* were no more than a descriptive treatment, it would still be a valuable source book for anyone interested in the events of the period or in the growth of black voter registration. Garrow's research and documentation are meticulous. He has carefully culled the journalistic treatments of the events. With these he has integrated scholarly analyses, publications of activist groups, congressional documents, and papers found in the Lyndon Johnson archives. I suspect that virtually no source relevant to the topic has escaped Garrow's painstakingly thorough research. These sources are all included in the extensive, interpretative footnotes.

Much of chapters 2 and 3 will be useful to anyone interested in a chronology of events in 1965 which culminated in the passage of the Voting Rights Act. Garrow presents a daily diary which includes the activities of the demonstrators in Selma, Birmingham and other communities, the responses by the president and members of Congress to the protests as they went about shaping the legislation, and the court battles fought over issues such as the right to protest and to register to vote. Trying to report simultaneously on the events along several fronts might, if attempted by a less skilled author, result in disjointed flitting from one scene to another. However, Garrow is able to move his reader from Brown's Chapel in Selma, to a federal district courtroom, to the anterooms of Congress, to White House strategy sessions, and back again, without confusion. Indeed, the author uses the day-by-day style, as have several popular writers, to lend drama to a sequence of events which, at least in their broad

outline, are already known to the reader.

While not discounting the utility of this book as a reference for the period, I suggest that it is even more valuable for its theoretical contributions. The 1965 Selma protests are compared with those in Birmingham two years earlier. Garrow offers reasons for the more direct response of federal policy makers to the Selma activities. Elsewhere the author demonstrates the learning process undergone by Martin Luther King, Jr., and the SCLC as they moved from earlier unsuccessful efforts in Albany, Georgia, to the Selma strategy of using peaceful protests to evoke a violent response which attracted media coverage and stimulated congressional support.

This exceptional book also explores the impact of media coverage on congressional attitudes and the link between constituents and legislator behavior. Legislators' comments published in the *Congressional Record* are reviewed to determine the source of their information and concern. Garrow controls for the proportion black in the legislator's constituency in an effort to determine whether legislative support of the protestors was triggered by conscience or simply constituted position taking. While I do not find the evidence compelling, the author concludes that proponents of relief for the protestors were not responding to actual or anticipated constituency demands.

*Protest at Selma* is a fine piece of scholarship. Its publication gives David Garrow much brighter career prospects than are enjoyed by other political science graduate students.

CHARLES S. BULLOCK, III

University of Georgia

**Essays on the Constitution of the United States.** Edited by M. Judd Harmon. (Port Washington, N.Y.: Kennikat Press, 1978. Pp. 202. \$12.95.)

An unanticipated bonus of America's bicentennial celebration (commencing in 1976 and not likely to conclude before 1989) has been the serious reexamination of the Constitution's origins, development and significance. For at least a generation, that quest had seemed virtually abandoned by a political science that had confined the study of public law almost exclusively to judicial process or behavior.

This slim volume is a welcome indicator of a renewed professional interest in American constitutional interpretation. Originating as a bi-centennial lecture series at Utah State University, *Essays on the Constitution of the United*



*States* reflects the matured judgments of "an all-star cast" of scholars who have kept the vestal flame alive. An insightful introductory essay by the editor, M. Judd Harmon, sets the stage nicely, calling attention to the remarkable common thread woven through these eight quite disparate contributions—namely, a seemingly "greater respect for the Constitution and confidence in its capacity to resolve problems than has existed for many years, perhaps greater than has ever existed" (p. 4).

The essays themselves begin with Alpheus T. Mason's impressively designed and written framework of conceptual underpinnings that comprise America's political heritage. Citing John Quincy Adams' observation that the United States had been founded not as a "democracy most simple" but rather "the most complicated government on the face of the earth," Mason prefers the term *free government*. "More easily described than defined," he notes, "it involves a complex of controls designed to temper together into one consistent work the sometimes opposite, sometimes complementary, elements of liberty and restraint" (p. 19).

The Bill of Rights serves as the main focus of two essays—those by the late Herbert J. Storing and by David Fellman. Storing deals with the origins of the first ten amendments to the Constitution, adding a fresh dimension to the conventional wisdom. In Storing's view, James Madison's insistence on sponsoring a particular set of amendments was the final stage in the remarkably successful early federalist strategy to secure an effective national government. This stroke headed off more far-reaching anti-federalist restrictions, as well as—in the absence of congressional action—an ominous portent of a state-called general convention under Article V that would propose disabling amendments (striking a modern note here). Fellman's essay next briefly summarizes the historical development that nationalized basic civil liberties via the Fourteenth Amendment.

The Supreme Court and the political process receive particular attention in three separate essays. With his customary skill at synthesis, C. Herman Pritchett discusses judicial supremacy from Marshall to Burger, pronouncing the Supreme Court's record, on balance, a success. "The courts," he reminds us, "are accessible to any individual or group with a valid case or controversy. Minorities that are too small to be effective in the political arena may find the courts are the only public agencies willing to listen to them" (p. 110)—with racial desegregation and reapportionment as major examples. This same point—that judicial intervention is

most justified when the path of political change is blocked—is reiterated by Henry J. Abraham. But his essay goes on to explore deftly the limits of judicial policy making, cautioning: "The Court is much better at saying what the government may not do than in prescribing what the government must do and how it must go about doing it" (p. 71). The Supreme Court's essential power still remains the power to persuade. The policy area traditionally most immune from judicial involvement—foreign affairs—receives thoughtful treatment in a fine essay by Louis Henkin. Noting with approval recent congressional assertion of authority, Henkin still concludes that neither Vietnam nor Watergate has significantly changed congressional handicaps in foreign affairs vis-à-vis presidential control.

By far the longest essays, aimed more at a professional than general audience, are authored by Martin Shapiro and Walter F. Murphy. Shapiro's piece on "The Constitution and Economic Rights" will raise eyebrows (and hackles) with such statements as this: "Those liberal commentators who applaud the activism of the Warren Court would do well to remember that the economic theories of the turn-of-the-century Court were as public interest-oriented, more clearly articulated, better scientifically grounded, and show greater survival value than the sociological, psychological and criminological theories that shimmered just below the surface of much of what the Warren Court did" (p. 80). But Shapiro's target is broader than the Warren Court. He takes on most of the scholarship in American constitutional interpretation since the 1930s, which he regards as a virtually monolithic approach, one he labels "the New Deal school" which taught judicial self-restraint in economic regulation but a special judicial responsibility to protect personal and civil rights. When Shapiro gets past this overdrawn historical backdrop, however, he develops a cogent and compelling analysis. There was a continuity in substantive approaches by the Supreme Court after 1937 that went beyond the oft-noted shift from economic concerns to personal rights. Shapiro argues convincingly that there was also a judicial continuity over a wide spectrum of economic concerns, as well as an increasingly recognized interrelationship between property and personal rights. "In any event," he concludes pointedly, "it is now fruitless to debate about whether the Court can or cannot involve, or has or has not involved, itself in constitutional economic policy making since the New Deal. It can. It has. It does. And there is no sign it is going to stop" (p. 98).

In the final essay, Walter Murphy probes the art of constitutional interpretation by concentrating on three questions in particular: (1) What are the scope and meaning of "the Constitution"? (2) What is the Constitution's relationship, if any, to some higher law? (3) What is the ranking of the various values that the Constitution seeks to promote? These far-reaching questions are probed with sophistication and scholarly care. Murphy concludes that human dignity is *the* fundamental value of the American constitutional system. Acknowledging the shortcomings of all such normative formulations, he sums up his thesis in these words: "In essence . . . it means that the individual, as a person, is the basic unit of legal—and moral—accounting; that government must respect all persons, in Kant's terms, as ends rather than as means; and that each person has equal claim to that respect, not because government so deigns but because we share a common humanity" (p. 157).

This book reflects the diversity and potential of a subject that calls for further scholarly exploration as we enter the final decade of the American Constitution's first 200 years.

GORDON E. BAKER

*University of California, Santa Barbara*

**Congress and the Budget.** By Joel Havemann. (Bloomington: Indiana University Press, 1978. Pp. viii + 247. \$12.95.)

Joel Havemann has concluded that the new congressional budget process is alive and well despite the fact that neither spending totals nor priorities have changed greatly since its enactment. In arriving at his conclusion, Havemann takes us through the development of the 1974 Budget Act and its first two years of implementation. He relates the legislative battles over jurisdictional turf and spending priorities. Much of the fighting during the first two years was over the integrity of the process itself. If Congress had been unable even to pass the concurrent resolutions (setting overall targets for revenue and spending) that are the heart of the process, the whole project would have been a failure.

One of the areas of greatest interest to students of Congress is Havemann's analysis of the establishment of the two budget committees. How was it possible to establish new committees that would likely threaten other power centers in the two houses? Havemann finds quite different patterns of accommodation in the House and Senate. Chairman Muskie

of the Senate Budget Committee was much more aggressive in defending his interpretation of the congressional budget on the floor than were the House chairpersons (Ullman, Adams, Giaimo) who tended to be more passive and accommodating to the wishes of other committees and the House leadership. This was due in part to the status of Muskie in the Senate and the focus of his committee's analysis. The Senate Budget Committee tried to concentrate on overall totals of revenue and spending, while the House committee scrutinized more closely individual spending items. Partisan battles continually plagued the new process in the House, but Senate Budget Committee proposals were easily successful on the floor.

Judging the impact of the new budget reform is filled with ambiguities. In one sense it is an impossible task, since no one can say what Congress would have done had the 1974 Budget Act not been enacted. One approach Havemann takes is to compare budget outcomes, in terms of fiscal policy and spending priorities, with what Congress did in the five years immediately preceding budget reform. His analysis shows that Congress did not alter the president's budget proposals significantly more with the new process than it had before the reform.

Certainly conservatives were disappointed that the new process did not lead to reduced spending, and liberals were disappointed that it did not eliminate many tax expenditures or increase support for social programs. About the only people who claimed a victory were those whom Havemann calls the "proceduralists" (p. 201), that is, those who were committed to the successful implementation of the new process and felt that it had to be ideologically neutral or it would be sabotaged by those who felt it was biased against them.

But what about those who argue that any reform is meaningless unless outcomes change? What difference does it make if we have two new committees, a tighter timetable, and a Congressional Budget Office that provides more information and better analysis if, in the end, we have the same winners and losers in the budgetary arena? Havemann argues that what the new process does is to force Congress to confront openly the consequences of its decisions when it votes on total revenue, spending, and deficits. Before 1974 the national budget turned out to be the sum of the separate appropriations bills which were passed. Fiscal policy has now become an explicit element of the congressional debate over the budget. But it would be naive to think that a new set of procedures could force Congress to do what it does not want to do. The same fragmenting

forces for spending are still operating as they always were.

One reason why the major outcomes of the process were not significantly different was that the budget committees (with a few exceptions in the Senate) were unwilling to assert themselves by challenging other committees on the floor. According to Havemann, this passive strategy was calculated to ensure the survival of the process—and the budget committees. It was accommodation in the short run aimed at consolidation of a stronger base in the long run.

One possible direction of budget committee power is the potential for centralizing budget control in the two houses. Budgeting can be seen as a constant struggle between the fragmenting thrust of many claims and the central controllers who have to say “no.” If the process survives and the budget committees are becoming stronger (and evidence indicates that they are), they may well play a role similar to OMB in enforcing central control and saying “no” to the always unlimited claims on the fisc. This controlling role will become increasingly important as political pressure is applied to decrease spending, and particularly if Congress is forced to adopt some budget-balancing device.

Joel Havemann is an editor of the *National Journal* who has covered the new budget process from its inception. His book is timely and extremely well written. Political scientists will not find analytic models or the systematic integration of his analysis with previous research on the behavior of committees in the congressional system. What they will find is a sophisticated analysis of the behavior of the individuals and institutions involved in the new congressional budgetary process. Havemann's access to the major participants infuses the book with many insights unavailable from the public record, and it tells us much about congressional behavior. If you want to know what happened in the first two years of budget reform, this is where to find out.

JAMES P. PFIFFNER

*California State University, Fullerton*

**National Party Platforms. Vol. 1, 1840–1956; Vol. 2, 1960–1976.** Edited by Donald Bruce Johnson. (Urbana: University of Illinois Press, 1978. Pp. xiii + 1035. \$35.00.)

This set is the latest edition of a basic

reference work which was first compiled by the late Kirt H. Porter in 1924 and has been repeatedly updated by Donald Johnson in recent years. These volumes continue their past contribution of making available in one place the policy documents of all major and minor American parties since 1840. The latest version goes beyond this bibliographic service, providing a number of new and useful features.

For libraries, an important new aspect is the separation of the series into two volumes, so that only the second will need to be replaced with each quadrennial outpouring of party rhetoric. For scholars, the addition of subject and name indexes are important new features. One gains a comforting sense of historical continuity from the fact that taxation has been the subject of electoral dispute from the first to the most recent Democratic platform. At the same time, we gain some sense of the passing of time by contrasting such later entries as “sociology of science” and “abortion” in recent platforms with earlier entries such as “saloons” and “Christian Sabbath.” However, the index would be more valuable, I believe, if it were combined in one place, rather than divided between the two volumes. Otherwise, this work is easy to use, well designed typographically and includes a convenient summary of candidates and election results heading each four-year section.

The principal use of these volumes, obviously, is for reference purposes, and no new research findings are claimed. Still, the very appearance of these documents implies their continuing significance in American politics. Platforms have grown greatly in length over the years. Almost half of the total pages are devoted to party manifestos dating from 1960. A specific comparison is provided by the Democrats, who required only nine short resolutions to summarize their principles in 1840, but took 30 times more space to present their policies in 1976. These changes reflect the increased complexity of American society, the increased reliance on the federal government and the presidency to cope with this complexity, and possibly the greater import given to the platforms themselves.

In a brief preface, Johnson discusses some of the significance of platforms: as statements of party principles, as criteria for judging an incumbent administration, as catalysts for party factions and interest groups, as aids to the voters and as guides to government action. That platforms are taken seriously is indicated by the continuing attention given to them by politicians (even those, like George Wallace, who deprecate them). This authoritative collection

will enable researchers to give them the further scholarly attention they deserve.

GERALD M. POMPER

*Rutgers University*

**The New American Political System.** Edited by Anthony King. (Washington, D.C.: American Enterprise Institute, 1978. Pp. 407. \$6.75, paper.)

Given a charter by the American Enterprise Institute, Anthony King recruited nine col-

leagues to join him in a consideration of changes in American politics since 1960. The result is a marvelous book, not least because of the distinguished company assembled for the task. Samuel Beer begins with an argument that neither the technocratic nor the romantic revolts of the sixties provided a coherent body of ideas to replace the New Deal philosophy of earlier decades. Fred Greenstein compares the Ford-Carter presidency to that of the Roosevelt-Truman-Eisenhower period. Hugh Hecllo writes of the specialized issue networks that have arisen, and of the problems these pose for political administration. Samuel Patterson considers changes in the constituencies represented in Congress, in congressional organization, and in decision making. Martin Shapiro compares the Warren and Burger courts as policy makers. Austin Ranney reviews variations in the electorate, in presidential party organization, in party rules, and in the consequent ability of the parties to respond effectively to the initiatives of other actors. Jeane Kirkpatrick arrays data on electoral activity against frameworks provided by three leading voting models. Richard Brody suggests that we are better able to account for the more demanding forms of electoral participation than for variations in election-day turnout. Leon Epstein deals with the increasing number of policies being administered by the states, and the consequences of categorical and general purpose grants to subnational units of government. Anthony King sums up with an argument that one cannot build effective coalitions out of grains of sand. These are lucid essays by seasoned scholars.

I think I learned most from the chapters written by Samuel Beer, Hugh Hecllo, and Martin Shapiro—in part because of the skill with which they were written, and in part because my own work has not dealt with the role of ideas and the executive and constitutional arrangements through which they are given expression. I suspect many readers will profit from the able summaries of subjects that

have not been in the forefront of their attention. The other half-dozen essays were all written with real authority, and bear the stamp of their authors' personal views as well.

*The New American Political System* should find considerable classroom use. It ought not to be used alone except with knowledgeable students. It presumes the reader knows a fair amount already. But at \$6.75 for 400 fact-filled pages, it is a bargain, and used along with a good text, it will give able students a chance to focus on the question of political change.

Whether American politics is "new" is a question that has been asked for a long time. (See, for example, Arthur Holcombe's 1933 *The New Party Politics* or Paul Douglas' 1932 *The Coming of a New Party*.) Anthony King's answer to this time-honored query is that "a strong case can be made out that the changes that have taken place in the American political system are both profound and widespread in their implications—that it is no mere rhetorical exaggeration to describe the system as it now functions as new, certainly in a number of crucial respects" (p. 381). If this answer sounds hedged, it should; the question is exceedingly complex.

In fact, many of the authors stress continuity. Fred Greenstein writes: "In any institution it is difficult in the short run to distinguish between permanent changes and changes that will turn out to have been only ephemeral. Many changes in the post-Eisenhower presidency were thought to have been permanent at the time but from the perspective of the late 1970's appear to have been rather drastic zigs and zags in patterns that had been established during the first three modern presidencies" (pp. 61–62). Samuel Patterson, who has the most detailed statement of what is meant by change, says: "Congress is not easy to analyze holistically. It is a subtle and complex collective decision-making body with intricate linkages to its constituencies and to other governmental agencies. . . . Changes tend to be both small in scope and large in number. Probably it is not too much of an exaggeration to say that Congress changes persistently. The constancy of change and the incremental character of change make it difficult to acquire a firm grasp of the meaning of change" (p. 130). Martin Shapiro compares the transition from the Warren to the Burger Court with that between the Marshall and Taney Courts: "In both instances the partisan political setting of the transition leads to overstatements of the differences. In both instances the new justices have somewhat different policy preferences from the old and thus often reach somewhat different resolutions of particular

cases. In both, however, the new Court receives and accepts the basic increments to judicial power and the basic theory of judicial review established by the old" (p. 203). Samuel Beer traces the technocratic confidence of the 1960s back to Francis Bacon, and points to the affinity between the Port Huron Statement and the romantic views of Wordsworth.

Questions can also be raised about the novelty of some of the analyses stressing change. Hugh Heclo writes: "In the old days—when the primary problem of government was assumed to be doing what was right, rather than knowing what was right—policy knowledge could be contained in the slim adages of public administration" (p. 103). Heclo's thesis that increasing complexity and specialization has given more influence to subject matter specialists is far more fully developed, but one may ask whether his fundamental argument is any different from Robert C. Wood's 1960 insight: "Complete as the reversal in the status of the professor among the politicians may be, there is nothing especially mysterious about it. . . . Up until the New Deal most domestic policy issues could be presented as simple moral choices. . . . Today the awesome complexity of national policy is immediately apparent. . ." (*New York Times Magazine*, 15 May 1960, p. 15). Anthony King holds that American politics has become atomized. "The politics of the 1930s and the 1940s resembled a nineteenth century battlefield, with opposing armies arrayed against each other in more or less close formation; politics today is an altogether messier affair, with large numbers of small detachments engaged over a vast territory, and with individuals and groups frequently changing sides" (p. 372). Opposing armies in close formation? In 1940, Pendleton Herring was writing that political parties had become less significant as rival organizations had grown. "This development has reached vast proportions today. It presents a vascular politics with an infinite number of arteries penetrating down into the community. Self-government demands free circulation of individuals' impulses. This calls for a varied network throughout the social structure. Disciplined parties, clearly differentiated, result in a hardening of these arteries" (*Politics of Democracy*, p. 110).

The difficulty of asking whether politics is "new" is that it forces a simple dichotomous answer to a very complex question. What is needed instead is a thorough understanding of the phenomenon in question, a sufficient knowledge of the antecedent state, an ability to sort out the threads of continuity, and careful specification of the conditions leading to the

changes that have taken place. Happily, the pages of *The New American Political System* are filled with materials fashioned to meet these needs.

JOHN H. KESSEL

*Ohio State University*

**Federal District Judges: An Analysis of Judicial Perceptions.** By William I. Kitchin. (Baltimore: Collage Press, 1978. Pp. 225. \$8.70, paper.)

Using a limited form of role analysis, William Kitchin conducted 21 lengthy interviews with U.S. trial judges throughout four circuits. He employed a "purposive, range sample" rather than a random sample of judges and said that his goal was "not to test hypotheses but to generate them, not to provide a comprehensive picture of the perceptions of district judges but to gain insights as to what those perceptions are." The first substantive chapter discusses the significance of whether judges possess "process orientations or result orientations," and this is followed by a chapter on how the district judges view their colleagues on the appellate court bench. A third substantive chapter outlines the judges' views on the degree to which they are bound by past precedents, and the next chapter follows up on this theme by addressing innovation on the district court bench. The following chapter focuses on the administrative aspects of the judicial role, and a final chapter presents the author's conclusions.

There are several strong and very positive elements in Kitchin's study. First, it deals with quite an important topic about which we have very little systematic empirical data. There are very few books indeed devoted exclusively to the lower federal judiciary and no more than a score of truly good articles dealing with "behavioral" phenomena. Thus, Kitchin's book comes as a welcome addition to the literature on this significant but little-studied subject. Two chapters in particular stand out for their interest and utility. The first is Kitchin's chapter outlining the areas in which the judges believe they have the greatest capacity for innovation, that is, those areas in which their personal values and ideals are more important in the decision-making process than *stare decisis*. A second chapter which stands out is the one entitled "The Administrative Role of U.S. District Judges." This chapter fits well into the developing body of literature on judicial administration.

Kitchin is also to be complimented on his clear, terse style and on the honest manner in which he presents his empirical data and conclusions. He is quick to acknowledge the limitations of his research and to admit errors in the way he developed some of his interview questions. Such information is useful for students planning similar inquiries.

There are also several other shortcomings that strike the critical reader. One is that while Kitchin duly acknowledges (in Chapter 2) that his is a non-random sample not necessarily typical of district judges in general, he often seems to forget this limitation throughout the rest of the book. Despite occasional caveats such as "assuming the reliability of the data," Kitchin tends to generalize as if he were speaking about federal district judges as a whole rather than about an admittedly non-random sample comprising only about 5 percent of the universe.

Second, Kitchin sometimes appears to fall into the trap of believing the world is as his interviewees describe it, or at least of not subjecting the judges' comments to insights that social scientists possess. For instance, at one point Kitchin warns against using Shapiro's model which describes justices "as political actors playing a policy-making role." Kitchin says: "District judges simply do not see themselves in political or policy-related jobs." Perhaps the judges do think of themselves as nonpolitical, but political scientists have long recognized that judicial decisions are very much a part of public policy and the political process, and we should not accept mutely the judges' protestations to the contrary.

Finally, Kitchin offers a set of supposedly testable hypotheses in his last substantive chapter. To me they sound like no more than ordinary conclusions, and one wonders why Kitchin elevates them to the level of rigorous hypotheses which lend themselves to operational definitions. For example,  $H_{11}$  posits that "judges of all levels of the federal judiciary consider protecting liberties and maintaining the separation of powers system to be important parts of their roles." Such is probably a valid observation, but I would hardly call it a rigorous, testable hypothesis.

The above criticisms are quite minor, however, in contrast with the total scope and quality of Kitchin's book. It is indeed important reading for any student of the federal judiciary in the United States.

ROBERT A. CARP

*University of Houston*

**Where Have All the Voters Gone? The Fracturing of America's Political Parties.** By Everett Carl Ladd, Jr. (New York: Norton, 1978. Pp. xxiv + 86. \$7.95, cloth; \$3.95, paper.)

In *Where Have All the Voters Gone?* Everett Carl Ladd, Jr. argues that the political parties must be revitalized and that a necessary element of that revitalization is the scrapping of recent party reforms. The essay was not written for an academic audience (the bulk of the work initially appeared in *Fortune*). Nevertheless, the logic of his thesis is compelling.

Today, unlike years past, most Americans believe that their government is out of control and non-responsive. A partial explanation for this belief is that the political parties are no longer fulfilling their obligation to aggregate the interests of the people. A party system that effectively aggregates interests must be competitive (the in-party must either offer policies that the public prefers or be ousted by the out-party), representative (the parties must offer policies that appeal to their coalition of constituents) and organized (the parties must take positions on issues so that the people can vote for or against those positions and the party can then, once elected, translate the positions into government policy). At present, the parties in the United States fail to compete, represent and organize.

Instead, the United States has a one-and-a-half party system. The half party, the Republicans, directs its appeals toward a distinct minority of the middle class. Hence, the Republicans are not strong enough to control Congress or for that matter more than a handful of states. Yet the Democrats (the party of everyone since even most conservatives in America accept the programs of the New Deal) are unable to bridge the schism between the Old Class Democrats (the New Dealers) and the New Class Democrats (the present-day liberals). Hence, their party membership is deeply divided on new issues such as aid to minorities and protecting the environment. During well-publicized elections, particularly the presidential contest, the Democratic coalition splits and the Republican candidate becomes competitive provided he supports the New Deal.

Recent party reforms exacerbate the difficulties by opening up the nominations to the control of the rank-and-file members who tend to be more interested in choosing candidates who reflect their own issue positions than in winning the presidential election. Since the rank-and-file Republicans are more conservative and the rank-and-file Democrats are more liberal than is the population, the one and a half

parties are becoming less and less representative.

In order to preserve the endangered parties, control of the parties must be returned to the professional politicians who in order to win the election usually pick candidates who appeal to a majority of the voters. If the selection of the president were placed once again in the hands of the party leaders, the parties could once again compete, represent and organize.

Ladd's argument is obviously not new. Indeed, it is one of the more often-used rationales for preventing the inclusion of new groups in a decision-making arena: in order to protect the people from their folly, decisions must be kept in the hands of an elite. Although the book contains new elite interview materials, there are no new interpretations and no new theories. It would be unsuitable for upper-division classes.

The book does have its strong points. It is well written, the data presentation is excellent, and the summaries of the problems in the Republican and Democratic parties are devastating. Finally, the essay is provocative. To accept Ladd's conclusions one must believe that the political parties can serve a useful function, that elected officials can control governmental policy, that political party machines served the citizens better than other alternatives, and that the content of governmental policy is more important than providing citizens with the opportunity to participate in determining that content. Thus, controversial assumptions underpin a tight logical argument for making the party nomination procedures less democratic. Further, the argument is articulated by a respected member of the academic elite for a business executive elite. For this reason, I highly recommend the book as either a supplement to an introductory American government course or as a provocative introductory essay for a political parties course.

JOHN R. VAN WINGEN

*University of Southern Mississippi*

**Law and Identity: Lawyers, Native Americans and Legal Practice.** By Linda Medcalf. (Beverly Hills, Calif.: Sage Library of Social Research, Vol. 62, 1978. Pp. 147. \$14.00, cloth; \$6.95, paper.)

It is much easier to denounce liberalism than it is to rectify its errors. It is much easier to identify the destructive aspects of a life permeated by liberal values than to describe constructive alternatives. It is much easier to decry the reification of law than to write a book that avoids reification. What is it about

liberalism or legalism that draws denunciation yet eludes either convincing prescription or effective solution? I suspect that in many cases neither liberalism or legalism is the real target. In many cases an attack on either is really an attack on capitalism. Politics and law are on the surface, economics is underneath. We say liberalism or legalism when we really mean capitalism because we really don't have a political economy which is worked out in sufficient detail to explicate their linkages. Yes, there is Marx, but somehow Marx has rarely been either well understood by American social scientists or well used.

What does all of this have to do with the new short book by Linda Medcalf? I suspect that a close reader would answer "everything and nothing." Everything, because for Medcalf liberalism is the enemy and law its malevolent instrumentality. Liberalism destroys human possibility and legalism legitimizes that destruction. The dirty work of liberalism and legalism is done by lawyers. As Medcalf sees it, even the "good guys" are "bad buys." Radical lawyers are, according to Medcalf, more lawyers than radicals. Their language is the language of legal rights. Although they seek liberation, they are still slaves to a pattern of thought that defines human problems in terms of rights and duties. Medcalf argues that there is no real freedom in liberalism or legalism.

Is Medcalf a Marxist? It's hard to tell from the body of the book, and that is part of the problem. I think, however, that her ideology is more complex and interesting than perhaps even a well worked-out Marxism would be. Perhaps it would be more accurate to say that the book is humanistic and therefore impatient with capitalism without being clear about either its perspective or its impatience. That lack of clarity is, at base, what is so frustrating and, at the same time, intriguing about the book.

Medcalf's book is based upon a study of the small population of "radical" lawyers dealing with the problems of Native Americans. It is a study that is self-consciously but not conscientiously theoretical. Her book is intended to be a case study in the adequacy of liberal legalism. What she wants to show is that even the best intentions cannot overcome the curse of liberalism and the routines of legalism. Her question is how far and where does legal liberalism lead as a developmental strategy for Native Americans. Her answer is that it leads in the wrong direction, although she never identifies what the right direction would be.

Medcalf describes the strategies of the lawyers whom she studied as being threefold. They attempt to cope with the problem of poverty



and powerlessness among the Native Americans by first securing their claims to their land; second, by establishing an effective structure of public authority to protect those claims and promote economic development; and, third, by identifying a set of rights which must be accorded to protect Native Americans from their indigenous, tribal governments. Liberalism recreated property, government, rights for Native Americans. Her view, however, is neither an accurate reconstruction of the political theory of Lockean liberalism nor the history of American liberalism. What really troubles her is that liberal legalism as an ideology of development inevitably leads Native Americans away from values that are both more authentic and more humane.

What values? This is the unanswered question. It is easier to tell what Medcalf dislikes than what she thinks is lost from the experience of Native Americans by pursuing the strategy being pursued by lawyers on their behalf. She deplores the depersonalization of politics, the ethic of efficiency, individuation, contest, adversariness, the idea of life as struggle, as power or as control. She seems to admire community, cooperation and unity. Yet we never hear the voice of Native Americans. We never find out what they want or what their experience would hold out as an alternative to the things Medcalf deplores. Medcalf's book is not well served by the absence of that voice.

Finishing Medcalf's book, I confronted a question I face in my own work and never answer fully. We seem so much better at criticism than construction, at stating what fails and why than figuring out what works and how to attain it. In moments I wonder and worry as much about myself and my work as about the books I read. Yet it isn't often that I'm driven to think such thoughts, and even a flawed book is valuable if it can call forth the right questions. How much better it would be to suggest plausible answers.

AUSTIN SARAT

*Amherst College*

**Labor at the Polls: Union Voting in Presidential Elections, 1952-1976.** By Jong Oh Ra. (Amherst: University of Massachusetts Press, 1978. Pp. x + 182. \$15.00.)

Jong Oh Ra has written a very interesting book of genuine value to scholars seeking a greater understanding of organized labor and the electoral system. His approach is ambitious in that it develops a model of union voting

behavior with theoretical foundations "in empirical theories, speculative thoughts, and historical observations," which, in combination, he uses to find how union members and their families vote and how we can explain their behavior. The result of this efforts is a multi-level analysis that is theoretically interesting, analytically crisp, and well written.

In contrast to much of the field's literature, the author finds, by way of an interesting reformulation of CPS data, that socially mobile union members from 1948 to 1964 tended to vote more Democratic than union members in general, postulating that the experiences that are linked to occupational mobility somehow impel union members toward a closer relationship with the Democratic party. His explanation of this finding incorporates the notion of "relative deprivation" and the idea that greater social mobility for unionists generates *more* rather than fewer political demands and a tighter link to the party more receptive to those demands.

Along the way Jong Oh Ra examines critically the claim that the affluence produced by the extension of the New Deal has weakened support of the Democrats among union members. He sees the drop in labor rank-and-file support for the Democratic candidates in 1968 and 1972 as short-run aberrations in a still close relationship, explaining the defections of unionists in these two elections as the result of elements specific to the candidacies of Humphrey and McGovern rather than the weakening of durable ties to the Democratic party itself. While I agree that the relationship of labor union voters to the Democratic party is much stronger than recent literature has pictured it to be—Carter did receive 63 percent of union votes in 1976—John Oh Ra tends to deal inadequately with the very role of labor unionists and their leaders in producing those Democratic candidates in 1968 and 1972. If one chooses to use variations in party candidates as a means of explaining voting behavior, one cannot, as this author does, omit discussion of labor's role in the very process of candidate selection itself.

Several other criticisms seem justified. In the effort to create a broad and scholarly picture, Jong Oh Ra does not adequately balance his critical analysis of the existing literature. As a result, one finds the book overemphasizing events, responses, and analyses before 1965, skewing an otherwise admirable historical approach to labor union voting by paying too little regard to the literature of the last ten years or so. "Recent" data quoted in the book (p. 79) turns out to have been written in 1963. Literature produced in the 1970s examining the



impact of suburbanization and racial hostilities on labor union voting is not dealt with as critically as the pre-1970 literature.

In addition, while rightfully attacking the totality of the claim that the foundation of the New Deal realignment has evaporated, Jong Oh Ra tends to underplay the relative declines that have actually occurred in popular (and union) support for specific policies that were once heavily supported only a decade ago. While it is important to correct the premature "burial" of important elements of the New Deal realignment, recent research clearly identifies important changes in the relationship of voters to new and old governmental policies and programs. Jong Oh Ra's important insights into the durability of links between labor union voters and the Democratic party should not be lost because some overstatement in the book offers an easier target for criticism than do his more substantive contributions.

Overall, *Labor at the Polls* is an intriguing work because of the variety of probes, methods, and analyses that its author brings to bear on his subject. Labor union voting patterns and their impact on electoral politics have been poorly treated in the literature, particularly where quantitative analysis is concerned. Jong Oh Ra has made an important linkage between levels of analysis, and by reformulating certain important questions, this book should stimulate more productive work in the field. Along the way the author has also displayed a mind of superior range, originality, and incisiveness.

RICHARD L. RUBIN

*Swarthmore College*

**The Politics of Efficiency: Municipal Administration and Reform in America: 1880-1920.** By Martin J. Schiesl. (Berkeley: University of California Press, 1977. Pp. ix + 259. \$11.75.)

Was it or wasn't it? Did they or didn't they? Were they or weren't they? It is these perennial questions about reform and machine politics in America's cities during the period from 1880 to 1920 that preoccupies historian Martin J. Schiesl. The questions are of course familiar. Was the municipal reform movement merely the manifestation of the narrow self-interested politics of an emerging upper-middle-class, urban entrepreneurial elite? Did the reformers' preoccupation with efficient administration blind them to the needs of the urban working class and poor? Were the oft-defeated reformers

"morning glories" which withered in the afternoon and passed unnoticed from the scene in the evening?

Following a wide-ranging survey of four decades of American urban history, Schiesl would answer each of these questions (had he formulated them at the outset) in the negative. In his final paragraph, he sums up what seems to be the book's main argument:

In an era of boss politics a reform administration, despite its short life, recovered a large amount of responsible civic management, instituted new forms of political leadership, and extended the welfare functions of the city. When the machine regained control of the government, those accomplishments were not lost; the people had been shown higher standards of public service which the boss wisely chose not to ignore (p. 197).

The territory that Schiesl explores is not untraveled. Indeed, mention of the issues causes names like Plunkett, Merton, Banfield, Wilson and Holli to spring to mind. In devoting a chapter to each of the major ideas and structural mechanisms that preoccupied urban reformers, Schiesl makes two contributions to our understanding of the politics of urban reform. First, because he ranges much more widely in the history of this era than any of his predecessors—from Boston to Berkeley, and Manistee (Michigan) to Galveston—Schiesl provides his readers with many fascinating and informative historical insights. To illustrate: how many students of intergovernmental relations or urban politics are aware of the important role played in the first decade of the twentieth century by the U.S. Census Bureau (under the guise of rationalizing data collection) in promoting urban reform and the broadening of municipal social services (shades of community action and model cities)? Second, Schiesl provides a new answer to the old debate over whether machine or reform politicians more effectively promoted democratic values and the well-being of the masses in urban America. He suggests that neither camp was without its internal contradictions and the synthesis that resulted from the competition between the two produced the best of all possible worlds. While the Panglossian quality of this analysis may initially provoke a few modest snickers, the more one reflects on Schiesl's analysis, the more persuasive it becomes.

While its central argument may be both fairly novel and persuasive, this is not a volume without fault. To begin with, it is by no means clear that my perception of the author's central theme corresponds with his perception. Indeed

upon occasion it is difficult to be certain of just what is the author's central point. What is clear however is that creative writing is not his forte. Granted, there are few budding Hemingways (or even Grant McConnells, to think of someone who writes in a similar mode but with elegance) roaming the academic ranges these days. Nevertheless, someone at the University of California Press should be required to turn in his or her blue pencil for failing to make this book read more crisply. The total absence of chapter subheadings represents exhibit number one in any argument for such stylistic aids. Moreover, even with them, the most fervent student of the topic would not be likely to argue that the book represents a model of either clarity or liveliness.

Of greater seriousness is Schiesl's apparent inclination to sometimes mistake a quote for a fact. At times, it appears that he suffers from the same malady that many of us suffered from prior to Nixon—an absence of skepticism about the public pronouncements of public people. Particularly in the early chapters there is a maddening tendency to attempt to depict the reality of urban life and politics at the turn of the century by stringing together a series of quotes from various personages. The fact that from time to time there appear to be implicit contradictions among those whom he cites to make a particular point does not seem to faze the author. Indeed, in a few instances, a subsequent quotation from a particular individual serves to subtly contradict the point which the individual's earlier cited words were assumed to have demonstrated (see, for example, the comments of E. L. Godkin to be found on pp. 8 and 23). Nevertheless, students of urban politics do need to be aware of this book and the author's argument. Once figured out, it does prove provocative.

ALLAN ROSENBAUM

*National Institute of Education  
U.S. Dept. of Health, Education and Welfare*

**Charting the Future: The Supreme Court Responds to a Changing Society, 1890–1920.**  
By John E. Semonche. (Westport, Conn.: Greenwood Press, 1978. Pp. xiii + 470. \$25.00.)

This is an odd book. In his preface John Semonche suggests that the role of the Supreme Court from 1890 to 1920 has never been put in its appropriate conceptual context, indicates—correctly, in my judgment—that many commentators have applied simplistic criteria to its

work, and intimates that he has found the master key. Unfortunately, he left it in the latch: the next 417 pages constitute an inch-by-inch trip through the *U.S. Reports* for the period.

This sort of exercise is not without merit *provided* it is guided by some thematic compass. Twenty-three years ago, in a burst of youthful enthusiasm, I read all the Court's decisions during the tenure of Justice Frank Murphy. But I did so with an *a priori* hypothesis which—not surprisingly perhaps—was triumphantly sustained. Uncharitable critics hinted I had strapped the poor justice to a bed of Procrustes, but whatever the views on the merits of my work, no one accused me of aimlessness. Semonche has taken the other route: with unnerving industry he had catalogued every tree in a judicial forest planted in a 30-year period.

Admittedly, he has done this task with care, although on occasion there is a distinctly ahistorical flavor. He writes, for example, of the country's "long devotion to the separation of powers concept" (p. 428) though the only early cases in which this devotion surfaced as part of the *ratio decidendi* were *Cooley v. Board of Wardens* (1851) and *Murray's Lessee v. Hoboken Land Co.* (1855). It was apparently Justice Benjamin Curtis' private hobby and with him vanished from the precedential scene for half a century. Semonche also considers the delegation of power to the states to regulate "demon rum" a constitutional innovation; yet in 1803 Congress (2 *Stat.* 205) prohibited the importation of slaves into states whose laws prohibited slavery.

Probably my most significant criticism of Semonche's vacuum-packed approach arises from his handling, or failure to handle, the "Chinese syndrome": the Supreme Court's surprisingly willingness to defend Chinese immigrants, overwhelmingly alien, from the police power. To make a long story—brilliantly limned by Howard Jay Graham in "Justice Field and the Fourteenth Amendment"—short, the status of the Chinese in California became a litmus test of the power of the railroad magnates in their struggle with the Populists, who assimilated racism into their assault on imported scabs.

The poor Chinese, willy-nilly, became pets of the federal courts operating under "Ninth Circuit Law": the bottom line was that Chinese aliens on the West Coast had freedoms denied to black citizens in, say, Alabama. Justice Brewer, Field's nephew, and other judges continued this tradition at the national level. All in all, Semonche would have profited from reading Graham's *Everyman's Constitution*.

There are some minor nits to pick with Semonche's interpretation of various cases: I would suggest the importance of the *Northern Securities Case* was the appearance of five Justices in support of the "rule of reason" (Brewer and the four dissenters). And the Court's decision in *Hammer v. Dagenhart* (1918) could have usefully been compared with *U.S. v. American Tobacco Co.* (1911): how did a ban on interstate shipment of goods produced by child labor (bad) differ logically from one on goods produced by "trusts" (valid)? However, we both love the first Justice John Marshall Harlan, a *mensch*.

Finally, after the long march, we reach the "Epilogue" in which Semonche returns to his rationale for all this labor. He explodes some "myths" which, with due modesty, I thought I exploded in the *University of Chicago Law Review* in 1963. (Maybe they have since been revived.) He feels the Court went through a time of troubles intact; true enough: nine justices went in and another nine came out. But at the analytical level we end where we began, with a chart but no compass readings.

JOHN P. ROCHE

*The Fletcher School of Law and Diplomacy*

**Bakke, DeFunis, and Minority Admissions: The Quest for Equal Opportunity.** By Allan P. Sindler. (New York: Longman, 1978. Pp. xi + 358. \$12.50, cloth; \$5.95, paper.)

Few current political and legal issues are as emotionally charged as the subject of "reverse discrimination." On one side are those who believe that affirmative action and preferential treatment are forms of benign discrimination essential to compensate for past evils. On the other side are those who argue that preferential treatment for minorities violates *Brown v. Board of Education*, Title VI of the Civil Rights Act of 1964, and the equal protection clause of the Constitution.

Allan Sindler's book contains an excellent analysis of reverse discrimination questions posed in *DeFunis v. Odegaard* (1974) and *Regents of the University of California v. Bakke* (1978). Less emphasis is naturally placed on *DeFunis* than *Bakke*, for in *DeFunis* the Burger Court ruled that the case had become moot. Nevertheless, similarities and differences between *DeFunis* and *Bakke* are capably addressed throughout the book.

Sindler presents a detailed account of how the *Bakke* case arose, moved through the California courts, was appealed to the Supreme

Court, and developed into a major test case. At the Supreme Court level, Sindler's focus is on the fragmented Court that, by 5 to 4 votes, ruled (1) that the quota program of the University of California at Davis Medical School was illegal, (2) that race could nevertheless be one consideration weighed in the admissions process, and (3) that *Bakke* should be admitted to the medical school. Unfortunately, the book fails to suggest timely recommendations on how reverse discrimination questions should be resolved in future litigation.

Despite its high quality, Sindler's book fails to convince the reader that he is a truly dispassionate observer. Underneath what appears to be an objective study are signs of a pro-*Bakke* bias. First, of all the opinions written in the *Bakke* case, only Justice Brennan's anti-*Bakke* stance comes under direct fire. For example, Sindler characterizes the Brennan position as tautological, incomplete, misleading, fallacious, as an "attempt to settle the matter by judicial fiat," and as seeking to "reconcile racial preferences and no reverse discrimination by defining reverse discrimination out of existence" (pp. 303-07).

As a second example, Sindler writes:

Since the granting of race preferences in any competitive situation necessarily involved some degree of reverse discrimination, a significant judicial role is required. Moreover, the argument's premise—that one could confidently and easily distinguish between "good" and "bad" racial discrimination—was unsound (p. 192).

But contrary arguments abound. The quest for equal educational opportunity must go beyond the judiciary. The Congress, the White House, and executive branch agencies all have a legal responsibility to promote affirmative action. Obviously some non-minorities are adversely affected by preferential treatment. However, non-minorities are still far ahead in terms of education, employment, and housing. The fact is that *Bakke* and *DeFunis* had multiple career opportunities aside from attending medical or law school. Minorities rarely have that many chances. Before we can approach the ideal of equal opportunity based on merit, therefore, steps must be taken to ensure equal footing. "Good" racial discrimination may thus be defined as the amount of affirmative action necessary to guarantee that all competitors are evenly aligned at the starting gate and that no one is slowed down by the weight of past discrimination.

Third, Sindler's pro-*Bakke* view is seen in the fact that he discards affirmative action employment cases, like *Griggs v. Duke Power Company*

(1971), as irrelevant to affirmative action in education (pp. 180–88). *Griggs* held that if employment requirements which were not job-related had a disproportionate adverse impact on the hiring and promotion of minorities, that that effect—not the requirement's intent—sufficed to prove discrimination. Yet Sindler rejects the effects test even though Title VI was prominently involved in *Bakke*. Essentially, then, Sindler accepts the virtually impossible task of proving intent, not effect, in affirmative action cases.

Fourth, Sindler seems to fear the length of time necessary for affirmative action programs to achieve equal opportunity (p. 263). He also argues that over time preferential treatment will "stigmatize the favored groups" (p. 269). Both arguments are questionable. It may take more than one generation to achieve equal educational opportunity, but affirmative action is not an unending process. And although feelings of stigma will occasionally occur, most minorities will probably feel that affirmative action was necessary to assure them the opportunity for a good education.

As a final comment, Sindler concludes that *Bakke* is not a "landmark" decision and presumably will have relatively little impact (pp. 294, 301). However, this may be a premature judgment. Even the conservative Burger Court is likely to use *Bakke* to continue to support affirmative action programs. Furthermore, a future liberal Court could construe *Bakke* to advance significantly preferential treatment in areas outside education. While perhaps not the most important civil rights decision since *Brown*, *Bakke* may ultimately qualify as a "landmark" decision and have a lasting impact on minority rights.

CHARLES M. LAMB

State University of New York, Buffalo

**The Limits of Technocratic Politics.** By Jeffrey D. Straussman. (New Brunswick, N.J.: Transaction Books, 1978. Pp. xii + 164. \$14.95.)

*The Limits of Technocratic Politics*, which began as a dissertation, is a readable summary of largely familiar material, combined with sensible (and again familiar) commentary. Technocratic politics is the term Jeffrey Straussman uses to cover the continuing use (and misuse) of various analytic and decision-making techniques and tools. In general, he is concerned with the policy and political roles of experts—and their relations with more responsible (or higher

status) decision makers. In particular, he devotes successive brief chapters to examining the rise and decline of the new (early sixties) economics, the PPBS (Planning, Programming Budgeting System), the social indicators movement, and the futures movement. Each chapter is a handy summary of major developments and each is well documented.

For the professional who followed these movements as they unfolded there is not much new. The preface by Bertram Gross says, "This book is the first empirical study of major elite movements in the technology of public planning: macroeconomics, system budgeting, social indicators, and futurology." But, in fact, the book is not particularly novel or empirical. It is a good, though brief, literature review in large part—and will be useful to students and others new to these fields. Observers who walked into the policy theatre while ZBB (Zero-Based Budgeting) was showing, or MBO (Management by Objectives), for that matter, may want to catch up by reading this book. In discussing the role of analysts Straussman points to legitimation, the focusing of issues, the selection of targets, and tactical guidance. Readers interested in further exploring the roles of analysts should by all means peruse Arnold Meltsner's book, *Policy Analysts in the Bureaucracy*—a book at once more empirical and more thorough. And, of course, one can find numerous individual volumes on each of the subjects to which Straussman devotes his individual chapters.

JAMES W. DAVIS

Washington University

**Political Control of the Economy.** By Edward R. Tufte. (Princeton, N.J.: Princeton University Press, 1978. Pp. xi + 168. \$10.00.)

For those concerned about the lack of elite responsiveness to electoral processes, this is an optimistic book. Contrarily, for those concerned about rationality in the development of macroeconomic policy, this is a pessimistic book. Optimists, unite! You have nothing to lose but the inconvenience of modest, and, occasionally, not-so-modest economic booms and busts.

Edward R. Tufte has authored a remarkable study, one that is provocative and concise, sometimes brazen in its theoretical assumptions and assertions, laced throughout with elegance and wit. Amid the vast outpouring of contemporary social science, *Political Control of the Economy* truly is a book for all seasons, important to every scholar interested in the

relationship of elections, government, and public policy.

The major theme, *contra* Marx and mildly ironic, is best stated by the author: "As goes politics, so goes economic policy and performance. This is the case because as goes economic performance, so goes the election" (p. 137). Tufte marshals an impressive array of new and secondary evidence, relying primarily but not exclusively on American data from the past three decades, to explore these ideas. The basic argument is not original (political economists and a few political scientists have been tinkering with these notions for years) but the author organizes his data and analysis in such refreshing fashion that even mainstream political science will be hard put to ignore the message.

In the United States, and increasingly in Western Europe, public officeholders have engineered an electoral-economic cycle geared to maximizing the reelection prospects of the party in power. For example, from 1948 to 1976 the U.S. electoral-economic cycle has consisted of (1) a two-year cycle in the growth of real disposable income per capita which accelerates in even-numbered (election years) and decelerates in odd-numbered (nonelection) years; and (2) a four-year presidential cycle in the rate of unemployment, with a decline occurring in (surprise) election-reelection years, and an increase beginning 12 to 18 months following the presidential election. Moreover, the greater the electoral stakes, as in attempts by incumbents to regain office, the greater the economic stimulation. The Eisenhower administration is the only postwar exception, a point to which I will return. The Nixon administration, in this as in most things, far outran all others in its enthusiasm to induce short-run economic gains. Tufte shows that the typical fourth quarter of a presidential election year is characterized, not only by a decline in unemployment and an increase in real disposable income, but also (a) dramatic increases in social security and veterans' benefits, usually and conveniently delivered to recipients in October and which account for almost 50 percent of all federal transfer payments; (b) substantial increases in the supply of money through various federally induced activities; and (c) sudden unexplainable bursts of federal grant deliveries to states and localities. American presidents seem to follow a variation on Mark Green's Golden Rule: he who distributes the most gold before election day, rules.

Tufte then makes clear that such activities are not confined to the United States but increasingly characterize the political economies of all industrialized democracies. Using

data from the seven largest, Tufte notes that GNP growth rate during election years, for example, is nearly double the rate in nonelection years. Election-year expansion in the U.S. appears to spill over to other countries, too, since the "growth rates in real GNP in other countries are nearly double in the years with U.S. presidential elections compared to years without elections in the U.S. or the country itself" (p. 66). What accounts for this?

Tufte provides a suggestive piece of empirical detective work. Since 1971, the major European democracies plus Japan have increasingly followed the American electoral calendar, holding elections in even rather than odd-numbered years. "With the synchronization of election calendars in large capitalist democracies, we have a recipe for an international boom and bust cycle" (p. 69). Tufte does not, however, have sufficient evidence to convince one that it is the electoral cycle, and not simply the increasing interdependence of a world economy, that is responsible for these effects.

The remaining parts of the book are concerned with examining the economic determinants of voting (a summary and slight expansion of Tufte's earlier published work) and specifying party and class differences in government economic policy, again focusing primarily on the United States but including a discussion of comparative data from Western Europe and Japan. Most of the party/policy data are taken from the recent work of Douglas Hibbs, Jr., and Tufte does not improve on Hibbs' earlier analysis. Nor, I might add, does he discuss any of the criticism that has been directed at this line of inquiry. Tufte does incorporate some imaginative content analysis of the *Economic Reports of the President* and the *Annual Report of the Council of Economic Advisers*. Consistently with the work of Gerald Pomper, John Kessel, and others who have examined party-government documents, he shows that the macroeconomic policy preferences of decision makers diverge sharply, depending on whether a "left-liberal" or "right-conservative" party is seeking and/or holding office. This generalization holds for both American and West European political systems. Given the apparent uniform effect of the election calendar, how "sharp" is party divergence? The question is hedged for the United States and Hibbs' conclusions are repeated for Western Europe. Tufte is quite sensitive to methodological deficiencies here (involving time-series analysis and within-country variation) as well as the very powerful cultural and historical differences between societies. Still, his conclusion is worth quoting (p. 104):

The single most important determinant of variations in macroeconomic performance from one industrialized democracy to another is the location on the left-right spectrum of the governing political party. Party platforms and political ideology set priorities and help decide policy. The consequence is that the governing political party is very much responsible for macroeconomic outcomes—unemployment rates, income equalization, inflation rates, and the size and rate of expansion of government.

Tufte's and other studies seem persuasive on this argument, so even closet responsible-party theorists and election enthusiasts can feel vindicated. However, the more fundamental question is: how important are elections and parties compared to *other*, largely nonelectoral, factors?

For a volume written by one of the profession's most sophisticated methodologists, *Political Control of the Economy* is curiously indifferent to the actual magnitude of policy change in some parts of the analysis, while greatly overestimating magnitude in others. Consider Tufte's comparative data on income equalization (at p. 95). Information about the share of national income held by the top 20 percent of income earners, before and after taxes, is presented for 11 countries. Predictably, those societies where the Left has governed the longest (Sweden) have the highest rates of income equalization based on the impact of direct taxation. But what is more striking, even if one accepts Tufte's measure as a valid index of government equalization activity, is how small the actual shift has been, even in Sweden. The pre-tax share of wealth held by the upper fifth in Sweden is 40.5 percent; the post-tax share, 37 percent, a net "redistributional" shift of 3.5 percent. Comparable figures for the United States are 44.8 percent before taxes, 42.9 after, a shift of 1.9 percent. Since this is the only income equalization data examined in the book, one can be forgiven for concluding, as Mark Twain did about his own death, that the impact of the Swedish and other left-liberal parties are greatly exaggerated in Tufte's analysis. Obviously, there are large and more important economic differences between countries like Sweden and the United States in terms of income and service distribution, but nowhere in this book are such differences examined. Indeed, the problem of income distribution, a problem one might consider central in a study of political control of economic policy, receives scant attention for any society, including the United States. Why? Since Tufte largely excludes nonelectoral factors, his "model," greatly truncated, inevitably looks like this: Parties → Elections → Economic Policy. One need not

be a Marxist (Neo- or otherwise) to posit a somewhat different causal sequence: Economic Structure → Parties → Elections → Economic Policy. The author, perhaps intentionally, avoids dealing with this issue.

Or consider the heavy emphasis placed on the *rate* of changes in real disposable income. The median growth rate in real disposable income when an incumbent American president is seeking reelection (Tufte's maximum-impact hypothesis) is +3.3 percent; in other years, +1.7 percent. For a family of four, with \$15,000 annual income and who are at the median, the actual dollar gain during the election year is a paltry \$240. I realize that disposable income is a summary economic measure, that the psychology of "good" versus "bad" times is more important than actual amounts, and so forth. Tufte nevertheless loads considerable theoretical baggage on such small changes. Further, he ignores the fact that the direction of change in real disposable income has moved up, even though the rate has been comparatively low, in 13 of 15 nonelection years, compared to 11 of 15 years in which elections were held. His conclusions depend substantially on excluding the Eisenhower administration, when real disposable income declined during two out of eight years, and focusing on the rate rather than direction of change. (I am indebted to William R. Keech for this observation.) In any event, structural characteristics of the American economy seem much more important in determining macroeconomic outcomes than does the rhythm of elections or the decisions of public officeholders, particularly as these factors bear on the harsh and enduring inequalities of American life. Tufte concedes some of this in a final chapter, but much of the book is written as if structural aspects of the economy, and their specific relationship to the political system, were too obvious to be addressed.

The volume's final chapter is devoted to examining policy and political implications. In the absence of elections, the Democratic party, and a few (overly) eager Republicans like Richard M. Nixon and George P. Shultz, Tufte reasons, macroeconomic policy in the U.S. would have developed along lines acceptable to the William McChesney Martins and James M. Buchanans of economic life. So the electoral process has helped keep the economy growing, has provided incentives to move large government transfer payments to those who are organized, and has maintained unemployment at levels low enough to prevent widespread working-class discontent. Like many of us, Tufte sees this as a plus. On the other hand, political economists like Buchanan increasingly

have legitimate complaints. Tufte forcefully argues there are considerable disadvantages in election-induced expansion followed by the inevitable contraction, when he writes that it "breeds a lurching, stop-and-go economy the world over. Governments fool around with transfer payments, making an election-year prank out of the social security system and the pay-roll tax. There is a bias toward policies with immediate, highly visible benefits, and deferred, hidden costs; . . . special interests induce coalition-building politicians to impose small costs on the many to achieve large benefits for the few. The result is economic instability and inefficiency" (p. 143). He concludes by suggesting some sensible remedies that would retain the representational advantages of the electoral-economic cycle but do not depend on reaching one of the Holy Grails of modern economics, some optimal point on a Phillips Curve.

The historical period covered ends in 1976. After four years of stagflation under both a Republican and Democratic administration, increasing concern over environmental destruction and the politics of scarcity, and an election year fast approaching, one wonders if Tufte's predictions about economic expansion will be sustained in the late 1970s and early 1980s. Inflation is a problem that even liberal Democrats must confront, if for no other reason than the electoral necessity of holding middle-class support.

JEFF FISHEL

*American University*

**Lobbying the Corporation: Citizen Challenges to Business Authority.** By David Vogel. (New York: Basic Books, 1979. Pp. xi + 270. \$14.95.)

David Vogel has written an extremely interesting study of citizen groups and their corporate adversaries. *Lobbying the Corporation* is a comprehensive analysis of the advocacy aimed at making business more accountable to the public.

The common goal of corporate accountability groups is to convince business people that citizens play a legitimate role in trying to shape corporate policy. Corporate decisions affect people outside the corporation; citizens must thus do what they can to influence the corporation. Business decisions are viewed by these interest groups as moral and political choices. Corporate directors and managers have, in turn, conceded little. Their general perspective is that

a corporation is autonomous, and that they have no obligation to be responsive to the demands of such groups.

Vogel traces the rise of corporate responsibility groups to the civil rights and antiwar movements. During the Vietnam War many Americans began to give serious thought to the role of corporations in American political life. Dow Chemical, Honeywell, and other corporations found themselves having to defend their "right" to pursue profitable, war-related activities. Antiwar groups were effective in raising the issue of corporate accountability, however, and further lobbying followed the war's end.

The largest portion of Vogel's study is devoted to examining specific campaigns by citizen groups to affect corporate behavior. He looks at three broad areas: shareholder proxy wars, corporate disclosure requirements, and corporate responsibility abroad. His data is comprised largely of case studies of numerous lobbying efforts. These short histories are well-documented accounts of various groups and their attempts to influence individual corporations. Included among these case studies are profiles of Campaign GM, the Angola-Gulf Oil controversy, and the struggle between citizen groups and Kodak. Vogel devotes a great deal of space to the conflict between citizen groups and corporations doing business in South Africa, clearly the most significant corporate responsibility issue of the past few years.

In his final chapter Vogel addresses the question as to whether these groups have had any impact—the bottom line, so to speak. He concludes that the direct influence of citizen groups on corporate decision making has been rather marginal. At the same time, though, Vogel argues that their influence is impressive in a more indirect way. Although corporations do not generally buckle under to hostile challenges to their authority, they now find themselves making decisions in a far different environment from the one that existed prior to the corporate accountability movement. Citizen groups have "politicized" the corporation; executives must make their decisions in light of the potential public scrutiny of their actions. Still, Vogel finds the successes of citizen groups far too limited, and he makes the case that they are unlikely to become more influential in the future. His solution to making the corporation more responsible is the traditional liberal approach: more governmental intervention.

A disappointing aspect of this book is that Vogel makes little effort to integrate his study into the larger literature on interest groups. He does not use his data to question prevailing generalizations or theories concerning interest

group organization, lobbying, or the role of such groups in a democracy. Nevertheless, Vogel's study adds immeasurably to what we know about citizen groups in American politics.

Overall, *Lobbying the Corporation* is a book well worth reading. It is an excellent work that forces one to consider the political and moral ramifications of corporate activity. Citizen groups have not yet been able to turn American corporations into the type of "citizens" that many of us might like. But their success is that they've made business people increasingly aware of the ethical dimensions of their actions. At least that's a start.

JEFFREY M. BERRY

Tufts University

**Criminal Lawyers: An Endangered Species.** By Paul B. Wice. (Beverly Hills, Calif.: Sage Publications, 1978. Pp. 231. \$15.00, cloth; \$7.95, paper.)

*Criminal Lawyers* is based on 180 interviews conducted with private practitioners in nine cities. To his credit, Paul Wice is frank in admitting that his approach was less than rigorous; he labels his methodology as one of "sociological journalism" (p. 19). He is also frank—though cryptic—in informing the reader that "it is estimated that 45 percent is strongly documented evidence, 40 percent strong belief based on incomplete evidence, and 15 percent based on insight and intuition" (p. 18). This confession suggests caution in reading this work. One is never quite sure from whence spring some of the author's contentions. Does argument A emerge from the 45 percent, the 40 percent, or the 15 percent category? Is generalization B a product of the author's insight or is it the modal response of the attorneys? Wice does present dissenting views to some of the themes he discusses, but often the reader is simply not sure about the representativeness of a particular argument.

The book is primarily concerned with describing the background, attitudes, working conditions, and work styles of the private criminal attorney. Though much of the material covers familiar terrain, and some of the analysis of the interview data is superficial, there are a number of interesting points tucked away in *Criminal Lawyers*. Especially significant and interesting are the discussions of: (1) the consequences of the increased threat of ineffective assistance of counsel suits on the behavior of defense attorneys (pp. 148–49, 165); (2) the Washington D.C. subsidy program for middle-

class defendants who cannot afford to retain counsel on their own (pp. 208–10); (3) the retention of criminal attorneys by large firms for white-collar crime, apparently a growth industry (p. 92); (4) the dynamics of fee-setting by the private attorney (pp. 108–15). In each of these areas (and in several others that space constraints preclude mentioning) Wice touches upon some new material, and whets our appetites to learn more about the issues at hand. Unfortunately, the "everything you always wanted to know about criminal lawyers" approach seems to lead him away from delving into these subjects in any detail.

A second area of strength of the book is its attempt at some comparative analysis of defense attorney behavior. Wice's thumbnail sketches of the nine communities are well done, and his delineation of the variables shaping their differences is an interesting catalogue of institutional, legal, geographic, political and social factors.

*Criminal Lawyers*, however, also suffers from several weaknesses. First, the "endangered species" theme is not well developed. What was intended here, I believe, was an argument that with the advent of the public defender system, the private counsel system of representation became "endangered." Yet with one exception toward the end of the book in which he reports the gripes of some attorneys about judges who assign public defenders to indigents who could afford counsel, and with the possible exception of opting for assigned counsel in lieu of public defenders, Wice never really demonstrates that the public defenders themselves are taking business away from the private attorneys, nor systematically addresses the question of how defendants who can afford counsel can opt for a defender. Wice reports that a consequence of diversion programs for the private attorney is that "their potential clients were now going to the public defender because his office could just as easily get them probation as a private attorney, and they could save a few thousand dollars in the process" (p. 115). But defendants cannot simply choose one over the other. The requirements for obtaining public defenders may be lax, and tightening them might make sense. But there is nothing inherent in a scheme of representation for the indigent that renders paid counsel obsolete or endangered. Relatedly, the author seems to mourn this perceived demise of the private attorney for reasons that he never makes very clear. He tells us that as his interviews progressed he became "more and more sympathetic to their plight" (p. 18), and the book is coated with a veneer of sympathy for "this beleaguered profession" (p. 13). Yet



these are the same attorneys who are antagonistic to diversion and rehabilitation programs because they "view these reforms as deleterious to their professional existence" (p. 216), and who "not only disliked their clients, who were generally categorized as either a necessary evil or simply a 'pain in the ass,' but were also contemptuous of their colleagues" (p. 219). Maybe extinction would not be such a bad idea after all!

A second weakness of the book involves its organization and style. Chapters end abruptly without much attempt at integration of the interview material. The book is dotted with seemingly irrelevant observations about the relative attractiveness of different cities and different lifestyles (see, for example, pp. 40, 82, 102), and often it is unclear whether it is the author or the respondents who are making these observations. And since no control group of attorneys was interviewed—a weakness the author recognizes—it is difficult to determine whether the personal problems and traits at-

tributed to the criminal attorney are unique, or whether these characterize other attorneys as well.

In sum, *Criminal Lawyers* is a wide-ranging survey of the lives and styles of the private practitioner. It is not as tightly presented and argued as one might like, and it does not develop its "endangered species" theme satisfactorily. Yet it also raises a number of interesting questions, presents some new information, touches upon a number of important issues, and provides an overview of what the private practice of criminal law is like. Given the current fashionableness of legal careers for undergraduates, some instructors might find this a useful supplementary text for an introductory legal process course. If nothing else, students will learn that they ought to give torts a second thought.

MILTON HEUMANN

*University of Michigan*

## Comparative Politics

**The Spanish Political System: Franco's Legacy.**  
By E. Ramón Arango. (Boulder, Colo.: Westview Press, 1978. Pp. xi + 293. \$20.00.)

There has been increasing interest in recent years in the evolution and dissolution of authoritarian governments. This is the central concern of E. Ramón Arango's study of Franco's political legacy. For Arango the origins of the Spanish Civil War, and hence of the Franco regime, lie in a late developing country's social divisions and lack of a common political culture. He attributes the longevity and apparent success of the regime to its atavistic appeal and to Franco's political skill. Civil war forged a coalition of privilege, and for nearly four decades Franco played one rightist faction against another. He gave each the necessary taste of power, but became the captive of none. Meanwhile his social programs took the edge off working-class militancy, but were not so ambitious as to bridge the cleavage on which his rule was predicated.

But if tactically adroit, Franco had little understanding and decreasing control over the new consumer society that resulted from his 1959 decision to abandon statist autarky for an increasingly unfettered capitalism. As Arango

states, economic growth produced a politically restive middle class while the maldistribution of new wealth assured continuing conflict between the old haves and have-nots. Well before the generalissimo's death, the decomposition of his regime began.

These circumstances augured ill for King Juan Carlos, much of whose shaky legitimacy derived from being Franco's hand-picked successor. But Juan Carlos soon demonstrated surprising political instincts of his own and a sense of long-term political realities far superior to Franco's. Standing at the apex of Spain's structure of privilege, Juan Carlos and his prime minister—Adolfo Suárez—have maintained the integration of their country while liquidating its rightist dictatorship. Their success—especially in maintaining the army's loyalty—stands in marked contrast to the leftist republicans and socialists who dismantled Spain's previous dictatorship in 1931.

Arango tells this story well. His generally good grasp of his subject is all the more striking because he undertook little apparent original research. His sources are disproportionately English and American, and all too often recent Spanish scholarship is neglected. His chapter on

the civil service, for example, is one of his strongest, but it makes no reference to Miguel Beltrán's work on bureaucratic elites. There are also the usual problems attendant upon writing contemporary history. Arango's concluding pages render his earlier detailed chapter on the National Movement and its potential role in post-Franco Spain of little more than antiquarian value.

Perhaps somewhat more importantly, he treats the Franco regime a bit more sympathetically than I am inclined to accept. Certainly most Spaniards were pleased by the extended social peace enforced by the regime, but it is excessive to state that in 1964 "that accomplishment alone would have been enough to justify Franco's rule" to his fellow countrymen (p. 209). Repression was a mainstay of the regime until its end. The shallowness of its roots have been more recently evident in this dismal electoral record of the franquista Alianza Popular.

Doubts also exist about Franco's "economic miracle" (p. 210). His last two decades in power saw a significant increase in national wealth. But much of that wealth derived from tourism and emigrant remittances. In other words, as a result of the limited employment opportunities and low wage and price levels associated with underdevelopment, Spain became the caboose on a train pulled by the booming economies of northern Europe. Industrial growth certainly occurred during the 1960s and 1970s, but it is still an open question whether Spain could hold its own in the EEC if its bid for membership in that organization were someday to be accepted.

Finally, there are a few minor quibbles. Primo de Rivera founded the Unión Patriótica, not the Unión Popular (p. 49), and Alfonso XII died in 1885, not 1855 (p. 30). Neither large nor small reservations, however, should detract from the main point of this review. Arango has produced a useful and well-written study of twentieth-century Spanish politics and especially of the transition from authoritarian to democratic government.

JAMES RIAL

*Allegheny College*

**Soviet Nationality Policies and Practices.** Edited by Jeremy R. Azrael. (New York: Praeger, 1978. Pp. xi + 393. \$25.00.)

Jeremy Azrael believes that nationality problems are unlikely to reach crisis proportions in

the USSR, but that "these problems are neither adventitious nor recessive, and could significantly influence the future development of the Soviet system" (p. 363). This sober approach characterizes his own examination of the political, economic and military consequences of the growth of the Central Asian Soviet population and the shrinkage of the European one. It also informs almost all the other essays in this collection, outstanding among works on Soviet nationalities for its non-polemical treatment of a highly controversial topic. In contrast to the usual conference volume, this collection contains articles of 25 to 40 pages in length, based on original research in primary sources, with extensive documentation and, for the most part, the effective application of quantitative techniques of analysis.

Articles by S. Frederick Starr on tsarist imperial policy, by Helene Carrere d'Encausse on the evolution of Soviet nationality policy, and by John Armstrong on the "mobilized diaspora" of the Baltic Germans in the tsarist empire set the historical background. Starr's excellent treatment is a rare analysis of tsarist imperialism and colonialism; one can see how, at least in this instance, Bolshevik policy departed significantly from Russian precedents. The relevance of Armstrong's highly detailed study to Soviet policies is not immediately apparent, but those familiar with his other work on "mobilized diasporas" will be able to draw the proper inferences.

Two articles, one by Yaroslav Bilinsky and the other by Charles Fairbanks, bridge the historical and contemporary. Bilinsky's essay on the Ukrainian "national Communists" Mykola Skrypnyk, active in the 1920s, and the more contemporary Petro Shelest, is informed by the writer's immense expertise on the Ukrainian Communist party and offers impressive evidence that Shelest did, indeed, follow in Skrypnyk's tradition. Shelest's well-publicized hawkishness in foreign policy—he pushed for the invasion of Czechoslovakia and against Brezhnev's policy of detente—is seen as a protective device to shield him from being accused of a "rightist, pronationalist deviation in both domestic and foreign policy: it was 'preventive hawkishness'" (p. 126). The weakness of the essay lies in its labored comparisons of Skrypnyk and Shelest and in its ultimate failure to link the two, though this does not vitiate the argument for the "remarkable persistence of Ukrainian national Communism."

Probing deeper than other scholars, Fairbanks makes a plausible argument that Lavrenti Beria tried to use the nationality issues "as his principal weapon in an attempt to become ruler

of the USSR" (p. 144). Fairbanks' persuasive, careful textual analysis shows Beria daring to oppose Stalin's "anticosmopolitan campaign" in 1949, and going against the leader's line on nationalities in a major address at the Nineteenth Party Congress (1952). Fairbanks argues that Stalin chose the nationalities issue around which to arrange his succession, while Beria tried to compensate for his lack of support within the party by appealing to the minority cadres. The fact that Beria did not succeed does not, in Fairbanks' view, prove that the nationality issue is irrelevant; Beria failed because of the East German revolt and the outcome of the "palace conspiracy."

While survey data on ethnic attitudes in the USSR are sparse, aggregate data on language use, residential patterns, and ethnic affiliation are relatively plentiful, though often tantalizingly incomplete. Jonathan Pool, Brian Silver and Barbara Anderson demonstrate brilliantly how aggregate data can be mined in order to discover trends among the ethnic groups of the USSR. All three come to roughly similar conclusions, though their foci are different: Pool argues that while Russian will not displace the 15 Republic languages, the other languages are likely to fade away; Silver shows that Russian may be acquired as a second language by many, but the next step—adopting Russian as a "mother tongue"—is a very long one indeed, taken by relatively few; Anderson shows that the peoples of the autonomous republics within the RSFSR are changing their nationality designations to "Russian." Alexandre Benningsen's essay on the "Muslim conservative opposition" takes on added significance with the increasingly visible role of militant Islam in domestic and foreign politics in a variety of countries.

To me the least persuasive essay is Steven Burg's on the "calculus" of Soviet anti-Semitism. Though he makes some factual errors (e.g., that Islamic society was free of anti-Jewish prejudices and pogroms), he sets forth an interesting thesis—that official anti-Semitism is a rationally calculated means "by which the Soviet leadership seeks to achieve its substantive policy goals" (p. 189). Burg then strains to fit available data into this argument, and to ignore that which does not conform to the 16 axioms, two diagrams, and one calculus formula he earnestly develops. Burg's "calculus" has the leadership moving against anti-Semitism when this is in their "rational" self-interest. He can only cite one passing reference in a single *Pravda* editorial as evidence of "moving against antisemitism." Likewise, his argument that the anti-Zionist campaign after the 1967 war hurt

Soviet production, research and development (because of Jewish alienation) is completely unsubstantiated, as his conclusion that "this led the soviet leadership to abandon antisemitism as a tool of policy late in 1969" (p. 208). Burg's thesis is serious and plausible, but in his overenthusiastic attempt at "scientific rigor," he becomes the willing prisoner of his own constricting formulae.

This volume is too diverse in its subjects to offer a synthesis of "Soviet nationality policies and practices," nor was that the aim of the editor, but it certainly offers fresh insights into some of them.

ZVI GITELMAN

*University of Michigan*

**Faction Politics: Political Parties and Factionalism in Comparative Perspective.** Edited by Frank P. Belloni and Dennis C. Beller. (Santa Barbara, Calif.: American Bibliographical Center—Clio Press, 1978. Pp. vi + 471. \$19.75.)

What is a faction? In this stimulating set of studies, you may take your pick. Are factions organized, relatively stable, and clearly identified intra-party groups, influencing the division of power and the making of policy on the national level? By and large, the investigations by Haruhiro Fukui, Frank Belloni, Myron Aronoff, and Arthur Cyr of factionalism in Japanese, Italian, Israeli and British parties, respectively, take this approach. From them we may glean such useful generalizations as these: intra-party factions often have their source in the groups which originally combined to form the party; faction members' apparent preoccupation with ideology and leadership are frequently mere covers for the pursuit of power and the other spoils of office; party rules devised to accommodate the presence of factions have the sometimes unlooked-for effect of institutionalizing them.

Is it useful to stretch the concept of faction to include clientelistic relationships, where organization is weaker and loyalties to individual leaders are stronger, where local politics is more often the key arena and attachment to national parties can be fragile and transitory? Those who think so will find the summary of empirical findings presented in Raphael Zariski's introductory essay enlightening, and will welcome K. G. Machado's study of factionalism in the Philippines before the 1972 coup, as well as Norman Nicholson's report on how Indian village-generated factions lack the "media of

exchange" and sufficiently generalized value structures to participate effectively in national decision making. Such readers will also appreciate Ronald McDonald's study of Colombian and Uruguayan factions in which the comparative method is used to such good effect.

So far, so good. But what shall we do about nations whose politics are so volatile that political groups keep bursting the bonds of parties altogether, forming cross-party liaisons, redividing and recombining with every electoral season? Bruce Campbell and Sue Ellen Charlton discuss these characteristics of French politics under the aegis of factionalism (neglecting in the process the opportunity to clarify machinations in the French center, but offering an insightful study of recent Socialist reorganization). Similarly, Christopher Mitchell and Jorge Nef believe factionalism can be evidenced by party mytosis within a single ideological movement and include divisions within "the left" and "the right" in their respective studies of Bolivia and pre-coup Chile.

Having gone this far, why stop? What else might we call faction? Peter Merkl labels nonpartisan voluntary associations which wrest control of local government *away* from parties as factions in his chapter on West German politics. Thomas Roback and Judson James find American factions in national candidate-oriented tendencies, cross-party legislative alliances, youth and women auxiliaries, extra-party reform clubs, and in the policy subcommittees of the parties' national committees. For Carl Linden factions may break the bonds not only of party but of nation: polycentrism in the worldwide Communist movement is his example. And Andrew Nathan takes us all the way through the looking glass: factions do not have to be seen in order to be analyzed; he suggests (not at all unconvincingly) that the Chinese have been acting for many years *as if* caught in the grip of factional politics.

For editors Frank Belloni and Dennis Beller, the question of "context" is interesting for classification purposes, but should not be made a basis for restricting use of the concept; factions *may* be found everywhere. For them the more interesting dimension is degree of organization, which they subdivide into factional cliques or tendencies, personal client-group factions, and institutionalized or organizational factions. Also intriguing to them are the causes of factions, categorized as societal, political or structural (a puzzling distinction, since several of the "political" factors they cite are systemic structural factors). But their chief concern is to give the concept of faction a new legitimacy and importance. They deplore what seems to

them an excessive preoccupation with party and eagerly point out the capacity of factions to represent neglected interests and to contain conflict. Unfortunately, this effort to clear the tarnished name of faction leads them to minimize the evidence of their own authors (especially Aronoff, Nicholson, McDonald, Fukui, Machado, Nef and Mitchell) that such functions are performed reasonably capably by factions only so long as politics is confined to intra-elite struggles, that when modernizing impulses (including the need for coherent planning as well as the push for mass participation) are channeled into factional politics, the most likely results are oppression, immobilism, right-wing coups d'état, and/or bureaucratic circumvention of factional politics in order to *get things done*.

This is not to say the editors do not have a point. If we cannot agree that factions are so frequently benign in their effects as they suggest, nor that the study of factions can reasonably be made so all-inclusive as in this volume, we can grant the need to direct more of our attention to "organized units of competition not fixed by definitions of party." It does not seem, however, that we get much farther by calling *all* such units "factions." If that concept is to be useful, it must have its parameters like any other—the efforts by Zari-ki in this volume and elsewhere would seem to be on the right track, as would the Machado definition: "Factions are groups within a party which have structure and purposes distinct from the formal structure and purposes of that party."

Furthermore, and finally, what these studies suggest is not only that we need to go beyond faction (as well as beyond party) in identifying units of competition but also that we need to go beyond identifying the units to identifying the processes of change from one to the next. Under what conditions do clientelistic networks evolve into political machines? When can we expect intra-party factions to form cross-party alliances—or to separate into individual parties? Are local nonpartisan associations ever susceptible to partisan assimilation? There are the beginnings of answers to these and related questions in the studies in this volume.

KAY LAWSON

*San Francisco State University*

**Constitutional Change in South Africa.** Edited by John A. Benyon. (Pietermaritzburg: University of Natal Press, 1978. Pp. xxiv + 297. \$15.00.)

In recent years there has been a marked upsurge in the debate about constitutional alternatives for South Africa. One of the hopeful signs in this vigorous debate is that it is now difficult to find any defenders of the status quo. There is a strong and broad consensus that radical constitutional reforms will be necessary, and that these should be designed in such a way as to ensure peaceful and democratic development. But here the consensus ends; profound disagreement prevails with regard to the precise form and direction of constitutional change.

*Constitutional Change in South Africa* represents one stage in the debate and it also offers an excellent introduction to the many issues and interests that are at stake in it. The book consists of the papers presented at the conference on "Constitutional Models and Constitutional Change in South Africa" which was held on the Pietermaritzburg campus of the University of Natal in February 1978. The conference was attended by 163 South Africans—academics, politicians, business people, journalists—representing a wide spectrum of political opinion and ethnic and cultural affiliations, although unfortunately, as the foreword points out, the views of imprisoned and "banned" individuals and outlawed organizations, which "may later prove important in any settlement," could not be represented (p. xiii). Most of the articles were contributed by the academic participants in the conference. The only exception is the chapter by D. Worrall, whose academic background is in political science and law, but who outlines and defends the constitutional proposals introduced by the National party cabinet in 1977 in his capacity as a politician and a N.P. member of parliament (pp. 127–35). At the time of the conference, the opposition parties had not yet completed the formulation of their constitutional counter-proposals. With one exception, the articles are in English. The one chapter in Afrikaans is followed by a three-page English summary (pp. 199–220).

On the whole, the analytical chapters are admirably clear and concise. Even more interesting is the lively discussion of the papers by the other participants in the conference. The editor wisely included extended summaries of their remarks in the book, and used approximately one-fifth of the available space for this purpose. The themes that are treated are the historical background of constitutional change,

the proposed alternative constitutional models and prescriptions—federation, confederation, consociation, partition, and so on—and the social, economic, fiscal, demographic, and strategic consequences and implications of the various constitutional options. Because these topics are closely interrelated and because so many authors and discussants present their views, it is a pity that the book does not have an index. The editor explains (p. xxiii) that the tight publication schedule did not permit the compilation of an index, but that the lengthy introduction, which tries to summarize the main lines of argument, is intended as a partial remedy. The introduction (pp. xv–xxiv) is indeed helpful, but it is not really a satisfactory substitute.

There are two reasons why the book is of special interest and significance. First, it is indispensable for anyone who wants to follow the evolving constitutional debate in South Africa. Second, both the chapters and the discussions abound with references to constitutional and political forms and practices in other countries; hence the book can be regarded as an example of *applied* comparative politics and government.

AREND LIJPHART

*University of California, San Diego*

**Voting, Caste, Community, Society: Explorations in Aggregate Data Analysis in India and Bangladesh.** By Harry W. Blair. (New Delhi: Young Asia, 1979. Pp. xvi + 199. \$12.00.)

Among the developing nations of the world, India is unique in maintaining two traditions, both of which are extremely important for scholarly analysis of social and political trends in the subcontinent. One is the unbroken tradition since 1950 of democratically holding competitive elections. The other is an equally important tradition of holding decennial censuses, dating back to 1872. The last complete census, consisting of some 1100 volumes, was taken in 1971.

A number of scholars have attempted to use census data to shed light on election data. This enterprise is marred, however, by one serious difficulty. The boundaries of the electoral constituencies and the census tracts do not coincide, thus making a match between census and electoral data nearly impossible. Several scholars have solved this problem by aggregating both the electoral and the census data to the level of the Indian district, where boun-

daries for both data are more likely to be similar.

Harry Blair's book is distinguished by the fact that he has managed to compare election and census data at the level of the polling booth by employing two techniques. One is a simple method of interviewing leading political candidates about the characteristics of the people in the polling station (e.g., whether a particular caste group is dominant); the other method employed is "isoplethic mapping," which consists of superimposing an electoral constituency map over a map depicting census demographic characteristics of areas (pp. 23, 36). Each constituency or electoral seat can then be appropriately coded by observing isopleths within its area.

*Voting, Caste, Community, Society* is a collection of essays of varying quality and analytic techniques. The book has no single theme or an overall thesis which can be easily summarized. Some of the questions raised are: the relationship between caste dominance in an electoral constituency and voting turnout, the differential impact of caste on electoral fortunes and debacles, the success and failure of the Muslim candidates and the relationship of their fortunes to Muslim concentration, the relationship between density of population, literacy, and rural development to voter turnout and party strength. Among the important findings in the book are these: that the upper castes are over-represented in the state legislature in proportion to their population; that Muslim representation has shifted from statewide basis to areas with heavy Muslim concentration; that population density is positively correlated with voting turnout; but that, contrary to expectations, rural development had a depressing impact on participation.

The essays are self-consciously methodological, including such techniques as simple correlations, multiple regressions, analysis of variance, and factor analysis. However, Blair seems to get bogged down in his methodological discussions. In the end, it is not clear which factors are most important and which least important for understanding electoral behavior in India.

The book is definitely designed for the specialist in Indian politics, one with a solid foundation in statistical analytic techniques. To make his book meaningful for the generalist, Blair could have been more comparative both in the context of what is already known about Indian political behavior as well as in the context of findings on political participation generally. Lacking such a comparative focus, the book is likely to stay on library shelves rather than be read by any sizeable section of

the scholarly community.

M. LAL GOEL

*University of West Florida*

**The Spanish Revolution: The Left and the Struggle for Power during the Civil War.** By Burnett Bolloten. (Chapel Hill: University of North Carolina Press, 1979. Pp. xxv + 664. \$29.00.)

The demise of the Franco regime, the restoration of monarchy, and the establishment of parliamentary democracy are recent occurrences in Spain which have attracted the attention of political observers. Given these changes, the appearance of Burnett Bolloten's comprehensive study of the Spanish Revolution is particularly timely and relevant. The study is an expanded version of the author's *Grand Camouflage*, published nearly two decades ago. Because of its wealth of documentation, detail, and insights, many specialists on Spain surely will acclaim this new work as one of the definitive histories of the Civil War.

The account traces events from 1936 to 1939. It is divided into six parts, five of which focus on the first two years and an epilogue which carries the discussion to the end of the civil war in 1939. This final chapter is to be developed into a separate volume.

The first part sketches events of the last years of the Republic up to the outbreak of civil war during July 1936. Ideological positions among individuals, groups, and parties contending for power are clearly juxtaposed so that one understands the tenuous position of the liberal republic which had prevailed since 1931. Inevitably, two Spains polarized by class and political allegiances confronted each other. Bolloten focuses here especially on the repercussions of collectivization carried out by the anarch-sindicalist and socialist movements upon artisans, small manufacturers, merchants, and peasant owners, tenant farmers, and sharecroppers.

The second part describes the rise of the Communists in the face of the emerging class struggle in the countryside and cities. Bolloten shows how the Communist party was able to embrace the disenfranchised interests of the urban and rural middle classes and defend them and their property rights in the face of the rampant collectivization drive. The Communists claimed that their struggle was on behalf of this bourgeois democratic, not the proletarian, revolution. There followed the establishment of the Seventh Congress of the Communist International in 1935.

Part 3 shows how the Communists attempted to dismantle collective farms under anarcho-syndicalist influence. However, a balance of class forces was necessary. On the one hand, a Communist alliance with the middle strata was used to undermine the strength of the extreme left. On the other, Communists attempted to gain control of the military forces.

Part 4 examines the effort to mold a regular army from revolutionary militia. Essential was the formation of the celebrated Fifth Regiment under Communist leadership, the presence of Soviet arms and military advisors, and the arrival of the International Brigades. The analysis follows developments in the government of Francisco Largo Caballero, a socialist who opposed the Communists and eventually broke with Moscow.

Part 5 looks at the struggle in Barcelona and Catalonia, analyzes the fall of Largo Caballero, and traces the emergence of the government of Juan Negrín, a sympathizer who, according to Bolloren, allowed the Communists to gain control of republican Spain.

The epilogue deals with the demise of the revolution under Negrín from May 1937 to the end of the war. Bolloren breaks this period into two parts: one in which defense minister Indalecio Prieto attempted to stem the military power of the Communist party until his ouster in April 1938; the other comprising the final months of the war, military defeats, the overthrow of Negrín and his Communist supporters by left moderate forces who unsuccessfully attempted to negotiate a settlement without reprisals.

These events are portrayed through a mosaic of quotations from speeches and writings of participants, statements from ephemeral periodicals representing various political factions from right to left, and important secondary analyses. Primary sources—memoirs, letters, interviews, manifestoes, and other documents—thus draw the reader close to the drama of revolution and civil war which Bolloren himself observed 40 years ago as a press correspondent.

This book is augmented by five maps which show the political divisions at various moments of the Civil War. In addition there are three appendices, a listing and description of the major participants, exhaustive notes, extensive bibliography, and a comprehensive index of some 50 pages.

In his foreword Raymond Carr states that this book is "a mine that will be worked over by subsequent historians. They may not accept all his judgments, but they will remain in his debt." Bolloren's work clearly is a major contribution to an understanding of the Span-

ish Revolution and Civil War. It is essential reading for all students and scholars interested in European and Spanish developments during the thirties.

RONALD H. CHILCOTE

*University of California, Riverside*

**Socialist Labor and Politics in Weimar Germany: The General Federation of German Trade Unions.** By Gerard Braunthal. (Hamden, Conn.: Archon Books, 1978. Pp. 253. \$20.00.)

Couched inconspicuously in the back pages of this impressive study of the socialist unions in Weimar Germany, primarily the ADGB, is their epigraph: "They promoted the gradual development of a democratic and social ethos in a nation not steeped in it historically" (p. 179). Gerard Braunthal analyzes this development from two broad perspectives, the historical and the topical, both of which are concisely drawn together in a concluding theoretical chapter.

The historical section, formidably buttressed by archival data and postwar as well as recent interviews with veteran union and socialist leaders, is segmented into "the formative years" of the Imperial era from the second half of the nineteenth century until 1918; the first tumultuous period of Weimar from 1918 to 1923; and the second period of political malcontent from 1929 to 1933. The relatively stable years under Stresemann diplomacy from 1924 to 1929, when the unions fared moderately well, are restricted to the analytical periphery because of the absence of critical patterns of change.

Beginning with the early repression of the unions and their rapid rise to "a respectable 'Establishment' position during the war" (p. 31), Braunthal traces the divergent phases of growth, the factional splintering, the decline and, during 1933, the final demise of the "free" (i.e., socialist) unions. He particularly emphasizes the shifts in balances of power and closeness of cooperation in the relationship between the ADGB and the SPD. The ongoing sense of fraternal cooperation between the unions and the SPD in their pursuit of pragmatic reforms within the capitalist system and of the long-range goal of economic democracy is outlined and juxtaposed to intermittent periods of union-SPD friction, in particular the unemployment issue in 1930 and, in 1932, the enamored stance of the ADGB toward Chancellor Schleicher.

A second major focus of the historical

analysis are the dynamics of internal policy choices within the free unions. Although there was a general configuration of uniform strategies and policy initiatives during Weimar, ranging from the forceful lobbying of government officials to strikes and mass demonstrations, the unions also suffered periods of turmoil and indecisiveness about goals and tactics. Braunthal poignantly questions the irresoluteness and ready political retreat of the union elite in times of stress during the 1932 Papen coup in Prussia and the Nazi consolidation of power in 1933.

The topical section of the book explores (1) the overlapping dimensions of influence between the unions and more conventional political units, i.e., the SPD, the Reichstag and the executive, (2) the organizational structure and weighting of power within the ADGB; and (3) the evolution of union economic policy. This last area includes cogent summary evaluations of the stance of the unions toward the eight-hour day, unemployment insurance, reparations, socialization and works councils, as well as long-range union interpretations of economic democracy and socialism.

Perhaps the most incisive section of the book is the brief concluding chapter, in which Braunthal assesses the plural dimensions of socialist labor in Weimar Germany and bonds together his assessments within a broad theoretical framework. Here he presents the thesis that although the success or failure of the socialist labor unions was determined largely by (1) whether the political and economic climate was favorable and by (2) whether socialists or nonsocialists were the leading policy makers, these factors did not affect union tactics and strategy. (A question emerges, however, regarding the tactical indecisiveness of union leaders during 1932 and 1933, as well as their problematic refusal to assume the reins of government, including the chancellorship, immediately after the 1920 Kapp Putsch. Do these incidents belie the thesis or constitute minor exceptions?)

Given the original division of the book into two separate sections, there remain some difficulties of theoretical exposition, which are only partially allayed by the definitive insights of the final chapter. Causal links to union goals, policies and strategies are not always clarified and might be more easily comprehended within a framework that integrates rather than dichotomizes the historical and topical investigations.

By contrast, Braunthal adopts an inclusive approach for other facets of his analysis. He draws useful cross-national comparisons among labor movements, i.e., the British and Scandina-

vian models versus union organization on the continent, and also presents where relevant, as on the issue of codetermination, linkages between Weimar and contemporary West German union politics.

In broad measure, Braunthal's study represents not only a needed and compelling addition to the literature on German trade unionism but also, and perhaps more importantly, a generous contribution to our understanding of the process of fostering the "democratic and social ethos" that was noted at the outset of this review. In that sense, the book indeed may be easily extended beyond the time range and geographical limits of Weimar Germany.

ZELIME AMEN WARD

*University of Texas, Austin*

#### **The Economic Constitution of Federal States.**

By Albert Breton and Anthony Scott.  
(Toronto: University of Toronto Press,  
1978. Pp. viii + 166. \$10.00.)

*The Economic Constitution of Federal States* is a significant contribution to the contemporary genre of literature that has in recent years appropriated to itself the mantle, if not always the title, of Political Economy. This literature is basically characterized by the application of neoclassical economic theory *qua* technique to problems that are, at least nominally, political. As such, the volume shares in both the virtues and defects of this school of thought.

Its virtues, in my judgment, arise mainly from the rigor lent to the analysis through the apparatus of mathematized economic theory. Its generic defects arise mainly from the distortion, and too often the trivialization, of the political dimensions of a problem that are necessary in order to bring this apparatus to bear. Although Albert Breton and Anthony Scott emphasize in the introduction that theirs is to be an essentially economic approach, this doesn't amount to much of a concession, since they soon go on to suggest that the only issues then remaining are legalistic or semantic. Indeed, it must be accounted somewhat remarkable in a book that describes itself as a basic analytical inquiry into the logic of federal political structure that one searches in vain for so much as a reference to the relevant classical literature in political economy (e.g., Hume, Smith, Hamilton, Madison, Tocqueville) or to the relevant contemporary literature that strives to incorporate modern economic theory into a nontrivial political analysis (e.g., V. Ostrom).



Criticism at this level aside, the volume is certainly one of the better analyses of this sort in the literature.

The early substantive section of the book is devoted to the explication and operationalization of a "centralization coefficient" that is designed to reduce certain structural characteristics of a public sector to a scalar measure. Although it is highly debatable whether this device plausibly measures the extent to which a public sector is "federal" (failing to capture, as it does, the nuances of overlapping jurisdictions and competitive authority and the consequent effective constraints on official power that are the heart of federal political systems), it is nonetheless an interesting and possibly useful measure of administrative centralization. There are some loose ends here. For example, the range of the index is never formally established (though, since it is a non-negative ratio of sums of products to products of sums, the Cauchy-Schwartz inequality suffices to constrain it between zero and one). The particular form of the index is not justified either analytically or intuitively, but by appeal to example (examples, I might note, containing computational errors). Most disappointingly, the index, once developed, is never explicitly integrated with the subsequent analysis. That is, although the symbol for the index is the key parameter in the equilibrium analysis that is the core of the book, its operational form never appears in that analysis. One is left with the feeling that the development of the index is essentially gratuitous, and should have appeared, if at all, as an appendix and not as a lead chapter.

The theoretical core of the book consists in a simple equilibrium analysis in which the magnitude and distribution of various sorts of costs (signaling and mobility costs for ordinary citizens, administration and coordination costs for government) are specified as a function of the degree of public-sector centralization. The model is then subjected to comparative static analysis as its specification is systematically made more realistic through taking into account the incentives and capabilities of the various types of actors in the system. The analysis here is often imaginative and interesting and occasionally permits some nontrivial conclusions (such as the stability conditions for the system not to degenerate towards completely monocentric or Balkanized extremes).

The third section of the book consists of a series of topical analyses conducted more or less in light of the previously specified model. The quality here is uneven and the relevant point not always clear, but the analysis of the role of interjurisdictional grants is well done

and suggestive of possibilities for useful research.

Overall, the book is rather disjointed, and is often awkward in development. As a single volume, it is not well motivated. Some strong editing would have helped. Substantively, it must be seen as a noteworthy contribution to the contemporary literature in the area.

VERNON GREENE

*University of Arizona*

**The Urban Question: A Marxist Approach.** By Manuel Castells. Translated by Alan Sheridan. (Cambridge, Mass.: MIT Press, 1977. Pp. x + 502. \$20.00.)

For some time now, efforts have been under way in the major advanced countries to reconcile the urban movements of the 1960s and 70s, together with the larger "urban crisis," with the Marxist intellectual paradigm. This development has produced many creative and provocative variants. The net result has been a reinvigoration of the literature on urban politics.

Because France has had both a strong history of left-wing party activity and a strong Marxist intellectual tradition, it has produced one of the richest examples of this "new urban politics." Although the scholars who have built up this literature in France are, as one might expect, a varied and highly polemically skilled group, Manuel Castells is perhaps its best-known and most prolific exponent. Castells is an exiled Spanish intellectual who has managed to climb to the pinnacle of the French academic system—the Ecole des Hautes Etudes. Given French ethnocentrism, this in itself was a major accomplishment. Well shy of 40, Castells has published, at last count, eight major books on subjects ranging from urban renewal and urban protest to regional economic development and Latin American urban politics. He has taught for varying periods in Canada, the U.S., and Chile, and maintains an extensive network of contacts in Spain and Italy as well as throughout Latin America. In *The Urban Question*, Castells presents the initial theoretical framework in which he intended, as of 1972, to situate his subsequent research efforts. The book is important because, however flawed, it remains just about the only such general theoretical effort the French Marxist urbanists have produced. (Castells has appended an afterword

to the 1977 English edition which criticizes his own formulations. For those wanting to extract the nub of Castells' argument from this difficult-to-read book, this is the place to look.)

The urban crisis has presented Marxism with an interesting challenge because its major themes—spontaneous political action, a base in ethnic neighborhoods rather than the workplace, and an emphasis on the stability of urban service bureaucracies—have only a minor, “secondary” place in the traditional Marxist intellectual constellation. One looks to this book, then, for answers to one basic question: how can we get from the structure of the capitalist economy (in which a class structure based on roles in production has theoretical primacy) to the specifics of urban political actions (often based on multi-class, residential social bases) and back again? The book stands or falls on how well one thinks it does this job both on theoretical and illustrative or empirical grounds.

Castells does three things to make his case. He first criticizes urban theories, primarily the Tonnies-Simmel-Park-American urban sociology school, which do not center their analysis on the capitalist mode of production. This purely critical part of the book is the most compelling, although it operates on a more purely epistemological plane than suits my taste. Secondly, Castells retools the French structuralist Marxist conceptualizations advanced by Althusser, Balibar, and Poulantzas, for use in the urban setting. This part of the book is least convincing, because it appears mechanistic, static and purely classificatory. Reliance on the structuralist intellectual inventory has led Castells to decorate his book with terms which superficially appear to be scientific but actually hide a great deal of vagueness. Words like “correspondence,” “instance,” and “conjunctural effect” are among the worst of the offenders, and the whole terminology will tend to thwart, and perhaps infuriate American readers. Indeed, the Althusserian penchant for trinitizing each mode of production into economic, politico-juridical, and ideological “instances,” and further for decomposing any given stage of development into the “conjunctural” interplay of its different coexisting modes and instances will strike many as resembling Parsons' A-G-I-L scheme. (Whether the resulting mechanistic formalism can be entirely or only partially attributed to Althusser himself, I am uncertain; in his afterword, Castells himself criticizes his “extremely formalized theoretical system” as “merely a classification grid.”)

Finally, the book turns to a third task: applying these conceptual distinctions to a series of case studies in urban planning (urban

renewal in Paris and the United States) and urban protest politics (in Paris, Montreal, Chile, and the U.S.). Because he introduces urban politics as a subject whose origins may be understood in structural terms and whose impact may produce “a qualitatively new effect on the social structure” (p. 262), one hopes for far more from Castells than one gets. On the determination side, Castells merely asserts that urban planning initiatives arise from problems which “run counter to the interests of the dominant classes and disturb the functioning of the urban system,” and which therefore “demand intervention” (p. 287). He goes on to explain planning interventions by their effects, a methodological faux pas which is only the most noteworthy of many. Castells' path model diagrams will cause American readers, grounded as they are in empirical analysis, to shake their heads in disbelief. On the political response side, Castells' cases of protest only weakly show structural determination. In short, Castells' empirical illustrations of his theoretical apparatus only further undermine the reader's faith in Marxist structuralism of the Althusserian brand as a useful tool for explaining urban planning and politics.

But if *The Urban Question* fails in its basic quest to take us from economic structure to urban political action and back again, it still makes three important contributions. First, its critique of environmental determinism in urban studies is devastating. Castells lays to rest the notion that “the city” or “the urban” can itself be an actor or an explanatory variable. Environments only provide a structured context in which actors engage in practices and make choices. Secondly, Castells opens the way for a real advance in Marxist theory by arguing that politics, though secondary in theory, is primary in many practical circumstances and must be placed at the center of scientific explanation. Though Castells' structuralist formalism does not provide a convincing vehicle for this type of explanation, he at least points out the central theoretical problem. Finally, almost against his will, the strength of Castells' empirical material carries him from the realm of production-generated classes to the realm of collective consumption interest groups. It is in the attempt to relate the politics of reproduction systematically and theoretically to the politics of production that Castells promises to make his greatest contribution both to Marxist theory and to urban studies. By claiming that reproduction and collective consumption lie at the heart of the urban question, Castells has taken a bold step which orthodox critics will probably say has taken him too far towards Max Weber. For

me, though, it is a journey to be encouraged and watched closely.

JOHN MOLLENKOPF

*Stanford University*

**The Soviet Theory of Development: India and the Third World in Marxist-Leninist Scholarship.** By Stephen Clarkson. (Toronto: University of Toronto Press, 1978. Pp. xii + 322. \$17.50.)

The August 1979 convocation of the International Political Science Association in Moscow underscored the increased presence of Soviet social scientists in international scholarly interchanges. As the novelty of participation by the Soviets finally begins to diminish, we can hope for a more sober assessment of the contribution of Soviet social science as it has been applied to the analysis and understanding of important global issues. Stephen Clarkson's book is a valuable step in this direction, since it attempts to examine Soviet scholarly writings "at their face value and . . . in their own terms as intellectual contributions" to the understanding and possible solution of problems of Third-World development (p. 6).

Since the quantity of Soviet material on the Third World has burgeoned in the last two decades, Clarkson has chosen to focus his evaluation on Soviet "political economy" writings on India, the country which has occupied the central place in Soviet development theory. However, he seeks to distinguish his own analytical focus from that of other recent monographs on Soviet policy toward India. Rather than examining Marxist-Leninist doctrine at the level of strategy for the communist movement or for Soviet foreign policy, Clarkson instead aims to concentrate on its "developmental and theoretical significance" (p. 15).

Clarkson is to be commended for his scrupulous fairness to the Soviet authors whose works he reviews. Indeed, his orderly organizational framework and lucid exposition impart to the "Soviet theory of development" a greater sense of coherence and stability than the original works themselves possess. In the process, the reader loses sight of the dynamic quality of a frequently changing Soviet analysis. Moreover, even though Clarkson does differentiate between the approaches of "radical" and "moderate" scholars, he makes little effort to convey the distinctive biases or relative scholarly or political standing of individual Soviet scholars. Another shortcoming in Clarkson's approach is the neglect of Soviet studies of Third-World

countries (such as those in tropical Africa) that are less capitalistically developed than India; the most notable examples of "creative Marxism-Leninism" are to be found in development studies not covered here.

These deficiencies in scope and method are far outweighed by the sound scholarship and penetrating analysis contained in this monograph, which is the product of 16 years' research. Despite his efforts to evaluate it as serious scholarship, Clarkson has found in Soviet development theory a web of contradiction and confused ideological thinking proceeding from a "crude they-are-bad-we-are-good" approach that "leads to a whole series of positions unacceptable to the non-believer" (p. 193). Soviet writers employ Marxist-Leninist concepts that are both value-laden and poorly suited to Asian or African social structures; the products of their scholarship are thus characterized by "a vocabulary and style of writing that makes their output as distinctive as it is dismissable" (p. 251). Policy conclusions are shown to follow not from independent scholarly analysis but from prior political judgments of the "progressiveness" of a given regime's policies toward the USSR.

If, as Clarkson concludes, the vast body of Soviet writing on the Third World has only limited significance as a model or theoretical guide for development, does it have greater impact in other realms? In his estimate, such scholarly output is not a reliable guide to Soviet foreign policy, has no real impact on Soviet foreign policy making, and has little influence on foreign communist parties. Clarkson concludes on the basis of interviews with leading Indian opinion and policy elites that "neither in theory nor in practice have the Soviets had any noticeable impact on Indian elites' ways of thinking or acting" (p. 262).

Clarkson's research thus supports the conclusion that the wells of "creative Marxism-Leninism," as directed toward analytical and policy issues of Third-World development, have long ago run dry. This conclusion, while hardly novel, is here given added credibility both by the solid research on which it is based and by the genuine effort of this Canadian political scientist to give Soviet scholarship the benefit of the doubt.

Sadly, the findings of this study give no reason to think that the vision of genuine mutual interchange and learning between Soviet and Western social scientists will be quickly or easily realized. Clarkson's own conclusion is a sobering warning to Soviet scholars of development who seek to have their works taken seriously outside their own country: "Until"



quality prevails over quantity in the research institutes of the USSR, they will be more likely to serve as an uncertain guidebook for watchers of the Kremlin than as an alternative paradigm for students of the third world" (p. 268).

ROBERT H. DONALDSON

*Vanderbilt University*

**The Difficult Flowering of Surinam: Ethnicity and Politics in a Plural Society.** By Edward Dew. (The Hague: Martinus Nijhoff, 1978. Pp. 234. Guilders 37.50, paper.)

This book throws light on how Surinam developed into a viable democracy through an in-depth analysis of the political system of this small tropical country of many nationalities and languages. It is an unfortunate characteristic of American students of comparative politics to avoid research subjects—when they do not touch upon an aspect of immediate significance in present-day world affairs—in which the primary resources are in unfamiliar languages. It is, therefore, a pleasure to see that this barrier has not impeded Edward Dew in his thorough analysis of the intricate political relationships of multiracial Surinam. Archival research and perusal of the press of this nation demanded the knowledge of languages not generally taught in North American universities.

The ethnic mosaic of this former colony of the Netherlands, on the Atlantic coast of northern South America, has caused it to have a bewildering political composition of parties and factions which on first glance give an impression of dangerous instability. Serious racial, religious, and ideological differences made Surinam's quest for independence, in the early seventies, appear to be a pursuit of doom. Many political observers had grave doubts about its future. There was a belief that the three major racial groups, the Creoles, the Hindustanis, and the Javanese, would be locked in fratricidal conflict. Independence would not bring genuine freedom, but riots and disorders more calamitous than those of neighboring Guyana. The final consequence would be an oppressive authoritarian regime. Happily for Surinam, these predictions of catastrophe proved false. Sovereignty did not bring dictatorship, but on the contrary, a modern and vigorous democracy.

Why did democracy, which has failed in so many young Third-World countries, flourish in Surinam? This is the question the author poses himself. Before answering it, he reviews Suri-

nam's political history. He examines first how Dutch colonial viewpoints on economy affected geographically and socially the settlement of the country; second, how shifting colonial policies with regard to ethnic issues caused differences within racial blocs which in later years forestalled easy transformation of ethnic strength into political power; third, how parties based on modern ideologies had little chance of success among the Asian groups, the Hindustanis and Javanese. Instead of attempting to erase cultural differences, colonial rule had accentuated them. Preservation of ethnic and religious identity, therefore, played a more important role in the politics of these groups than a desire to reshape society and better one's individual economic position according to the appeals of new political theories. At times, this attitude gave Surinamese politics a rather rigid conservatism which, it would seem, could make interracial cooperation impossible and could become the cause of a violent explosion. That this did not happen the author attributes to the party system. Economic and cultural factors, and in part the electoral system, caused ethnic groups to be represented by myriad parties. To govern, both the large Creole and the large Hindustani party needed the support of several smaller ones. Governments were, therefore, in nearly all cases coalitions in which all racial groups were represented. This fact compelled these governments to follow policies accentuating interracial cooperation. Dew explains how this is done very well, and his analysis of party elites is admirable.

I would have been interested in a more profound study of the factors that give Surinam's present parliamentary system such strong legitimacy in the eyes of the people. It is important to realize that the competing political elites are quite sensitive to accusations of not having observed the rules of the system. Unlike the people of many other developing countries, the citizens of Surinam have a deep respect for their democratic institutions. Tampering with the checks and balances provided by the Constitution is not to be permitted either by a charismatic leader or a "man on horseback." I hope that this writer whose enterprising and meticulous pioneering scholarship has highlighted so many facets of ethnic politics may decide to examine further the sociocultural roots of the legitimacy of Surinam's constitutional system.

ALBERT GASTMANN

*Trinity College*

**Distrust and Democracy: Political Distrust in Britain and America.** By Vivien Hart. (New York: Cambridge University Press, 1978. Pp. xvii + 251. \$19.95.)

This is a puzzling book. Vivien Hart addresses one of the most important phenomena of contemporary mass politics: the rise of significant, pervasive, and highly vocal discontent with the political process, with political leadership, and with policy outcomes in Great Britain and the United States. The puzzle begins with organization. At the outset Hart develops a sharp distinction between political distrust on one hand—the unfavorable but quite rational evaluation of politics by citizens judging reality in terms of the “mainstream” norms of a democratic political culture—and alienation, apparently identical in her view with anomie, on the other. The analysis thence proceeds to a detailed discussion of certain contemporary survey data, coupled with a blistering attack on Jack Dennis’ gloomy conclusions about the rejection of existing politics by British youngsters. Then we abruptly turn to the book’s core, two chapters on nineteenth-century “radicals,” Kansas Populists circa 1890 and the “rank-and-filers” of the Birmingham Liberal caucus in the mid-1880s.

Hart’s purpose seems clear. Her central proposition is that people who are distrustful of politics are essentially rational actors reacting to a perceived gap between democratic promise and actual political performance. But that democratic promise is not somehow alien to the established political culture, still less a radical critique of its assumptions about power, authority, and human nature. Rather, it is of the essence of the mainstream. This proposition leads to two important conclusions. First, the increasingly influential school of “democratic elitists” have missed the point about protest from the distrustful. This protest is in no sense a genuinely radical attack on the existing political order. Accordingly, it is not to be misunderstood as a serious threat to existing institutions, nor should political elites and the intellectuals who support them use this protest to express their fears about the “crisis of democracy,” but rather as an occasion for self-criticism: rational protest by the distrustful (who are not the prisoners of various worrisome psychological disorders) arises when the elite themselves, by their actions, widen the gap between promise and delivery. Second, survey analysts and other political scientists should be very careful to avoid the apocalyptic, or to inflate the significance of expressions of political disaffection.

This argument contains much truth. No doubt the “rank-and-filers” of the Birmingham Liberal caucus did not view politics as the primary vehicle for social change. Similarly, much of Populist “radicalism” was hardly alien to very important aspects of nineteenth-century American political culture. Also, this study duly—and accurately—confirms well-known differences in American and British political cultures. Students of politics, no less than worried elites unused to being challenged by sectors of the public, may well remember that organized political protest in these countries has always reflected some features of an ambiguous but hegemonic political code. The larger this movement, the more faithful will be this (albeit skewed) reproduction of traditional mainstream themes.

Yet how far does such analysis take us? This study is vulnerable to a number of criticisms. For example, while the American Populists were no doubt authentic representatives of an American political tradition, they also were engaged in the most serious critique of emergent corporate capitalism and its march to hegemony which the United States has ever produced, as witness the behavior of frightened contemporaries in the industrial heartland. See the recent and magisterial analysis of this movement by Lawrence Goodwyn’s *Democratic Promise* and Louis Galambos’ *Image of Big Business in America, 1880–1940*. To say that Populists were, after all, all good American liberals would miss the point even if that were wholly true.

Another problem is the extraordinarily isolative character of this analysis, if genuine concern for historical dynamics is involved. First, Hart largely concentrates on criticisms of politics and the “growing complexity of government” around 1890, and in doing so misleads us as to the basic point of the critique. This was not governmental complexity but the utter domination of government by corporate capitalism and its agents. While some of the rhetoric dealt with legal-institutional boundary problems (such was currently the dominant mode of public discourse), the Populists which Goodwyn describes did not see political institutions in splendid isolation from the socioeconomic uses to which they were being put, or *could* be put.

Connected to this is the failure of more than a tendentious link between past and present. For instance, the 1890s Populists were a small minority, never winning more than 11.5 percent of the total national vote. If we assume that the distrustful expressed themselves in protest movements in electoral politics, then at

least five-sixths of the very fully mobilized electorate of that era would have to be excluded. If we assume a much larger proportion of the distrustful, then we need to explain the extraordinary turnouts and the equally extraordinary devotion to their major parties displayed by the overwhelming majority until the general smashup of 1894–1896. In any case, as the Harris data shows (p. 69), in the 1970s more than a majority of the entire adult population believe themselves “alienated and powerless.” Clearly, the “distrustful” are now to say the least very numerous—almost certainly a larger proportion of the electorate than in the 1890s. One of the largest turnout rates of all time for an off-year election occurred in 1894, when 73.4 percent of the electorate outside the South voted. Not surprisingly, the smallest of all time was reached in 1978, when the non-Southern turnout rate hit 37.9 percent of the potential electorate.

As I have often said, American politics in the late nineteenth and late twentieth centuries are very different breeds of cat. Then, disenchantment with the existing political order led to intense mobilization through parties. Now, it leads to the dissolution of parties and massive, heavily class-skewed abstention from voting. Parties, it is commonly supposed, are important integrative and communicative links between rulers and ruled. Their dissolution in the current era raises problems of a kind never seen in the past 150 years, and approximated only during the height of industrial-capitalist hegemony in the 1920s. In this context, Hart’s reflection makes strange reading: “The outburst in America in the 1960s was a return to normal rather than a dramatic collapse of normative integration—the distrustful there are not philosophical extremists but are true to the mainstream of the American political tradition” (p. 182).

This statement gets us near the heart of the problem. Like many others, this statement has its grain of truth. But in a broader context it is profoundly misleading—as it is misleading to imply that, because Populists were in the American democratic tradition, they and their critique of the emergent corporate political economy were non-threatening to the established elites in that political economy. This study’s flaws are ultimately conceptual flaws. It lacks a sense of historical transformation, of the links between government and dominant economic power which occasionally give rise to protest among the distrustful, or of the fundamental implications for the problem of distrust caused by the very recent change in what the state does *to* as well as *for* the citizenry. One is

left with the impression that “distrust” operates in a narrowly political and wholly timeless continuum, and that it is wholly non-threatening.

This is all rather precisely liberal, to be sure. But mainstreams are both very broad and ambiguous affairs. Who can doubt, after all, that Hitler and the Ayatollah Khomeini reflected important elements in the mainstreams of their respective cultures? The analysis of such right-wing revitalization movements would require quite different analytic tools than are provided by the liberal academic mainstream represented here. So, one suspects, would the analysis of the etiology, scope, and implications of the *present* mass hostility to “politics” in the United States and Britain.

WALTER DEAN BURNHAM

*Massachusetts Institute of Technology*

**Housing Policy in the Developed Economy: The United Kingdom, Sweden and the United States.** By Bruce Headey. (New York: St. Martin’s Press, 1978. Pp. 276. \$21.95.)

In this valuable survey of the development of housing policy in Sweden, Britain, and the United States, Bruce Headey shows how divergent policy outputs among “similar systems” can be. To summarize his conclusions briefly, Headey finds that in Sweden the government has achieved high levels of housing construction and design along with a major redistribution of housing services and costs in favor of working-class families. “The position has been reached in which the dwellings and neighborhoods of most working-class families are of comparable quality to those of most middle-class families. Furthermore, due to state subsidies and housing allowances, working-class people pay less for their housing than middle-class people” (p. 229). There has been a coincident effort (at least until recently) to encourage the construction of multi-family rather than single-family housing.

The contrast with the other countries reviewed is sharp. In Britain, the more modest aim of security Beveridge-type “minimum standard” housing may have been achieved. The price in financial resources and political controversy, however, has been high. Working-class housing remains qualitatively distinct. And subsidies as a whole continue to benefit middle-class homeowners over working-class renters. In the United States, tax subsidies and timely intervention during the Great Depression have

so far helped to promote a life of suburban ease for the majority of middle-class Americans. The cost has been borne by the less well-off and by those currently without a private home. To use Headey's terms, vertical and horizontal inequalities have been increased.

The great merit of this book is to present and contrast these developments clearly and persuasively. The argument is further advanced by clever calculations of rates of return on housing and by some trenchant criticism of British housing policy in particular. (I have the impression that Headey considers American policy hopelessly regressive. He could well be correct.) Finally, Headey outlines proposals that would move housing policy away from providing disproportionate assistance to higher-income families, which, to repeat, is presently the rather perverse situation in Britain and the United States. These proposals are carefully thought-out and are derived logically from the main body of the text.

What the book accomplishes less successfully is the elaboration of an explanatory scheme to account for the differences that are described so well. Plausible explanations abound—Headey mentions divergent orientations of the major "left" party in each country, institutional constraints, the presence or absence of one-party dominance, ideology (public opinion), differing government-interest group interactions. These ideas, however, are not linked or ordered systematically. To approach the matter from another direction, since Headey does not relate housing policy to other policies or to patterns of state action, the significance of his findings is less clear than might be hoped. This problem, I must note, is not relieved by the rather ad hoc use of James Q. Wilson's categorization of policy programs in cost-benefit terms. These reformulations, when not merely intrusive, tend to obscure more than to enlighten. The elements of a compelling explanation need more rigorous articulation.

In sum, while Headey may fail to advance the at best nascent attempts to develop appropriate theories, in outlining what has happened and how, he is superb. The book warrants wide attention as one of only a few comparative policy studies with a purpose. This attention would be facilitated by issuing a paper edition which would help generate the audience, particularly among students, that the book deserves.

NORMAN FURNESS

*Indiana University*

**Elections Without Choice.** Edited by Guy Hermet, Richard Rose, and Alain Rouquié. (New York: Wiley, 1978. Pp. xi + 250. \$14.95.)

Most of the publications of the Committee on Political Sociology of the International Political Science Association and the International Sociological Association that have appeared so far have concentrated on the political and electoral processes of Western countries. The present volume deviates from this pattern by focusing on the function of elections, prevalent in the non-Western world, that are not free and competitive. It is the first full-length and broadly comparative book on "elections without choice" following the earlier shorter analysis by Richard Rose, one of the editors of the book under review, and H. H. Mossawir in the journal *Political Studies* in 1967 (Vol. 15) and the pioneering articles on sub-Saharan African elections by a number of scholars, some of whom also contribute to the present volume: Joel D. Barkan, John J. Okumu, and Denis Martin.

*Elections Without Choice* begins with two analytical chapters. Guy Hermet presents a conceptual framework for the study of state-controlled elections, and Alain Rouquié discusses the nature, types, and functions of elections that are not controlled by the state but that are nevertheless not really competitive as a result of the strength of clientelist relations. Of the other eight chapters, five are case studies and three are comparative treatments of groups of similar countries. The single-country analyses deal with Cameroun (authored by Jean-François Bayart), Kenya (Joel D. Barkan and John J. Okumu), Tanzania (Denis Martin), Syria (Elizabeth Picard), and Portugal (Philippe C. Schmitter). Juan J. Linz's discussion of non-competitive elections in Europe first deals with the crucial semi-free elections in Germany in March 1933 and in Italy in April 1924 that helped to establish dictatorial rule in these countries, but then becomes a wide-ranging analysis, not strictly limited to Europe, of elections in totalitarian, authoritarian, and pseudo-constitutional regimes. It partly overlaps Alex Pravda's chapter on Communist party states which is mainly based on evidence drawn from five countries: the Soviet Union, Czechoslovakia, Poland, Hungary, and Yugoslavia. Richard Rose's final chapter is a paired comparison of elections *with* choice but without full legitimacy and consent in Northern Ireland and the American Deep South.

It is very useful to be reminded of the fact that competitive free elections are the excep-



tion rather than the rule in the contemporary world. Almost all states conduct elections of one kind or another but, as the editors emphasize, elections without choice represent "the ballots most frequently held" (p. vii). Secondly, although it is tempting to dismiss such elections because they do not perform the crucial functions of free elections in Western democracies, "the fact that elections do not have the same meaning when they are without choice is not evidence that they lack any meaning. Instead, it is an indication that their meaning is different" (p. vii). For instance, Hermet identifies four functions of state-controlled non-competitive ballots: communication, education, legitimation, and the adjustment of the internal equilibrium of the governing circles (pp. 13-17). And it is also worth remembering that elections do not have to be perfectly free and competitive in order to give the voters at least a *degree* of democratic control.

The three criteria that Hermet uses to distinguish between democratic and non-democratic elections are unexceptionable: freedom of voters, competition between candidates, and a meaningful effect on government policies. However, the way in which the authors interpret and apply these criteria is often unrealistically restrictive and betrays a strong inclination to adopt the Westminster model of democracy as the ideal norm—a surprising bias since almost half of the volume's contributors are French. For instance, government policies can be profoundly affected by the outcomes of elections even when these do not entail "replacing office-holders by leaders of the opposition," which Hermet calls "the fundamental rule of elective representative democracy" (p. 4). A similar narrow interpretation is offered by the three editors in the preface when they imply a condemnation of elections in multiparty systems because these may not offer "a verdict upon a government" but only the choice of representatives to negotiate a coalition government (p. viii). And they list "an executive responsible to an elective body" as one of the preconditions for democratic elections—thus unnecessarily excluding legislative elections in American or French-style presidential systems (p. viii). It is equally unnecessary to interpret the criterion of voters' freedom as requiring the absence of "categories dividing the electorate and revoking the idea of popular sovereignty" (p. 3). Geographical divisions of voters are a common feature of democratic elections, and it is not clear why non-geographic divisions (e.g., ethnic categories) should be considered inadmissible if each category is entitled to a fair share of the total representation. In fact, an

explicit recognition of the need for special rules for minority representation may be the only way to remedy the systematic suppression of minorities to which majority rule may lead in severely divided societies such as Northern Ireland (see Rose's discussion, pp. 206-10). A final example is Rouquié's unqualified labeling of election systems in divided societies with a high degree of "vertical solidarity," in which voters naturally divide themselves into ethnic parties, as "structurally authoritarian" (p. 22).

These conceptual and definitional problems indicate that *Elections Without Choice* is not the definitive comparative work on the subject. But its empirical breadth as well as its theoretical depth and originality make it a highly significant initial global-level exploration of the diverse functions of elections in different types of regimes.

AREND LIJPHART

*University of California, San Diego*

#### **Civil-Military Relations in Communist Systems.**

Edited by Dale R. Herspring and Ivan Volgyes. (Boulder, Colo.: Westview Press, 1978. Pp. xiii + 273. \$21.00.)

This is a collectively authored volume whose title is self-descriptive: it is about civil-military relations in communist systems. Like most collectively edited volumes, the quality of the chapters varies considerably. Like all too many collectively authored volumes, several of the articles have appeared elsewhere. Unlike many edited volumes, fortunately, it has a theme; Dale Herspring and Ivan Volgyes have put together a book, not a non-book. Its theme, namely, the nature of party-military interactions in communist systems, is nicely told by three of the better statements about those interactions in the Soviet context. Roman Kolkowicz's image of Soviet civil-military relations as entailing constant conflict between "two definable and dichotomous institutions" (Dale Herspring's introduction, p. 1) is presented. William Odom's view that the military and the CPSU are similar and highly interpenetrated institutions is also reproduced. Finally, Timothy Colton articulates a participatory model which envisages the military as an institution whose interests have thus far largely been satisfied by the Communist party. In particular, Colton asserts the party has given the military a sense of participation in decisions, has allowed for the considerable self-regulation of the military, and has generally pursued policies that are congruent with mili-



tary interests. These alternative perspectives are followed by country studies treating the Soviet Union, the German Democratic Republic, Hungary, Rumania, Yugoslavia, Cuba and China. Jaded peruser of collective volumes that I am, I am pleased to report that several of the country studies actually make reference to the general perspectives articulated by Kolkowicz, Odom, and Colton. In short, the editors have rendered a useful service by bringing together a reasonable sampling of articles dealing with civil-military relations in communist systems, a collection that should prove useful for advanced courses in comparative communist systems.

What the book does not accomplish, however, should also be noted, if only to identify what ought to be a major item in the research agenda for Soviet and more generally communist studies. The volume presents three of the major plausible alternative perspectives on the communist military: as an interest group ranged consistently against the party qua apparat, as an institution indistinguishable from a thoroughly militarized society, and as a participating institution whose support is cultivated by the party leadership. The volume fails, though, to provide much by way of empirical evidence with which to assess the relative explanatory potency of the three perspectives. There are numerous methods by which this task could be accomplished. An impressive example of one way the behavioral consequences of the alternative perspectives could be specified and then evaluated against the realities of communist civil-military relations is provided by William Ting's article on Chinese factionalism in the June 1979 issue of this *Review* (73, pp. 478-93). Whatever the chosen methodology, however, it is only through such efforts that scholars will be able to develop criteria for preferring one perspective over others, or, even better, for specifying the circumstances in which, for instance, party and military interests are likely to conflict, when some party apparat elements and some military elements are likely to clash with other groupings within the party and the military, when the military and the party are likely to coalesce against other key actors in communist political systems, and when (if ever) it makes much sense to think of communist systems as permeated by the military.

WILLIAM ZIMMERMAN

University of Michigan

**The Political Economy of Inflation.** Edited by Fred Hirsch and John H. Goldthorpe. (Cambridge, Mass.: Harvard University Press, 1978. Pp. xi + 307. \$14.00.)

We on the dark side of the Phillips Curve sense that inflation may be the existential theme of the *fin de siècle*. Technical debates about monetary and cost-push causes of rising prices have been overwhelmed by widespread public protests and tax revolts. Crises in the world political economy and energy supplies underscore the social and political nature of the inflationary experience. Prices go up, paper currencies go down and everyone searches for a financial security to counter spreading uncertainty. As our situations become inflated, we feel driven to invest ourselves elsewhere to hedge against the risk of losing our value.

Many who should know better believe that we need a seminal thinker to solve our economic crisis and inflation problems—a new Keynes. This nugget-packed volume edited by the late Fred Hirsch and John Goldthorpe suggests that this is not the case: multidisciplinary teams are better capable of taking apart our situation, even if they cannot promote a golden panacea to solve our inflation sickness once and for all. With more doctors, there is less chance that a single doctor's cure will be worse than the disease.

Inflation, argues John Flemming, is essentially a monetary ailment characterized by an overdose of liquidity provided by profligate money suppliers steered by government policy. Historian Charles Maier views inflation in twentieth-century capitalist societies in terms of distributive conflict and system stability, calling the concept of growth as a surrogate for redistribution "the great conservative idea of the last generation" and indicating that indexation appeals when "guidelines" or social compacts fail. In contrast, the communist states of Eastern Europe have largely avoided inflation over the past two decades, even though money incomes and real consumption have grown, argues economist Richard Portes. David Pichaoud looks at the effect of inflation on unequal income distribution in Britain, while Alan Peacock and Martin Ricketts maintain that the size and growth rate of the public sector of a national economy are directly related to the rate of inflation. Showing stronger statistical support for his case than Peacock and Ricketts, Milivoje Panić, a bank advisor, relates the rate of inflation inversely to the levels of income and consumption that a nation has achieved, but relates it positively to the

degree of inequality in its internal income distribution.

In the piece by Samuel Brittan, an economic journalist, political actors—voters, bureaucrats, politicians—all “maximize” as much as they can, stimulating excessive injections of money into the economic system and threatening the existence of liberal-democratic institutions. Starting with the inconvenient “residual categories” which economists’ theories of inflation often exclude, sociologist John Goldthorpe argues that high rates of inflation reflect recent changes in the form of social stratification within capitalism, which has taught the “mature” working classes to exploit their bargaining position on the market to the full. Such maximizing behavior is a justifiable expression of citizenship rights in a decaying status order by formerly dominated interest groups. Colin Crouch, also a sociologist, treats inflation as one of society’s interest-regulation problems. The powerlessness of state institutions to confront inflation is the focus of Malcolm Anderson, a political scientist. And economist Fred Hirsch sums everything up in terms of the conflict between the classical equilibrium market ideology and Marxist analysis: “Both see the economy as governed by the imperatives of a closed system of abstract economic relationships, but where the one model programmes the variables for harmony, the other sets them for auto-destruct” (p. 273). The book is a superlative example of a cooperative sociology of knowledge stemming from the Warwick Conference of 1977 which makes much individual research appear selfish, if not inadequate, in comparison.

Inflation appears as an individualist scar in the superego of capitalist societies, resulting in part from the high expectations promised by quasi-free market systems, and representing a cover for the social costs of the gap between the demands of individual-maximizing behavior and the ability of democratic political economies to satisfy these demands. As paper money, grades, degrees and academic publications become more liquid and more liberally distributed, their individual values go down, but not the expectations of their recipients. To go against the mainstream “redistribution ideology” of inflation is non-democratic and technocratic, and such policies are unlikely to be carried through consistently enough to be effective in the politicized economies of late twentieth-century democracies.

ROBERT A. ISAAK

*Johns Hopkins University*

**Origins of the Israeli Polity: Palestine under the Mandate.** By Dan Horowitz and Moshe Lissak. (Chicago: University of Chicago Press, 1979. Pp. xii + 292. \$19.00.)

In this volume two Israeli political sociologists analyze the social and political systems of the Jewish settlers in Palestine—the *Yishuv*—during the British Mandate preceding the establishment of statehood in 1948. To present a systematic, detailed and sophisticated analysis of a very complex and continuously developing society, with reference to different levels of political organization, in 250 pages of highly readable text, is by itself an accomplishment. Concerned about a proper methodological mix between a descriptive case study and a generalization-oriented social science analysis, the authors end up writing a good traditional political science study, frequent reminders of their sociological allegiances notwithstanding.

As the main organizing concept for their analysis Dan Horowitz and Moshe Lissak have chosen Shils’ “center-and-periphery” model, although they admit that “the center was not the predominant source of authority . . . entrepreneurship and symbolic creativity” (p. 10). In order to interpret the coalitional structure of the “national institutions” and their coordinating role in the relationships between ideologically competing sociopolitical movements, the authors combine the center-periphery dimension with the “consociational” model of democracy. Structural models, however, do not provide explanations; indeed, it is necessary to explain why such structures developed, or why they were adopted, and this seems to be the intended theoretical relevance of this case study.

Thus, a voluntary political system lacking formal coercive authority—but entrusted with the distribution of resources received from the Jewish people in the Diaspora—inevitably had to seek an agreement between the constituent parties and settlement organizations with respect to a distributional formula for such resources. Consequently, all Zionist parties accepted—most of the time—the “authority” of the Jewish Agency Executive and the “National Council” elected by the settlers. The counterpart of this coalitional “imperative” was the need to somehow accommodate all participant political entities in order to prevent separate appeals to external factors, e.g., the British authorities or Jewish organizations in the Diaspora. Hence, the consociational structure was the result of the absence of sovereign authority. Given the intense ideological differences between the socialist labor movements, the mid-

dle-class farming and urban sectors, and the religious parties, this wall-to-wall "confederation" could not have survived without the underlying and legitimizing common nation-building goals and the fact that moderate leaders from the different sectors could cooperate on at least some of the issues. Arab violence also helped in cementing cooperation.

The book documents and analyzes the successive stages in the development of the national political center, its functions, powers and authority, as well as the role of the major "subcenters"—parties, settlement organizations and the General Federation of Labor (*Histadrut*). It then emphasizes the growth and predominance of the largest Labor party, *Mapai*. The unique role performed by the parties in a society-building as well as nation-building period is highlighted by the dual aspect of manpower mobilization: by recruiting candidates for immigration in the Diaspora and then providing for their needs after immigration to Palestine, the parties fulfilled both the Zionist task of Jewish settlement and the political task of building and enlarging their own respective constituencies. In almost every area one finds unique conditions. As the tasks became more complex, patterns of political activity were institutionalized and there was an increasing professionalization of party personnel. Yet institutions were flexible in adapting to changing conditions and within parties, relations were extremely informal. Since the pioneering ethos of a future-oriented society excluded material and status rewards, the measure of success was achievement in the nation-building enterprise. Achievement, however, was evaluated according to ascriptive, particularistic criteria: contribution to one's own party, movement, kibbutz, etc. Elite positions were interchangeable and did not carry outer signs of status, but an ideological and organizational elite developed on the basis of seniority. Much capital was invested regardless of economic considerations of profit and loss; the primary consideration was the settlement effort and the conversion of capital into political assets.

The book surveys a broad spectrum of levels and rates of development, forms of settlement, economic activity, sociocultural differences and political conflict. It also reminds the reader that no parallel processes have been observed in the nation-building experiences of other developing countries which achieved statehood in the post-World War II period. While this is true, the comparison of the *Yishuv* with these other nation-building experiences is not really legitimate. It is the post-1948 state of Israel, absorbing mass immigration from African and

Asian countries, which fits the comparison much better. The pre-statehood immigrants, and especially the pioneer settlers, had been motivated by national and social ideologies which had their philosophical, social and political roots in late nineteenth- and early twentieth-century Europe. Moreover, they were attempting to build a new society, or rather, several competing models of a new society. If comparisons can be made at all, they should be sought in some of the experiences of other nations of Western immigrants (Australia, Canada, the United States). The different historical periods involved may cause such comparisons to be of limited interest, if not meaningless; but the fact that nation building in the *Yishuv* and former colonial territories in Africa or Asia were contemporaneous processes does not make those comparisons meaningful either.

It is regrettable that Horowitz and Lissak have condensed the transition to statehood into one chapter, although they accurately identify and briefly discuss the major issues. A thorough analysis of the heritage of the *Yishuv*, its differential pace of change in institutions and in political culture, combined with new functions and new decision-making rules—but with the same leadership—deserves an entire volume by itself. It would reveal the early "decay" of the center-periphery structure through a much larger concentration of important functions at the center, beginning probably no later than Ben Gurion's 1942 decision to opt and organize for full statehood and independence, and possibly even earlier. The functional division of labor between the Jewish Agency Executive, the National Council and the expanding political role of the *Histadrut*, actually contributed to the development of a hierarchy of power, especially within the parties of the Labor sector. It was this leadership, and the party structure, which survived throughout most of the independence period. And if there was a fairly rapid "routinization of charisma," it was achieved by the very same leadership, who had now turned into "conservative revolutionaries," as Horowitz and Lissak do not fail to notice.

Although the chapter dealing with "The Ideological Dimension" is outstanding, because of the depth, breadth and structure of the analysis, the entire book is not merely the only systematic introduction to the politics of the *Yishuv* available to the English reader; it is also a very good one.

MOSHE M. CZUDNOWSKI

*Northern Illinois University*

**The Many Faces of Communism.** Edited by Morton A. Kaplan. (New York: Free Press, 1978. Pp. x + 366. \$14.95.)

This book is true to its title. It includes chapters on communism in Italy (Franco Ferrarotti), France (Jean V. Poulard), Portugal and Spain (Eusebio M. Mijal-Leon), the Soviet Union (Mikhail Agursky), Eastern Europe (Teresa Rakowska-Harmstone), Yugoslavia (Nenad Popovic), Japan (Haruhiro Fukui), and China (Tang Tsou). In addition, a chapter by Pavel Machala explores the interrelationship among superpower detente, East European communism, and West European politics, while an introductory essay by Morton Kaplan examines the impact of variety in the communist world on stability in the international system. Because of this breadth of focus, the volume will serve as a useful (and somewhat unique) resource for scholars and students alike. The individual chapters are, in almost all cases, well written and of high quality.

In addition to its breadth, the volume is marked by diversity of approach to the subject matter. Some chapters deal primarily with the triangular relationship among a communist party, its domestic context, and international communism (Ferrarotti, Poulard, Mijal-Leon, Rakowska-Harmstone, and Fukui). Other chapters are concerned primarily with the broader context of superpower relationships and international relations in general (Kaplan, Agursky, Machala, Popovic, and Tsou). Some chapters rely heavily upon documentation; others provide almost none. Some take an explicitly comparative approach; others focus on a single country. Some make an effort to appeal to specialists, offering new theories or bodies of evidence (Mijal-Leon, Agursky, Machala, Fukui, and Tsou); others gear their chapters toward students, offering comprehensive overviews of existing knowledge (Ferrarotti, Poulard, Rakowska-Harmstone). Most of the contributors are successful in what they apparently set out to do. But the diversity of approaches makes the volume as a whole difficult to evaluate—if only because the criteria for evaluation would have to be inconsistent.

Some chapters deserve special mention. Mijal-Leon's comparison of the Spanish and Portuguese communist parties is an intelligent, disciplined exercise in comparative analysis. Machala's chapter is a brilliant and subtle analysis of the dynamics of European and world politics, leaving me eager to read further contributions by this new talent in the field. Fukui's chapter on the JCP is a welcome addition to the English-language literature—a

comprehensive and first-rate history of JCP ideological and organizational revision. Tsou offers an innovative interpretation of Chinese foreign policy orientations, arguing that Sino-American rapprochement, and the official Chinese theory of "three worlds," reflect the studied efforts of an insightful Chinese leadership to regain the crucial middle position in the strategic triangle. The chapters by Ferrarotti and Poulard, while less innovative than those just mentioned, provide competent overviews.

All the chapters in this book have their strengths, but some have greater weaknesses than others. The essay by Rakowska-Harmstone provides a wealth of information for the interested student, but will be disappointing to the specialist. Its analytic framework is based on traditional stereotypes (e.g., *equating* "the priority of central planning" with "the primacy of the party" [p. 214]), and ignores the welfare-oriented and egalitarian social values these regimes typically promote (a mistake *not* repeated in Machala's chapter). Popovic's contribution is marred by lack of concern for documentation of controversial assertions (e.g., that the "thoroughly socialized" economy "creates a major prosocialist inertia, which in substance is opposed to the West" [p. 275]), and by assertions simply proposed as self-evident (e.g., "There is no doubt that the United States has necessarily to be involved in any issue anywhere in the world where human rights and democratic values and practices are at stake" [p. 276]).

Morton Kaplan's introductory essay should be of interest to students, scholars, and policy makers alike, but its provocative arguments and insights are tainted in several ways. First, they accept without question the proposition that "the Soviet regime is sitting on a powder keg," from which the author extrapolates likely Soviet foreign policy (p. 11). Second, they rest upon a concept of "finlandization" that has gained little currency among students of Soviet foreign policy (pp. 12, 13, 15, 16, 17, 26). Third, in an effort to appeal to policy makers, Kaplan too often reverts to normative, "in-group" terminology (e.g., that the orientations of a given CP are "dependable" or "unhelpful" [p. 17]), or advances propositions as if they were self-evident truths (pp. 24, 25).

Mikhail Agursky's chapter evokes both excitement and skepticism. In some ways it is a highly creative analysis of the history of Soviet ideology in terms of a struggle between Marxist-Leninist internationalism and "national Bolshevik" conservatism. Agursky sees the contemporary regime to be in the midst of a legitimacy crisis, "able to control its empire

only by increasing effort and cost" (p. 186). The consequence is an unstable "push-pull" relationship between expansionism and isolationism, rendering Soviet leaders both divided and ambivalent about the desirability of foreign communist movements, whose rebellious orientations might further exacerbate the domestic Soviet legitimacy crisis. The argument is dazzling—in some places highly insightful—but leaves unanswered crucial questions about whether the mainstream Soviet political elite is indeed this polarized. Agursky anticipates the objection: "The presence of at least two competing trends does not mean that all the members of the Soviet leadership have a polar identity. There is also a growing indifference toward ideology as a whole. . . . But the decisive political role always belongs to people with an active identity" (p. 178). Perhaps, but how are we to classify the Brezhnev's and the Arbatov's? Is their intermediate position less important and potentially less influential than the positions of extremists to their far right and far left? This issue can hardly be decided by deduction, definition, or assertion.

My status as a specialist—primarily on Soviet and East European affairs—may have oversensitized me to weaknesses in some chapters rather than others. Readers will have to judge for themselves. Nonetheless, this strikes me as a valuable book—useful for specialists in some areas, useful for students in others.

GEORGE W. BRESLAUER

*University of California, Berkeley*

**Österreichische Konsensdemokratie in Theorie und Praxis: Staat, Interessenverbände, Parteien und die politische Wirklichkeit.** By Reinhold Knoll and Anton Mayer. (Wien, Köln, Graz: Verlag Hermann Böhlau Nachfolger, 1976. Pp. 198. 340 Schillings; DM 52, paper.)

This is a concise and theoretical analysis of the Austrian political system. Although Reinhold Knoll and Anton Mayer have not explicitly applied the concept of consociational democracy as postulated by Arend Lijphart and Gerhard Lehmbruch, they have in effect dealt with it to a large extent. A more explicit reference to the theoretical literature on consociational democracy would have, however, placed their findings within a broader perspective.

The concentration is upon political parties,

the official chambers, pressure groups, and the public sector of the economy. Throughout the book, Knoll and Mayer first deal with the constitutional and legal aspects of each topic. This approach provides the necessary background for evaluating how far the actual political behavior adheres to the legal norms.

As is true of the Basic Law of the Federal Republic of Germany, parties are barely mentioned in the Austrian constitution. They are, however, key instruments in the decision-making process. Knoll and Mayer have concluded that there are strong oligarchical and bureaucratic tendencies in the ÖVP and the SPÖ. Unfortunately, the FPÖ and the KPÖ receive no more than a few passing references. Moreover, many politicians are also chamber or pressure-group functionaries. All of this is true, but more documentation could have been provided by citing the works of Ludwig Reichhold, Kurt L. Shell, and Karl Blecha. When there is documentation, references are frequently made to studies dealing with similar developments in the German Federal Republic.

The section on the Chambers of Commerce, Workers and Employees, Agriculture, and Forestry, and the nonofficial pressure groups is the best part of the book. Their considerable influence has been clearly demonstrated. There is as well a stimulating discussion about the possibility of creating a Council of Chambers, or as a replacement for the *Bundesrat*. Knoll and Mayer also devote considerable attention to the *Sozialpartnerschaft* between business and labor, which has been an important mechanism for guaranteeing labor peace and substantial economic progress.

Much of the Austrian economy has been nationalized. During the period of the ÖVP-SPÖ coalitions (1945–1966), positions in the nationalized enterprises and the government bureaucracy were divided according to a party *Proporz* formula. While this is no longer quite the case, there is still considerable politicization in these areas. This, and the great influence of the parties, the chambers, and the pressure groups, have reduced the parliament to a rubber stamp of the decisions already reached by mainly extralegal entities. It would have been beneficial if the authors had also ventured some predictions about possible future developments of consociationalism in Austria.

On the whole, this is a solid work. A number of omissions and commissions, however, detract from its value. There is no index; too many quotations are too long, some even without footnote references. The bibliography is fairly good as far as the Austrian works are concerned, but it lacks FRG and American publica-

tions on Austrian politics.

JOHN DREIJMANIS

*Fitchburg State College*

**Public Goods and Public Policy: Volume 3, Comparative Political Economy and Public Policy Series.** Edited by William Loehr and Todd Sandler. (Beverly Hills, Calif.: Sage, 1978. Pp. 240. \$18.50, cloth; \$7.95, paper.)

In a volume which seems to offer "something for everyone," Roger Hanson notes that "the theory of public goods is appealing to the extent that . . . (1) there is a wide range of political variables that can be treated as public goods; (2) there is a large number of political problems that involve questions of the demand and supply of public goods; (3) the richness and complexity of political determinants, processes, and outcomes can be explained by abstract models that generate principles of behavior" (p. 73). The essays in this edited volume try to illustrate that the range of applications of public goods criteria to public policy questions may be quite wide: from tax questions to the rules and structures of the Olympic Games. They indicate that many questions (e.g., defense, use of natural resources, the effects of economic development assistance, etc.) can be viewed as problems of the demand and supply of public goods. But they do not indicate that the richness and complexity of politics can be explained by the abstract models.

Part of these difficulties could stem from one of the obvious objectives of the volume: to provide pedagogical guidance to the theory's implications for public policy. Thus, at least four essays are relatively "simplified" for teaching purposes. These successful pedagogical pieces include an exposition of the rudiments of the theory of public goods (by the editors), a fine illustration as to how catastrophe theory may be applied to public goods problems (by Elsie Knoer), and a discussion of simple techniques for the empirical estimation of demand for a public good (by Jon Cauley). Hanson's essay is an insightful critique of the research program relating to the understanding of politics via public goods theory.

As with all such essays, it is possible to quibble that some important items should have been included, given greater emphasis, etc. Indeed, I have such quibbles: why did the editors consider group size a dimension of analytic importance equal to that of divisibility and excludability in the analysis of public goods? Why did the important work on agenda

manipulation, structurally induced equilibria, and collective choice not get incorporated into Hanson's essay on understanding politics? But these are quibbles. More serious lacunae include the total absence of discussion of demand-revealing techniques and the totally inadequate exposition (for students) of the technical material in the empirical estimation article. Editorial care is important for a volume which is (even partially) pedagogical. That one of the most obvious pedagogical essays has the stylistic difficulties of Cauley's essay is most unfortunate. Much of the worthwhile substance of that essay is sure to be lost on its readership.

Although laying a pedagogical foundation for answers to Hanson's questions may be necessary, we must look elsewhere for the answers themselves. Does the remaining 60 percent of the volume indicate that the richness of politics can be explained via public goods models? Does it illustrate how a wide range of political questions are illuminated by this theory? Unfortunately, the answers are not all forthcoming in the remaining essays. First, only two are applications to "real" phenomena. DeSerpa and Happel write of the Olympics while Amacher and Tollison analyze the law-of-the-sea negotiations. And these applications are both somewhat unfortunate. Despite the authors' statement that "the 'product' of the Games is such a valuable one and the potential harm from large-scale politicization (of the games) so great. . .," one is struck by the political frivolity of the example. And the analysis is not obviously meritorious, as it is not clear that one can achieve the authors' major objective: to apply the theory of clubs to the games so as to explain their historical success. Many reasons for this come to mind, not the least of which is the difficulty in defining the real-world analogues to the two theoretical variables: membership (in the Olympic club) and quantity of the public good the (Olympic) club supplies. The second essay, using the law-of-the-sea negotiations as an example, is less technical, more successful, and yet still not very convincing. Rather than buttress their arguments using the theory of public goods, the authors merely indicate in a very informal manner how this might be done. As such, it is suggestive, but little more.

The other four essays deal rather technically and theoretically with matters of application of the theory of public goods: the effect of "mixed (i.e., not pure) public goods" on the analysis of alliances (Stephen Schaffer); methods of analysis of environmental externalities and the calculation of optimality conditions (V. K. Smith); the analysis of optimality conditions

of natural-resource exploitation and taxation to finance public goods (Morgan and Shelton); and the effect of foreign aid for developing countries on their patterns of income distribution (Thomas Crocker). Implications for public policy design are contained in the externalities article, although one wishes they had been further explicated. On the other hand, the natural resource article falls by the wayside by making much out of nothing: building a complex model by making empirically restrictive and uninteresting assumptions. This is unfortunate since it is obvious that there are many important applications of public goods theory to environmental and natural-resource questions.

The remaining two essays have difficulties of a different class. Crocker's essay is not self-contained: it uses "transaction network" and "signaling" theory without giving the reader sufficient background to comprehend (much less critically examine) the material. Shaffer's essay is unfortunate. Many other essays deal with the subjects he raises in a far less pretentious, more productive manner, and some of these have been written by the volume's editors. It is a shame that these two less successful essays cover material that is so important to political scientists. After all, the interplay between distributional questions and public policy is at the normative heart of much political science. And the real world has little to show in the way of pure public goods: almost everything is impure. Hence, the essay which deals with the analytics of such "mixed" cases is crucial to the scholar and the student. Further, this is the only essay which introduces game-theory material into the analysis; and, as again, as such it is extremely weak.

In sum, the volume provides the scholar with technical ideas which could be useful, and provides the student with a rather neat set of pedagogical essays, and a slimmer set of quality applications. As a text, it certainly should find users in public policy and political science courses. Its three strongest essays (by Hanson, Knoer, and Smith) will be very useful to all who take the time to read them. But the volume could have been far more valuable with stronger essays on mixed goods as well as on the link between public goods and distributional questions. Finally, greater care by the editors in expository matters would have helped achieve the pedagogical objectives of the volume.

JOE A. OPPENHEIMER

*University of Maryland, College Park*

**Conservative Dissidents: Dissent Within the Parliamentary Conservative Party, 1970-74.** By Philip Norton. (London: Temple Smith, 1978. Pp. 331. £10.)

For many decades the British House of Commons earned the stereotype of a legislative institution that primarily debated and then legitimized policy decisions rather than being a place in which those decisions were actually made. Philip Norton's examination of British parliamentary politics from 1970-74 calls this stereotype into some question. He argues that backbench dissent within the parliamentary Conservative party increased sharply during 1970-74, that the Conservative government at the time changed many of its policies in response to political pressures from within the House of Commons, that the incidence of government defeat during the standing-committee stage increased sharply over what it had been in the 1960s and earlier, and that the government began to experience defeats on the floor due to backbench dissidence for the first time in over 30 years.

Norton gives us an invaluable description of parliamentary politics during the period. He provides not only summary statistics as to the incidence of defeats, attempts to influence government policies, and outcomes, but also excellent descriptions of the individual issues and key personalities involved. To complete the portrait, Norton presents alternative explanations as to why behavior changed in the House of Commons during the 1970-74 period and then systematically assesses the relative merits of each explanation. He finds, for example, that the whips and constituency associations (the former more than the latter) were largely ineffective in muting backbench rebellion on the floor, even rebellion that would defeat the government, unless the government's very life was at stake. The main cause of the backbench dissidence is not to be found in any of the traditional variables as much as in the stubbornly insensitive leadership of Prime Minister Heath.

Norton does not argue that permanent change in the behavior of Conservative backbenchers or in the relations between the House of Commons and the government took place during the 1970-74 period. After all, the principal reason presented for the change (Prime Minister Heath's leadership) is peculiar to the 1970-74 years. However, Norton suggests that the very behavior of Conservative backbenchers in defeating the government established precedents and perhaps new norms



that conceivably could result in a permanent change of behavior.

The 1970-74 years were fascinating and perhaps critical ones for the British House of Commons. Norton has made a substantial contribution to the literature by giving us a careful and comprehensive study of parliamentary politics during these years.

JOHN E. SCHWARZ

*University of Arizona*

**Ethiopia: Empire in Revolution.** By Marina and David Ottaway. (New York: Africana Publishing Co., 1978. Pp. vi + 250. \$22.50, cloth; \$12.50, paper.)

It is extremely rare for political science research to be conducted in the midst of a revolution. It is even rarer for that research to be relatively objective, extensive and intensive, and quite sophisticated. For this exemplary study, we owe Marina and David Ottaway a great deal. Combining to advantage (for the second time—they did a major study of the Algerian revolution a decade ago) the skills of academia and journalism, they have illuminated recent Ethiopian politics as no one else has. More than this, although the Ottaways are not engaged explicitly in theory building, their research is so informed by contemporary revolutionary theory that their study will promote the integration of Ethiopian events into the ongoing task of building theory.

On the level of political reporting, the study covers the period from January, 1974 to the middle of 1977. It traces at some length the increasing mobilization of various sectors of the Ethiopian population, the imperial government's response to that mobilization, and the overthrow, like a punctuation mark in that process, of Haile Selassie by a military council, the *Derg*, in September, 1974.

It then traces the evolution of that council (in origin more a Soviet than a junta) from a body originally representative in character (if representative only of the military) to an increasingly elitist one, and from a group whose internal decision-making processes were apparently democratic to one dominated by a single man. The Ottaways document also the tendency of the *Derg* to become increasingly less reformist and more repressive.

One of the great strengths of the book is in its multi-layered approach to this political

history. In part, the book is a study of the *Derg* and changes in its members, policies and processes. But that change is not treated as solely self-generated, but, at least in part, as a consequence of *Derg* response to events and processes beyond its control.

The mobilization of the population which catapulted the *Derg* to power continued after its "success." The Ottaways discuss the political evolution of various social forces: the old aristocracy, largely destroyed by the revolution; student and labor groups, initially part of or sympathetic to the anti-imperial movement, and later splitting into two groups, the smaller tentatively supporting the Provisional Military Administrative Council formed by the *Derg*, the larger, the backbone of the militant opposition to the PMAC; and finally, the peasants. In addition, the Ottaways discuss extensively the role of ethnic forces—if the Eritrean movements, Somali irredentists, increasingly conscious Oromos, and less major groups, can be so aggregated. Finally, the study describes and assesses the impact of external forces, especially American, Soviet and Cuban involvement.

Given such a complex situation, it is a marvel that one emerges from the book with the sense that the complexities have been made comprehensible, the strands woven into a reasonably coherent whole. This alone would make the book valuable. It does more, however, in attempting to answer general questions arising out of revolutionary theory: Is the revolution increasingly radical? What is the role of ideology? To what extent and by what means has the revolution been institutionalized? What is the political role of the peasants in this predominantly peasant country? What factors account for the *Derg*'s increasingly repressive treatment of its opponents? To say that the book does not fully answer these questions is less an indictment than a recognition of the immense difficulty of so doing.

The book does have its flaws. The suggestion that the revolution is increasingly radical is substantiated neither by analysis nor evidence. While one of the book's strengths is its willingness to make general statements in the absence of *definitive* evidence on important questions (what else can you do during a revolution?), the absence of evidence is nevertheless also a weakness, and extremely frustrating to the reader. For example, the Ottaways suggest that the *Derg* is meaningfully linked to peasant associations, and that the radical civilian group supporting the *Derg* won substantial nationwide support. Despite these qualms, however, this is the book for students of the Ethiopian revolution, and a crucially important resource for



students of comparative revolutions or comparative communism.

ROBERT D. GREY

Grinnell College

**Soldiers and Oil: The Political Transformation of Nigeria.** Edited by Keith Panter-Brick. (Totowa, N.J.: Frank Cass, 1978. Pp. xii + 375. \$24.00.)

In the anarchy of academia one cannot hope that an anthology will be coherent, organized around one theme, with a lucid introduction and conclusion. If *Soldiers and Oil* disappoints, then, it is less the fault of the individual contributors and more due to weaknesses inherent in compilations of this kind. Compendia can succeed only if editors make their initial conceptions clear and insist that authors hold to it, or have organizational schemes agreed upon beforehand. As this is seemingly utopian, *Soldiers and Politics* suffers from the common lack of an overall plan and suggests individual themes rather than a consistent overview.

Lack of consistency is evident in title and contents. Although the volume is entitled *Soldiers and Oil*, only one of its five parts deals with soldiers and another with oil. The remaining three parts, though touching on these themes, are more concerned with relations between federal, state and local governments and constitutional change. The book covers a variety of subjects at different levels and by different methods. Some of the essays are historical and others statistical.

The political history of the military regime is dealt with in the first part. Valerie P. Bennett and A. H. M. Kirk raise the fundamental political questions, such as the growth of federal authority, the increase in the number of regions, and the potential political parties under military rule. Henry Bienen and Martin Fitton use survey and interview techniques to elucidate the relationships among politicians, civil servants and military officers, largely in the western region. This valuable discussion of the sharing of powers has bearing on countries other than Nigeria. One of the best essays in the book, however, is that of Ian Campbell, who is able to clarify most convincingly the complexities of change in the Nigerian military regime. M. J. Dent is somewhat more optimistic than Douglas Rimmer, another contributor, on the corrective character of Nigeria's military rule. Rimmer also discusses the political economy of the Nigerian oil industry and the impact of government participation. Terisa Turner adds an interesting essay on commercial capitalism

and the 1975 coup. She points to the role of the state as an important market because of its command of vast oil incomes, and as a major buyer and regulator of the economy, controlling "opportunities to profit through commerce, politics becomes dominated by struggles for positions in the state or for access to those who have influence over governmental decisions" (p. 167). Her analysis of politics as economics shares applicability to wider areas with that of Bienen and Fitton. It is admirable in its clarity and force.

The two articles in part 3 on the creation of new states and revenue sharing in Nigeria, while important to an understanding of its politics are unexciting, though the authors, Ali D. Yahaya and S. Egire Oyobare are perhaps handicapped by the nature of the themes themselves. Part 4 contains analyses of change in local government, which is interesting to both specialists and generalists. Books on developing countries often neglect local government, despite its importance as a political arena. It is pleasant, therefore, to have these useful essays by African scholars who can bring an especial intimacy to the theme. Alex E. Gboyega and Oyeleye Oyediran give "A View from Ibadan," and Abubakar Yaya Aliyu the view "As Seen from Kaduna." Both essays point to the extension of the power of the federal government under military rule, and assess the costs and benefits this has involved when contrasted to the continuing localism. They also point to ambiguities in the jurisdictions of national, state, and local government in Nigeria under military rule.

The fifth and last part is an authoritative and lucid analysis of the work of the Constitutional Drafting Committee. Written by the editor, Keith Panter-Brick, it deals with the complex issues involved in constitution making in a society as complex as that of Nigeria. Panter-Brick also contributes an introduction, but the lack of a conclusion is to be regretted. In addition, the book has a useful set of sketch maps and selected bibliography. It is indexed.

Despite its lack of organization, the volume's individual essays could be useful to readers with special interests. The essays by Bienen and Fitton, Campbell, and Turner can be more widely recommended.

EDWARD FEIT

University of Massachusetts, Amherst

**Intergroup Accommodation in Plural Societies: A Selection of Conference Papers with Special Reference to the Republic of South Africa.** Edited by Nic Rhoadie. (New York: St. Martin's Press, 1978. Pp. xxx + 482. \$22.50.)

This is a book that should be read in context, with one's eyes wide open. If read *in vacuo* it will be at best confusing, at worst downright misleading. In 1977 in Cape Town, South Africa, a conference of 55 participants was held under the auspices of the Institute for Plural Societies of the University of Pretoria, the editor's home institution. Twenty-eight of the papers presented at the conference are collected in this volume. The conference, officially opened by the South African Minister of National Education, included a number of academics who have been active in South African public affairs. The conferees from abroad, among them such reputable scholars as the late Talcott Parsons, Walker Connor and Arend Lijphart, drew on experiences in such plural societies as Britain, the U.S., the Netherlands and Israel.

None of these circumstances by itself need undermine the scholarly value of a post-conference collection. But in fact this volume does appear to have suffered badly from the conditions which generated it in the first place. Taken as a whole, the perspective is profoundly apolitical and ahistorical.

While only about half of the relatively brief articles deal with South Africa directly, all to some extent seem informed by the authors' consciousness of presenting their papers in Cape Town in a critical period in South African history. Each in its own way implies that by understanding the accommodating formulas adopted elsewhere, sometimes successfully, sometimes glaringly unsuccessfully (e.g., Lebanon), one can not only evaluate steps taken today in South Africa but also predict the effects of alternative future reforms. That is, the book is implicitly prescriptive. But what undermines its value is that the parameters of prescriptions offered are so circumscribed and the mode of analysis so legalistic and static. For example, the state is scarcely taken into account at all. Several of the contributors do mention government policies—in Pakistan, Uganda, Syria—but without ever explicitly questioning whether relations between ethnic groups can be understood simply at the level of "intergroup" relations. The very title of the book is suggestive of the narrowness of analysis here. To analyze relations between Afrikaans, English-speaking whites, Zulus, and others in

South Africa in the 1970s without deliberate exploration of the state and its own ethnic base is rather absurd. A useful analysis of South African intergroup relations, and also those in other multiethnic societies, must directly address the role of the state and the changing capacities and bases of that state over time.

A second blind spot in the collection concerns socioeconomic class. It is not a matter of choosing between class and ethnicity in one's investigations of the sources of antagonism between sectors of a society. Rather, it is necessary to ask how class and ethnicity interact historically so as to either exacerbate or blur stratifications of power and status. Class is pertinent to analyzing intra-ethnic group relations as well. The ethnic groups touched on in most of these articles generally are treated as solid aggregations. Yet Lijphart's own concept of consociational democracy—an openly prescriptive concept—rests on the condition of sufficient intra-communal trust and allegiance within the several accommodating groups that elite members of each can be assured that they will have rank-and-file support for whatever compromises they reach. Lawrence Schlemmer, who has done valuable work on South African social change, does note that it is naive to presume that the Afrikaner whites are a united community; but he doesn't tell us any more. Without an awareness of the growing class divisions among Afrikaners themselves over the last 50 years (partly owing to the access of some more than others to the state's apparatus), it becomes impossible to comprehend the political implications of the recent "Muldergate" scandal.

The third sin of omission running throughout these articles concerns the international political economy. Once again, the South African context, of which each writer appears conscious, makes this neglect particularly puzzling. Relations among ethnic groups—and the likelihood of any policies producing genuine harmony among them—are not always and everywhere shaped by the international political economy. Yet it is a critical variable to monitor. In the current phase of world political evolution it is probably wise to start with the assumption that forces outside the given society—other states, multinational corporations, multilateral lending agencies—will indeed affect which groups have disproportionately greater access to capital and coercive force and how free they will be to use those to determine the cultural division of labor in their own society. That assumption, naturally, must be tested; but at least it should be on the initial research agenda. In the collection under review those

factors hardly appear, except in Peter Janke's article, where he raises the specter of Soviet intervention in southern Africa. This omission is striking because quite a few of the contributors, according to the useful notes accompanying the articles, are or have been affiliated with organizations such as the Georgetown University Center for Strategic and International Studies. Perhaps, however, it was the conference organizers' very decision to choose so many of the participants from such security-oriented institutions that in the end produced a set of articles that makes altering inter-ethnic relations seem akin to moving pawns (and knights and kings) around on a one-dimensional chessboard.

CYNTHIA H. ENLOE

Clark University

**The Fragmentary Class Structure.** By K. Roberts, F. G. Cook, S. C. Clark, and Elizabeth Semeonoff. (Atlantic Highlands, N.J.: Humanities Press, 1978. Pp. viii + 200. \$12.50, cloth; \$6.25, paper.)

Sociologists seemingly have an insatiable appetite for studies of social class and inequality. This brief book helps whet that appetite.

The central theme of this volume is that the British class structure is changing—in fact, fragmenting. Rather than assuming the existence of a monolithic middle class, the authors argue, we need to differentiate among its various strata. The *new* middle class is the growing sector of salaried employees resulting from Britain's movement toward a post-industrial society. The *traditional* middle class is comprised of shopkeepers and the self-employed. The *proletarian* middle class is the unionized segment who identify with the working class. The *middle mass* is the modal category located in the center of the social hierarchy. Similarly, analyses of working-class voters draw distinctions among substrata of this group: proletarian, bourgeois, central working class, and so on.

Certainly many of these ideas are not new, but this work is thought-provoking in presenting such a range of subgroupings. Moreover, virtually every chapter provides a useful review of the relevant research by British sociologists. But alas, this strength of the volume in describing subgroupings is also a source of weakness. The authors fail to provide a sufficiently coherent typology of class which incorporates (and justifies) the various types. Subgroups are presented in one chapter, and completely new

groups might be taken up in the subsequent chapter. The reader might justifiably ask, for example, how the new middle class of chapter 6 is related to the middle-class groupings of chapter 8. There is no clear answer. The same problem hampers their discussion of working-class subgroupings. To some extent this situation reflects the theoretical focus of each chapter, but the need for final synthesis is obvious.

The empirical base of this work is a 1972 survey of 474 economically active males living in Liverpool. As such, the study continues a long tradition of local surveys in British social research. While some may question the validity of such a regional sample, this criticism does not fully apply. Even a local sample can provide fertile ground for exploratory work on class imagery and class substrata. On these points, the authors' insights and analyses are valuable. However, despite disclaimers in the introduction, they also attempt to generalize from a regional cross-section sample to comment on national longitudinal trends. Their ideas on the implications of a fragmented class structure are interesting, but largely based on speculation rather than empirical evidence. If these exploratory analyses were directed to the extensive time series of British election studies, the results and conclusions might be more impressive.

Because this is an empirical study, its methodology also deserves special scrutiny. This, I fear, is the weakest part of the volume. The authors too easily dismiss the need for significance tests and measures of association—especially important with their small sample size. My rudimentary checks find them occasionally making much ado about a statistical nothing. Multivariate methods were never applied, even when the research question required them. Furthermore, indicators of concepts such as deference and class imagery were presented without validation. Even their definition of class groupings left the reader with a sense of ad hoc decision making. This research claims a scientific label, but needs more scientific rigor.

In summary, this book contains fresh ideas on old questions that many of us face. The last chapters, especially "The New Radicals" and "Politics in the Fragmentary Class Structure" are often stimulating reading. The merit of this volume is thus in convincing researchers that those crude categories of "middle class" and "working class" are insufficient for studying contemporary politics in advanced industrial societies.

RUSSELL J. DALTON

Florida State University

**The Soviet Procuracy and the Supervision of Administration.** By Gordon B. Smith. (Alphen aan den Rijn, The Netherlands: Sijthoff and Noordhoff, 1978. Pp. xii + 154. \$25.00.)

This is an important book on an aspect of Soviet law not recently given the attention it deserves. Gordon Smith, who did much of his research in the Soviet Union, was able to make use of judicial statistics and archival information not generally available. Moreover, he frequently cites conversations with Soviet lawyers, and he has obviously made good use of these conversations in making judgments as to the reliability of data and impressions gained from other sources. The bulk of Smith's data is from published sources, however, and the most important of these are 433 cases concerning the general supervision function of the Procuracy, published in the journal *Sotsialisticheskaja Zakonnost'* between 1955 and 1974. The analysis of these cases is the central focus of chapter 5 on general supervision, the key chapter in the book.

Smith considers the Procuracy the most important organ among those performing control functions in the Soviet Union, and surely this is the case. His emphasis on procuracy control, however, leads him to consider other forms of control, to the point where the title of the volume is somewhat misleading. There is an interesting chapter on court control of administration and one on citizen complaint procedure, both of which make some reference to the procuracy but have little to do with supervision. When these two chapters are taken together with the theoretical chapter 6, it can be said that something over one-third of the book has little or nothing to do with procuracy supervision.

This last-mentioned chapter, incidentally, is the least successful in the book. It contains numerous references to writings on organization theory by Western authors, but the whole discussion has at best a tenuous relationship to the data and analysis that precede it. It bears the mark of a theoretical discussion required of the author by a dissertation committee. There is other evidence of the same problem: parts of the discussion are cast in terms of hypotheses and their confirmation. This implies a level of rigor that the analysis simply has not achieved, partly because the nature of most of the data simply cannot support it. And on a couple of occasions the author goes to the trouble of calculating the statistic Tau-C. The correlations achieved in these cases are quite high, to be sure, but nothing meaningful is gained by the

use of the statistic. Smith's work stands quite well on its own, without the use of these flourishes, which, for me at least, only detract from the impact of his analysis. Aside from the social science rhetoric, the author's writing is quite clear. There are times, however, when more explanation would have been helpful. Smith writes several times, for example, of "standing," as if some doctrine analogous to the American law of standing existed in Soviet law. Such is clearly not the case.

Smith's short concluding chapter is interesting and contains several small surprises. First, he demonstrates in the procuracy's work in recent years a "clear trend toward numerical diminution of protests expressing citizens' grievances . . . and the marked increase of representations seeking to preserve state economic interests." Further, he discusses the "primacy of the control function" (mostly involving state economic violations) over the "redress function" (largely involving citizen matters) in procuratorial supervision. This leads him to note that occasional procuratorial actions "protect state interests at the cost of individual interests." Along with a number of Soviet scholars, Smith believes that this makes the expansion of judicial review of administrative acts affecting citizens all the more important.

DONALD D. BARRY

*Lehigh University*

**Israel: Pluralism and Conflict.** By Sammy Smoocha. (Berkeley: University of California Press, 1978. Pp. xviii + 462. \$24.00.)

As Leo Kuper points out in his effective foreword to this excellent book, "The theory of plural societies and of divisive pluralism has generated much controversy in the brief period of its development" (p. xiii). Inspired by the work of J. S. Furnival, M. G. Smith provided the first important contemporary theoretical formulations, and Leo Kuper, Pierre van den Berghe, R. A. Schermerhorn, and now Sammy Smoocha (among others) have further refined and developed variations of the theory. Smoocha's work is an excellent illustration of the application of the pluralist model (not to be confused with the tradition of the same name in American political science) to an analysis of the highly complex web of intergroup relations in Israel. It is one of the best illustrations of the considerable strengths and the few disadvan-

tages of this approach which I have read. If the reader is looking for a single sophisticated macro-sociological analysis of ethnic relations in Israel, I strongly recommend this book.

Smootha distinguishes five major plural divisions in Israeli society: the Palestinian Arabs in the occupied territories and Israeli Jews; Israeli Arabs and Jews; Druze, Christian and Muslim Arabs; Oriental and Ashkenazi Jews; and religious and non-religious Jews. From this wide range of various levels of social, cultural, economic, and political divisions he selects the divisions between Israeli Arabs and Jews, between Oriental and Ashkenazi Jews, and between religious and non-religious Jews, for close analysis. Using a typology of plural divisions, Smootha applies different models of plural relations to each of these sets. He analyzes Oriental-Ashkenazi relations in terms of a "dynamic paternalism-cooptation model," religious-non-religious relations in terms of a "contested accommodation model" (adapted from Lijphart's theory of consociationalism), and Arab-Jewish relations in terms of an "exclusionary domination model."

Smootha rejects the nation-building approach which has long dominated much of the sociological research on Israeli society. This approach views Israel as an exemplary melting pot (if not a pressure cooker) which has successfully "absorbed" the socially and culturally heterogeneous masses of immigrants and which is characterized by harmonious group relations. Among the devastating criticisms which he levels against the absorption-modernizing model are: it is ahistorical; it accepts official ideology at face value; it tends to neglect power factors; it tends to ignore the phenomenon of discrimination; and it tends to analyze Oriental-Ashkenazi relations in isolation from the other important plural divisions in the society. He is not the first to raise such objections to this approach, and unfortunately Smootha fails to give adequate treatment to alternative approaches which have been employed. He does reject the radical-left image of Israel as a white-settler, neo-colonial, racist, theocratic entity. But given the highly polemical nature of this approach, it is more of a "straw man" than a serious intellectual contender, although it does not lack vehement proponents.

Among the serious weaknesses of Smootha's formulation of the pluralist analysis of ethnicity in Israel is his rejection of what he terms "wasteful ethnographic and historicist approaches" (p. 13). Although in his own critical evaluation of pluralist theory, he makes modest claims for it, the previously mentioned rejection of the relevance of a rich and extensive

anthropological literature on ethnic relations in Israel is anything but modest. Both in his critical evaluation of alternative models, and in his substantive analyses, e.g., of pluralism and inequality, Smootha justly criticizes the inadequacy of data gathered through social surveys and opinion polls, but either ignores or fails to consider adequately the wealth of anthropological studies which have been carried out in Israel. While it is true that many of these ethnographic studies fail to make macro-sociological generalizations, they could and should have been more effectively incorporated into Smootha's models and analyses.

Smootha is at his best in his application of his dynamic-pluralism-cooptation model to the analysis of Oriental-Ashkenazi relations. He shows how the Orientals were gradually co-opted into the Ashkenazi-dominated system from the bottom upwards. While professing the ideals of the "ingathering and merging of exiles," the Ashkenazim looked down on the Orientals as a "generation of the desert." This unofficial paternalistic ideology has at first restricted full equality to a select few, and has delayed the acquisition of equality on a wide scale to later generations. Smootha traces the ongoing transition to a liberal competitive system where the Orientals can freely enter into competition with fellow Ashkenazim, but tend to lose out because of inferior skills and informal discrimination. This transformation marks continuing processes of weakening of ethnicity, heightening of class and power inequality, and overall improvement of Oriental-Ashkenazi relations. Ethnic calm is thus maintained by diverse mechanisms ranging from activation of Jewish solidarity and national consensus to neutralization and control.

It is impossible in such a brief review to do justice to the rich array of data and analysis contained in Smootha's impressive book. The 86 pages of tables in the appendix alone are a virtual gold mine of valuable information. Nor do I feel justified in engaging in the type of detailed scholarly criticism which would interest only specialists. In sum, this book would have considerable appeal for scholars interested in theories of social and cultural pluralism and their application to the explanation of the complexities of intergroup relations in the social scientist's ideal laboratory—Israel. It is the best and most up-to-date macro-sociological study of ethnicity in Israel, and should be essential reading for those interested in the political ramifications of such relations in Israel. It is not "light" reading, and is therefore more appropriate for upper-division undergraduate or graduate courses on the politics of

ethnicity, of Israel, or of the Middle East.

MYRON J. ARONOFF

*Livingston College, Rutgers University*

**Law and Politics: The House of Lords as a Judicial Body, 1800–1976.** By Robert Stevens. (Chapel Hill: University of North Carolina Press, 1978. Pp. xviii + 701. \$30.00.)

This exhaustive study of the British House of Lords contains reference to or analysis of close to 1500 legal decisions as well as commentary upon the historical and political context within which the decisions were made. It further contains a vast amount of biographical data about the lords who rendered decisions over the past 175 years, humanizing an otherwise alien judicial system. In a prolonged and painstakingly detailed account of how the original legislator in the British system developed its responsibilities as a final appeals court, even as it lost many of its legislative functions, Robert Stevens, a legal specialist, establishes beyond peradventure both his stamina and capaciousness.

In the introduction Stevens sets the stage by observing that while in 1800 the House of Lords was an equal partner, if not the senior partner, with the monarchy and the House of Commons in the governance of England, today it has a marginal effect on legislation, at best; it is a physical home for the salaried judges who form the final court of appeal, and has little of the influence it had when its constituency was the aristocracy. The forward march of democracy and utilitarianism challenged at an early date the assumptions about the inherent right of the peers to act as the final court of appeal.

Part 1 deals with the manner in which law and politics interacted to render a professionalized legal system by 1912, even though Lords remained under the influence of political Lord Chancellors, and was staffed by Lords of Appeal who were generally chosen for their political rather than professional success. The lower and intermediate appellate courts, on the other hand, were dominated by a professionalized judiciary handling doctrines that were increasingly regarded as self-contained and apolitical. While concern over the objectivity of the legal doctrines applied was voiced, as was the question whether they were derived from the logic of the common law, those who ran the House of Lords and Privy Council were aware that the appeals that reached them were not dealing with core situations but with policy

issues fundamental to the outcomes reached.

Part 2 treats with the reversed situation in the subsequent phase, with enactment of the Parliament Act of 1911, Haldane's chancellorship of 1912, and the Appellate Jurisdiction Act of 1913, and extending up to the onset of the Second World War. In this phase, "the law" with its emphasis upon objectivity and logic came to be more highly regarded; judges were to emphasize the more "scientific" spirit of the English tradition. While, therefore, legislation was used increasingly as a means of nationwide social control, law—as understood by the English judges—was confined within narrower intellectual and practical bounds.

We learn, in part 3, of what Stevens calls an era of substantive formalism, ranging between the years 1940 and 1955, characterized by an urge for certainty, a stress upon literalism and consistency. The implicit politics of the period was clearly one of total deference to the executive, with the value of second appeal undergoing serious attack. Legal rationality became an end in itself; the literal meaning of words was to be the only criterion of statutory interpretation. Lords, in brief, eschewed every opportunity to legislate judicially; parliamentary sovereignty was construed to mean that there should be no serious questioning of acts of Parliament or even decisions of the civil service, which arm was committed to a judge-like insistence that it was uninvolved in policy making, and might well have been the strongest force in government. Part 4 of the book spans the period between 1956 and 1976, in which years Great Britain as a world power saw decline, experienced economic truncation, and endured serious criticism of the monarchy, even as the peerage was reduced to a novelty. The law in this phase underwent demystification and the simplistic belief in substantive formalism gave way to a willingness to admit a limited interstitial legislative role in Lords, even if there has been no clear reason. Its readiness to address the rationale of the appellate process has made more probable the survival of the House of Lords, at any event.

The view that final courts of appeal are inevitably creative in their function, taken by Holmes and Cardozo, among others, in the American system, is scarcely accepted in England today. A literature abounds in America that even threatens to render extinct the doctrinal basis for adjudication, as do quantitative judicial studies, but little of this nature is to be found in the United Kingdom. Debates about judicial activism and judicial restraint, commonplace in the United States, fall short of even possessing the consciousness in England,

given the strong taboos about closely examining the work of judges, and the generally accepted view that law and the courts play a less vital political and social role there. There is considerable validity to the view that the British legal system has changed little since the devastating nineteenth-century portrayals of marathon cases in Dickens' *Bleak House* and Trollope's *Orley Farm*. It is restrictive and enamored of form and ritual; the barrister-solicitor distinction promotes impersonality; it is oligarchic. These are but some of the observations provided by the author and they probably help to understand why a royal commission is now conducting inquiry in the direction of improved legal services.

Robert Stevens' *Law and Politics* is an impressive and vast undertaking and study of the House of Lords as a judicial body. Stevens has consulted all of the reported decisions of the House of Lords and even the unreported decisions since 1830. Documents by, to, and about the law lords have been consulted as well, along with their speeches, and the enrichment provided by Stevens' extended political and psychological commentary makes this quite evident. The future of the House of Lords as a judicial body will lie in an acceptance of the element of discretion in the final appeal process, Stevens concludes, coupled with a responsibility to articulate the purposes and goals of different branches of the law. While the courts in Britain will probably never compete with the legislature, inconsistent with their political and intellectual history as this is, some complementarity and intelligent creativity can be hoped for. For the present, at any event, Lords as a judicial body is no longer living under threat of imminent demise and the idea that appeal judges are engaged in some form of lawmaking is widely accepted politically and intellectually.

HILLIARD A. GARDINER

Queens College,  
City University of New York

**The Politics of Terrorism.** Edited by Michael Stohl. (New York: Marcel Dekker, 1979. Pp. xiii + 419. \$10.00, paper.)

The prospective editor of a collection of essays on political terrorism has ample reason to feel intimidated. Trying to exert some centripetal force over any group of articles, all naturally flying off in different directions, is problem enough. To make it worse, terrorism has proved a difficult subject for academic inquiry. There is little agreement on what

terrorism is, much less on an explanation of causes, processes, or effects. The vagueness of the concept means that the centrifugal pull in such a collection is almost irresistibly strong. Furthermore, the controversiality of the issue, while it inspires a profusion of books, articles, reports, and conferences, often leads to emotionalism.

Despite these obstacles, this collection of analytical essays edited by Michael Stohl is a useful addition to the literature on terrorism. The articles, most of which were written for this volume by scholars new to the field of terrorism, are published here for the first time. The first section of the book contains four essays on theoretical approaches to the study of terrorism. Ted Robert Gurr presents an empirical survey of domestic terrorist campaigns in 87 countries from 1961 to 1970. Peter Grabosky considers urbanization as facilitating the resort to terrorism. Harry Targ employs a Marxist framework to argue that social structure is a major determinant of terrorism, which he finds more likely in pre-industrial and post-industrial societies than in industrial ones. Peter R. Knauss and Donald A. Strickland investigate the relationship between terrorism and the fear of anarchy, concluding that society is enormously resilient to disruption. The more extensive second part of the book, "The Practice of Political Terrorism," focuses on various geographical regions: Western Europe (Raymond R. Corrado), sub-Saharan Africa (Mary B. Welfling), Latin America (John W. Sloan), the Palestinians (Vaughn F. Bishop), Bengal (Richard C. Hula), the United States (Frederic P. Homer), and an overview of the transnational dimensions of the phenomenon (Edward Mickolus). The division of subjects is conventional, but it is hard to understand why Gurr's description is classified as a theoretical approach, while the similar survey by Mickolus of transnational terrorism world-wide from 1968 to 1975 is placed in the historical section.

Some of the individual articles are informative and provocative; the analyses by Gurr, Grabosky, Targ, Welfling, and Hula are worth singling out. However, the volume suffers from insufficient coordination. Had the essays on historical incidents of terrorism tested or at least taken into account the hypotheses proposed in the theoretical chapters, the volume's claim to theory-building would be more credible. With one exception—Welfling's comments on the relevance of Gurr's conclusions to the case of Africa—each chapter stands alone. Some of the historical essays present surveys of entire regions; other focus narrowly on single terrorist organizations. Since the theoretical virtues of

the book are limited by the autonomy of the contributions, an introduction outlining the common themes of the essays instead of demolishing conventional wisdom about terrorism might have supplied the necessary unity. The readability of the volume would also have been improved by uniform citation styles and care in avoiding typographical and factual errors.

One distinctive characteristic of the collection is that it employs a concept of terrorism that includes actions both by and against the state, although only half the essays attempt to analyze both forms. The impartial use of the term "terrorism" to apply to governments as well as opponents is welcome, especially since many works on terrorism use the term pejoratively. It is also important to emphasize that governments as well as challengers may be the instigators of violence. While defining the concept broadly may be philosophically and politically desirable, in application such inclusiveness can result in imprecision and the sacrifice of depth for breadth. It is also wise to remember that government repression is not necessarily terrorism. Furthermore, the revolution-repression dichotomy can obscure the fact that revolutionaries can use repressive terrorism against their own followers.

The essays raise intriguing questions precisely because of their eclecticism. If, as Targ argues, terrorism is not likely in industrial societies, how can urbanization facilitate terrorism, as Grabosky contends? Although his data are limited, Gurr suggests that terrorism is more frequently a means of protest than of revolution, but both Targ and Welfling agree that terrorism occurs when other methods of seizing power (e.g., mass revolution or coup d'état) are unavailable. They assume, as does Stohl in the introduction, that the purposes of terrorism are either to maintain a regime or create a new one. Moreover, if the absence of other means is a source of terrorism, how does one account for the high incidence of both coups and terrorism in Latin America? Another dilemma lies in the circularity of some explanations of terrorism. In general, confusion between factors that are causes of terrorism and those that are effects seems endemic to the study of terrorism. For instance, lack of active popular support for would-be revolutionaries is considered by many, including Targ, Sloan, and Welfling, to be a condition for terrorism. At the same time, the effectiveness of terrorism as a political strategy is judged by its users' ability to gain mass support. Corrado argues that both the Provisional Irish Republican Army and the Rote Armee Fraktion in West Germany are anachronistic, because they failed to gain mass

followings. It is not clear whether he means that these groups resort to terrorism because they are traditionalists opposed to modernization, or whether they are anachronistic because terrorism is inappropriate. The danger lies in inferring cause from effect.

The serious theoretical intentions of this volume distinguish it from the bulk of the literature on terrorism, much of which is policy-oriented or loosely descriptive. The book is supposed to be valuable for specialists, educated non-professionals, and undergraduate and graduate students alike. However, its complexity does not make for easy reading, and the lack of a bibliography also decreases its usefulness as a text for students.

MARTHA CRENSHAW

*Wesleyan University*

**Revolution from Above: Military Bureaucrats and Development in Japan, Turkey, Egypt, and Peru.** By Ellen Kay Trimberger. (New Brunswick, N.J.: Transaction Books, 1978. Pp. viii + 196. \$14.95.)

This interesting book attempts to develop a model of revolution from above based on an analysis of the Turkish (1923) and Japanese (1868) cases, and a brief comparative review of more recent developments in Egypt and Peru. The model is "distinct from either coup d'état or mass-bourgeois or socialist-revolution from below" (p. vii). Beginning with the assumption that revolutions from above are exceptional and unusual, Trimberger concludes from her study that the preconditions for "this type of social change are becoming increasingly prevalent in many Third World countries," and that it is mass revolutions from below that "are the truly exceptional or rare historical phenomena" (pp. vii-viii).

Stated succinctly, Trimberger's thesis is that revolution from above occurs in situations in which there is "a relatively autonomous bureaucratic state apparatus" and "a dynamically autonomous state bureaucracy." In short, control of the state apparatus is divorced from control of the economic system, or the means of production and exchange. The bureaucrats, in other words, are not members of the dominant class; nor does the dominant class necessarily control the institutions of the state. In fact, in revolutions from above, bureaucrats use their control of the state apparatus to turn



against the dominant class and destroy the status quo which nurtures that class. This is most likely to occur "when there is no consolidated landed class, as in nineteenth-century Japan and Turkey, or when a landed oligarchy is in economic and political decline," and when the nascent bourgeoisie either is weak or dependent on foreign interests (p. 5). Military bureaucrats are especially well situated to undertake revolution from above because they have direct control of the coercive instruments of the state.

In the further development of her thesis, Trimberger elaborates five "necessary and sufficient" conditions for revolution from above by military bureaucrats. The first has already been stated: the autonomy of the officers from the dominant socioeconomic class. Second, the officers must develop political cohesion. Third, they must perceive a threat to their own interests in the form of penetration by foreign interests; by contrast, "military bureaucrats with ties to vested interests react to nationalist movements from below with conservative repression" (p. 42). Fourth, the international situation must be such that the military bureaucrats are capable of exploiting it for purposes of increasing national autonomy. Finally, rebellious military bureaucrats require a provincial power base from which to mount their campaign against the central government. All of these conditions are found to have prevailed in nineteenth- and early twentieth-century Japan and Turkey. In China and Prussia, by contrast, the bureaucrats were too closely tied to dominant economic interests, leading to a conservative reaction against nationalist movements rather than the forging of an alliance with them. Analysis of the Nasserite movement in Egypt and the 1968 upheaval in Peru leads Trimberger to drop the fifth of the above conditions on grounds that the prior existence of a unitary and homogeneous nation-state (in contrast to the relatively decentralized Japanese and Ottoman Turkish regimes) obviates the need for a provincial power base and a civil war against the *ancien regime*.

Given this pessimistic conclusion, one wonders about both the applicability and the utility of the model set forth here. If revolutions from above fail to break the stranglehold of international capitalism, are they to be preferred to any other alternative? For that matter, is any significant improvement in social conditions in Third World countries possible without a major upheaval in the prevailing international political and economic structure? Unfortunately, larger questions such as these are not explored in this book, save for suggestions in the last two pages

that only relatively populous Third World countries with large potential internal markets and "strong and independent mass socialist or communist" movements capable of mobilizing the masses for the "prodigious human effort necessary to achieve autonomous industrialization" can in the long run succeed.

It must also be noted that this laudable effort is marred by the publisher's allowing far too many glaring errors to slip through the production process, including in particular a crucial garbled sentence at the bottom of p. 5. The book might also have benefited had more recent literature been included (very few items published after 1973 are included in the bibliography).

In sum, this is an interesting essay, although in important respects it remains a truncated effort.

FRANK TACHAU

*University of Illinois, Chicago*

**Social Deviance in Eastern Europe.** Edited by Ivan Volgyes. (Boulder, Colo.: Westview Press, 1978. Pp. xv + 198. \$16.00.)

This collection includes eight separate articles, each with its own understanding of "social deviance," and each with unique questions and approaches. Not only is the book *not* integrated, not even cosmetically by an introduction or conclusion, but also the authors individually make little attempt at comparing patterns of deviance and control among East European countries or between them and other countries. When an occasional cross-national difference does emerge, the authors rarely attempt to explain it. All the same, many of the articles are worthwhile, and most of them will interest students of East European politics and society.

The book contains one theoretical essay (Shapiro); three characterizations of deviance in a single country—Romania (Gilberg), Yugoslavia (Klein), Czechoslovakia (Ulč); two depictions of the private sector of the economy—in Hungary (Volgyes) and in Poland (Korbonski); and two studies of particular forms of deviance—alcoholism in five countries (Kerr) and prostitution in Hungary (Volgyes and Peters).

The opening and most thought-provoking contribution is the theoretical article by Paul Shapiro. The author succeeds in demonstrating the utility for the study of deviance in Eastern Europe of some distinctions made by Robert K. Merton in his 1938 article "Social Structure and Anomie." A line of argument which

emerges from Shapiro's analysis is that the exclusive and small number of definers of what is officially deviant in most East European societies makes unquestioned legitimacy of their rule a critical precondition for social acceptance of the norms they generate. Fearing the absence of such legitimacy, political leaders in Eastern Europe tend to overcriminalize, that is, to treat most or all deviance as aberrant and little or none of it as merely nonconforming. As engaging as these hypotheses are, they are not addressed by the authors of the more empirical contributions.

The three studies of deviance in a single country differ so much in character that one cannot try to make comparisons. Trond Gilberg pictures the problem of social deviance in Romania as the result of the attempt by a puritanical modernizing leadership to impose its standards upon a premodern, rural population. Gilberg goes on to use the official ideals of political, economic, social, and cultural behavior (which he derives from Ceasescu's speeches) as the norm against which to judge the amount and significance of deviance in Romania. Whether Romanian authorities take themselves so seriously and treat low productivity, political apathy, and divorce as deviant, I am not at all sure. George Klein offers a fine account not of deviance in Yugoslavia but of the major trends in the social structure of that country which might help one to understand deviance there. Unfortunately, Klein only alludes to features of Yugoslav deviance itself, such as the absence of economic crime in the socialist sense because of a different law and the disruptive effects of the migration of workers to Germany. In his study of deviance in Czechoslovakia Otto Ulč also pays little attention to actual trends in deviance or control, offering instead a series of analytical distinctions of variable quality (deviant versus criminal; political versus ordinary criminality; normal versus pseudo-deviance). One does learn *inter alia* that since the traumatic Soviet invasion in 1968 corruption and the use of drugs have markedly increased.

The two interesting articles on the private sector of the economy in Hungary and in Poland deal more with legal than with illegal activities. Volgyes goes on to describe the recent increase in Hungary of "bakshish" culture (tips, honoraria, bribes). Andrej Korbonski shows how the presence of the largest legal private sector in Poland has in no way stunted the growth of an illegal one. Apparently, the "gap in the assortment of goods and services provided by the state" is large enough to support both.

The studies on particular crimes yield limited insights. J. L. Kerr briefly describes the varying patterns of alcohol use, abuse, and control in five countries. He concludes that the authoritarian regimes in Eastern Europe have attacked their alcoholism problems no more vigorously than Western states, partly because of a sensitivity to public opinion and partly because of the economic revenue at stake. From Ivan Volgyes and John Peters one learns that in relatively tolerant Hungary three types of prostitute have emerged, each serving a different client—workers, upper-class clients, and Italian tourists.

PETER H. SOLOMON, JR.

*University of Toronto*

**Egypt: Burdens of the Past, Options for the Future.** By John Waterbury. (Bloomington and London: Indiana University Press, 1978. Pp. vii + 313. \$19.50, cloth; \$9.95, paper.)

John Waterbury is a skilled writer and an astute observer of politics in North Africa. *Egypt: Burdens of the Past, Options for the Future* reflects both attributes. The book is a compilation of 16 American Universities Field Staff reports prepared by Waterbury between 1973 and 1977. The reports are organized around seven interrelated themes: guns and/or butter; coping with population growth and fertility control; agricultural versus industrial development; socialism or state capitalism versus private enterprise; authoritarianism versus controlled liberalism; Arabism versus Egypt-firstism; positive neutrality versus alignment with the West.

While suffering some minor repetition, the book is far more than a reader. The articles fall in time sequence, with the later articles building upon information presented in the earlier ones. Collectively they take the reader on a Cook's tour of the myriad factors that have rendered the political, social and economic development of Egypt seemingly impossible. Stops along the tour include before-and-after analyses of the October 1973 war, a trenchant analysis of the population explosion which has seen the Egyptian population grow from less than 3,000,000 at the end of the eighteenth century to a projected 60,000,000 in the year 2000, innumerable examples of bureaucratic ineptitude, equally innumerable examples of the misapplication of socialist principles, the imponderable problems of municipal government and management in Cairo, the intellectual debates surrounding Sadat's cautious program of de-Nas-

serization, the shift to a mixed public/private economy, and the multitude of complex problems relating to land-water management. Along the way the reader is depressed by the plight of Egypt's poor and yet entertained by the plight of Cairo's bus system.

The most impressive aspect of the book, in my view, is the detailed portrayal of Sadat's persistent movement toward peace with Israel and rapprochement with the United States. The key to both, as well as to the initiation of a new economic policy, in Waterbury's view, was the Egyptian success in the October War.

The crossing of the Suez Canal was a necessary although not sufficient condition for moving ahead on issues of economic liberalization, foreign investment, and political reform. It established Sadat as a national hero, and having acquired his own image and stature, independent of his association with Gamal Abdul Nasser, he could afford to advocate and implement new policy initiatives. Of equal importance was the fact that the October War restored the Egyptian army to the role of a conventional military force rather than the collection of officer-led cliques that had involved themselves so intimately in domestic politics under Nasser.

Once Sadat had established himself as his own man, according to Waterbury, it then became possible for him to approach the question of peace with Israel and rapprochement with the United States. Peace with Israel was viewed by Sadat as being the only means of restoring the viability of the Egyptian economy. Egypt had to choose between guns and butter. The country could not afford both. Rapprochement with the United States, in turn, was perceived by Sadat as a prerequisite for peace with Israel. In Sadat's view, according to Waterbury, "Only the United States, because it has consistently underwritten Israel's military superiority and economic viability, could cajole or push the Israelis toward compromise."

The political and economic logic for rapprochement with the U.S. and peace with Israel contained in Waterbury's analysis is compelling. Had the book been published a year or two earlier (when the last of the reports were published), Sadat's peace initiatives would have come as far less of a surprise.

In methodological terms, the various reports are a mixed bag. All reflect Waterbury's five years of residence in Egypt. Some rely heavily upon the Egyptian press and upon interviews with leading officials. Others, such as the analysis of Egypt's land and water resources, rely upon a wealth of detailed economic analyses and feasibility studies. Having lived in Egypt for a year, I can offer the observation that

Waterbury's descriptions and analyses ring true. In terms of a value orientation, it is clear that Waterbury likes Egypt and the Egyptians. He takes little delight in the hopelessness he describes.

MONTE PALMER

*Florida State University*

**India at the Polls: The Parliamentary Elections of 1977.** By Myron Weiner. (Washington, D.C.: American Enterprise Institute, 1978. Pp. 150. \$3.75, paper.)

The Indian parliamentary election of 1977 was an event well worth political scientists' attention. Called by Indira Gandhi at a time when her own dictatorial power seemed secure, it astonished those who follow Indian politics. Observers of political change in the developing nations must have been taken similarly by surprise. Elections terminated by dictatorships have been common enough in poor, heterogeneous ex-colonies; India demonstrated the opposite.

Myron Weiner was enterprising enough to get to India a few weeks before that dramatic event. He visited five major cities during the campaign. He talked with candidates, party workers and the newly unmuzzled newspaper reporters for whom, of course, the defeat of Mrs. Gandhi at the polls was the story of a lifetime. This information, together with the election returns, including follow-up state elections during the next year, were the raw material for this book.

The first half of the book simply tells what happened. Weiner quickly supplies the background for Mrs. Gandhi's declaration of emergency on June 26, 1975, and summarizes its oppressions and accomplishments. He joins those who have speculated as to Mrs. Gandhi's reasons for submitting her regime to elections, but without making that decision seem quite rational. Certainly, he points out, having silenced critics, she lacked feedback as to popular resentment toward the more brutal uses of government power during the emergency. Weiner makes good use of newspaper reports and campaign rhetoric to convey a sharp picture of that resentment. For example, he quotes a Muslim notable, the *imam* of Delhi's great mosque, explaining during the campaign his willingness to support militant Hindu activists against Mrs. Gandhi:

It was not the RSS or the Jana Sangh that fired on Muslims at Turkman Gate, but the government. It was not the RSS and Jana Sangh that introduced coercion into family planning, but

the government. . . . Freedom for Muslims depends not upon the election of a pro-Muslim government, but on living in a country in which the laws protect all communities.

Appendices supply the texts of party platforms and other relevant documents. Of two states whose election politics were not dominated by resentment against government excesses—Tamil Nadu and West Bengal—Weiner presents short sketches based on his observations. I must confess that his account of Tamil Nadu left me still puzzled as to the lack of reaction to the unseating by Mrs. Gandhi a year earlier of the admittedly corrupt but nevertheless popularly elected state government.

The second half of the book is an analysis of the election returns. The reader can find here a record of the party vote by states, comparisons with the last previous election (1971), and comparisons of certain categories of constituencies with the general vote by states. Weiner uses this last form of cross-tabulation to get at the question whether some groups of voters turned against Mrs. Gandhi more strongly than others. Explanations of voting behavior from aggregate data (survey research is well developed in India, but in 1977 Mrs. Gandhi surprised the pollsters as well as the rest of us) is, of course, vulnerable at best. The applications of it here are of uneven quality. Most valuable, to me, was Weiner's showing that constituencies reserved for candidates of Scheduled Castes (ex-Untouchables) not only turned against Mrs. Gandhi at least as strongly as the rest of the electorate did, but also generated a sharper increase in turnout in 1977 than did their states taken as wholes. This phenomenon was confined to the northern states where Mrs. Gandhi's son, Sanjay, bullied the bureaucracy into forcing sterilizations and bulldozing squatter hutments. Since it is now accepted that the lowest strata of society bore the brunt of these brutalities, Weiner can thus offer circumstantial evidence that the election provided a weapon of defense to those who needed it most, and they used it.

Upon other possible explanations of the vote the evidence is not so cogently marshaled. Weiner does not attempt to disentangle the two most plausible explanations for the dramatic regional disparity of the returns. Indira Gandhi's Congress won only two seats in the Hindi-speaking North; the victorious Janata won only six seats in the Dravidian-language South. Was it because, as we just saw, people in the North were abused more during the Emergency? Or because the former opposition parties which banded together to create the Janata party (Weiner helpfully provides previous elec-

tion data aggregated for these parties) had their long-standing support-bases in the North? The two explanations are almost but not quite congruent; the returns could have been manipulated to do some testing. Likewise, a closer look at returns in some heavily Muslim constituencies would have tested, at least crudely, Weiner's very plausible proposition that Muslims defected from the Congress even more massively than others.

This book is probably, therefore, not the last word in the scholarly interpretation of the meaning of the 1977 elections. Weiner himself may push analysis further, as he has done for previous elections. Meantime, it is a worthy increment to the American Enterprise Institute's very useful series of worldwide election reports. In addition, it is a clear, authoritative, and evocative lesson in the value of the ballot to those who most need it, as distinguished from those most accustomed to it.

HENRY C. HART

*University of Wisconsin, Madison*

**Pressure Politics in Contemporary Britain.** By Graham Wootton. (Lexington, Mass.: Lexington Books, 1978. Pp. xii + 256. \$19.95.)

Graham Wootton has been one of the most prolific writers on the place of pressure groups in British politics in this century. His latest book, which draws upon events from the mid-sixties to the late seventies, attempts to assay the extent to which the new position of such groups gives the British system increasingly corporatist features or leads to the condition that Samuel Beer has called pluralist stagnation. His conclusions are mixed and tentative. For him, as for most students of pressure politics, good data and ready answers are not easy to come by.

Wootton does not employ grand theory or especially intricate methods, although he does go beyond simple description. Because of the vast number of politically active groups in Britain, ranging from the lovably eccentric and bizarre ones dear to Anglophiles to the militant unions so unpopular with recent voters, the need for classification and selection is apparent if generalizations are to be soundly based. Wootton finds previous attempts at coining workable classifications unsatisfactory, and introduces his own, a fourfold typology based on the degree of groups' political specialization and the degree of openness of their membership. He proceeds to put into its cells a selection of great and small groups, whose

pressure activities he chronicles in the body of the book, which is rich in political detail and mainly narrative in form. This rather modest effort at constructing a typology grows out of good intentions; but its theoretical basis, or the improved power it provides, are far from self-evident, and the choice of the mini-cases to carry the burden of each type of group, though presumably defensible "on the basis of apparent political significance," ties the generalizations mainly to well-reported recent controversies such as the third London airport and commercial radio.

This framework does enable Wootton to gain a basis for judging the validity of the pluralist stagnation thesis, however. Even though his findings understandably are based on a small fraction of recent pressure group efforts, they are sufficient to lead him to conclude that Britain is neither gripped by a paralyzing inability to change policy in many areas nor on the brink of Mussolini-style corporatism. He shows that groups of all of his types have both succeeded and failed in the pursuit of their interests, by a growing variety of tactics. Although they induce a healthy skepticism over the position which elevates private group power to the center of the British political system, Wootton's efforts do not put to rest completely the concern of other commentators that group power has been exacerbated by failures of

structure and will in the traditional governmental center.

The strengths of this book lie in the questions it raises about trends in British interest group involvements, its focus on outcomes and effects beyond mere process matters, its up-to-date selection of material, and its combination of graphic detail and sensible assessments. It is also written with verve and wit, though occasionally with a surfeit of word play. The book does not add notably to interest group theory, nor does it purport to give a comprehensive tour (in less than 200 pages of text) of group life in British society or the range of its links with political institutions. But it does convey a strong flavor of the newer issues which pressure groups have helped put on the agenda of British decision makers in the past 15 years.

Britain remains a valuable site for exploring, from both analytic and normative perspectives, changes in the evolution of the conventional distinction between the public and the private realms; and pressure group behavior provides one key to the tougher dilemmas which have emerged. To that end, Wootton's insightful, if impressionistic, book provides a useful stock-taking of where we are, and how far we still need to go.

JAMES B. CHRISTOPH

*Indiana University*

## International Politics

**The Foreign Policies of African States.** Edited by Olajide Aluko. (London: Hodder and Stoughton, 1977; Atlantic Highlands, N.J.: Humanities Press. Pp. vi + 243. \$9.00, paper.)

Olajide Aluko of Nigeria has attempted to cover the foreign policies of a vast array of African states in this volume. In 12 chapters—three of which the editor prepared himself—scholars deal with Algeria, Egypt, Ethiopia, Ghana, Guinea, Ivory Coast, Kenya, Nigeria, Tanzania, Zambia, and Zaire. Aluko's introduction is called "The Determinants of the Foreign Policies of African States."

Aluko deserves much credit for assembling essays on a representative sample of countries, particularly when he has been able to recruit writers such as Boutros Boutros-Ghali of Egypt and Thomas Kanza of Zaire who have played important roles in their own countries' foreign

policies. A chapter on Libya or Sudan would have been welcome, but an editor has to set the limits somewhere.

All essays are clear descriptive surveys which seem to have been written in 1972 or mid-1973 with some last-minute update work here and there. In his introduction Aluko explains domestic and extra-African influences on foreign policy through the 1960s. While the categories—economic, political, colonial heritage, ideology, geography, and cold war environment—are useful, the examples are quite dated: Aluko points, for example, to the now-defunct East African Community as the most successful attempt at economic cooperation ignoring more recent efforts such as the Economic Community of West African States. And, the statement that African states "have very little to fear from each other" (p. 12) rings ironically these days. Chapters are arranged alphabetically by name of country rather than by analytical categories.

Boutros Boutros-Ghali's essay is a five-page outline dealing with the historical continuities in Egyptian policy, but there is no analysis of the country's important break with the Russians. Negussay Ayele's piece on Ethiopia is really about Haile Selassie's policy. 'Ladipo Adamolekun provides his usual sympathetic account of Sékou Touré's idiosyncratic and hypocritical foreign policy.

On the other hand, John Akumu's chapter on Kenya, Aluko's chapter on Nigeria, and Timothy Shaw's contribution on Zambia are good for their analyses of basic problems and issues, although they, too, are dated. Okumu gives a detailed explanation of Kenyan relations with Somalia and other neighbors. He explains Kenya's strategic position and presents useful (although old) data. Aluko describes changes in Nigerian foreign policy beginning in 1970 with respect to its immediate neighbors, Africa in general, and the non-alignment movement. He shows how policy makers are trying to increase economic ties with the rest of Africa and deals with the new concerns of Nigeria in southern Africa although a discussion of Angola is conspicuously absent.

Shaw's contribution is refreshing because of his brisk application of class and elite theory to Zambia. He examines the role of multinational corporations, Kenneth Kaunda's personality, and the importance of elites' efforts to protect their status. In his view, the most important determinant in Zambian policy making is the ruling classes' desire to avoid "a radical restructuring of the political economy of southern Africa" which would loosen their own internal control (p. 231). Even though one might not agree with Shaw, he supplies a provocative framework for viewing foreign policy in Africa.

Because of the absence of other similarly comprehensive texts, this book could serve as an introduction to African foreign policies in undergraduate courses on Africa or in general courses on foreign policy. The absence of a bibliography and index restrict its utility for this purpose, however.

BRIAN WEINSTEIN

*Howard University*

**Romanian Foreign Policy Since 1965: The Political and Military Limits of Autonomy.** By Aurel Braun. (New York: Praeger, 1978). Pp. xii + 217. \$18.95.)

In discussing the extraordinary course of Romanian foreign policy over the past two decades, students of the subject typically ad-

dress themselves to two questions: (1) Why have the Romanians engaged in this consistently delicate and often dangerous challenge to Soviet dominance? and (2) How have they been able to get away with it? Considerations of the subject have not always been careful to separate these two aspects, often emerging as a confusing mixture of necessary and sufficient factors. The strength of Aurel Braun's book is that it does not fall victim to such confusion. Braun addresses himself clearly to the second of these questions, and he remains true to his focus throughout.

This is, however, also a source of weakness in the volume. Because Braun sticks so loyally to his central question—a delineation of the "defenses" the Romanians have been able to use since 1965 to protect their foreign policy—he never touches the major question that fairly screams to be answered, i.e., what are the Romanians up to? What is the purpose of this policy? What goals is this policy aimed at fulfilling and/or what stimuli, internal and external, drive this policy through those twists and turns that Braun describes? These questions are not addressed. While this is not the main purpose of the book, such questions are clearly germane, even crucial, to the volume's stated subject.

Thus, the book's title is somewhat misleading; for what this book is *not* is a study of Romanian foreign policy since 1965. It is, rather, an analytic essay, carefully constructed, well organized and amply documented (though rather thin on previous Western works on the subject). Braun examines the "active defenses" of Romania's autonomous foreign policy, i.e., those skillfully wielded by Bucharest, and the "passive defenses," those inherent in Soviet policy which presumably have allowed a degree of nonconformity to emerge or even flourish. The reader is best advised to come to this work already well informed on the specifics of Romanian foreign policy, pre- and post-1965 (especially the former), in order to be able to learn from it. Except for an occasional reference, for example, Braun does not discuss the Gheorghiu-Dej period. This leaves some rather striking lacunae, such as the almost complete absence of discussion of the 1964 Romanian Workers' Party Statement, a bedrock document in the development of Romanian foreign policy.

Regarding the period under study, Braun does make several interesting, if somewhat narrowly drawn, points: for example, his discussion of the Soviet view of international law and its relationship to relations among socialist states, and his attempt to sketch out how

various Soviet military leaders might view non-conformity in East Europe. But in both these cases and several others, Braun either fails to demonstrate the impact of such factors or fails to gauge that impact accurately. His consideration of the role of China, for example, overstates the economic impact of that state on the Romanian situation, understates its political aspects (in contrast see, for example, Jacques Levesque, *Le Conflit sino-sovietique et l'Europe de l'Est*, l'Université de Montreal, 1970) and offers no conclusive assessment at all as to its military aspects. A more serious failing lies in the section dealing with Romania's "political/ideological defenses." Here very little attention is paid to the Romanians' decade-long attempt to redefine themselves as a socialist developing country and to seek support from nonaligned and Third-World countries—as well as increased aid from developed states—on that basis.

The book's best chapter deals with the Romanian capacity to offer "active military defense of foreign policy autonomy," i.e., to deter a possible Soviet intervention and defend the country should such an attack take place. Braun does not confine himself to a mere weapons-and-forces calculation, but explores also questions of preparation against surprise, overall morale, party control and the use of irregular forces.

The book is written in a somewhat self-conscious tone and is plagued by several disturbing contradictions: e.g., Soviet foreign policy is described as "not *per se* interventionist" (p. 190) but also as "paranoid about controlling the bloc and ensuring strict cohesion" (p. 195); periods of Soviet military maneuvers are labeled merely "intimidation" at one point but as "crisis years" further on.

Finally, while anyone studying Romanian foreign policy is bound to be impressed by the skill with which the various situations and issues have been handled by Bucharest, to view the evolution of this policy over 20 years as "a very complex set of defenses that had to be coordinated carefully in order to provide an effective defense network that would have allowed the country to pursue its autonomous foreign policy" (p. 200) makes too many of this mistakes associated with rational-actor models and makes the greater error of mistaking for an end in itself a foreign policy which is in fact a means to an end, an end which in this work remains unexplored.

RONALD H. LINDEN

University of Pittsburgh

**Secession: The Legitimacy of Self-Determination.** By Lee C. Buchheit. (New Haven and London: Yale University Press, 1978. Pp. xi + 260. \$17.50.)

The overlapping claims of domestic and international law were never clearer than in the case of secession by a minority from a legally constituted state. The contest is highly charged as both protagonists invoke morality, international law and broken promises and usually involve outside states and international organizations in their dispute. The problem of secession by a group of people who occupy a particular territory in a legally constituted state, can be considered in several ways, none of which provides complete and satisfactory answers. It can be examined as a domestic national issue, as an international question, or as both. Lee Buchheit touches upon all three aspects of the problem but chooses to focus upon "the implications for the international community of appeals to the principle of self-determination by secessionist groups within independent states." It is within this context that the following remarks are made.

At the heart of the problem of secession is the doctrine of self-determination. As was noted by scholars during the early post-World War I period—when the old order of multinational states and empires was being replaced by new national states—it was not self-evident either as to how these states could be constructed so that they included only all, or nearly all, the "national" political unit which seeks to establish a state of its own, or what to do with minorities who are physically trapped within these new states. If the doctrine of self-determination were carried to its extreme, there would be no group too small or territory too insignificant to be denied the benefits it implied.

After World War II, the victorious states, through their promulgation of the United Nations' Charter, committed themselves to—among other things—the end of colonialism. In the first article of that document they declared as one of their purposes to "develop friendly relations among nations based on respect for the principle of equal rights and self-determination of peoples." Buchheit points out, in a succinct discussion of the drafting of that article, that the representatives of the nations which were drawing up the charter were not in agreement in their use of the terms "states," "nations," and "people," and, more importantly, on the question of whether or not the right of self-determination implied the right of a people to secede from an existing state. Earlier,

he commented that "no State's population is so homogeneous that its leaders may openly embrace the right of [self-determination] without some lingering uneasiness. Nor could the United Nations (whose membership, interestingly, is limited to states—not nations or people) realistically be expected to advocate a principle that might operate to dismember the territory of its members." Buchheit notes the inconsistency between justifying on moral and legal grounds the desire of a colonial people to cast off the domination of their foreign governors and denying the plea of a minority which feels itself imprisoned in a state and governed by an alien majority of an identical means of relief.

In contemporary terms, Buchheit takes particular note of the interference or non-interference of outside forces on behalf of both the legal group and its secessionist opponents. He examines legal precedents and new legal doctrines which have been used, both to support and to deny foreign intrusion into "domestic matters." In a careful review of the Congo issue of the early 1960s, of the Kurds' attempted secession from Iraq between 1961 and 1975, of the tragedy of Biafra, of the problem of Somali people in the relations between Kenya, Somalia and Ethiopia, of the unending struggle of the Nagas from India and, finally, of the secession of Bangla Desh from Pakistan, Buchheit searches inductively for the standards of secessionist legitimacy which transcend the particular cases and can be raised to general principles for use in settling future secessionist cases without resorting to war and interference in domestic matters by the military forces of the UN or of independent nations. No clear principles or guidelines emerge for modifying international law so that it can speak with authority on the issue of secession. As the author makes plain in several places throughout his study, the issues which provoke the demands for secession are internal and so long as international law deals mainly with interstate relations it can offer no consistent guidelines for solving secessionist questions.

To conclude his study, Buchheit attempts to develop what he calls a standard of legitimacy, which, if it were adopted and well publicized, as well as subscribed to by states, might provide a means for adjudicating secessionist disputes without seeing them resolved by force of arms. Admittedly, his proposal is vague and imprecise; however, it moves the argument away from considering each case by different standards and relies instead upon creating new law which will take account both of the state's right to maintain its territorial integrity and the secessionist's claim to self-determination. Buch-

heit's argument is clear and reasonable, and it makes a good beginning. Whether it will cause legalists and politicians to join efforts to find a peaceful way to resolve this important question remains to be seen.

This is an important study which both traces and summarizes the existing literature while at the same time commenting upon it. It seeks to carry us beyond the quagmire of particularism into a discussion of the issue of secession against a universal standard which provides a possible way out. It is well written, tightly reasoned and a pleasure to read. One hopes that the audience for whom this superb book was intended—legal scholars, states and their discontented minorities—will read and profit from it.

JOSEF SILVERSTEIN

*New Brunswick, New Jersey*

**Power Politics.** By Martin Wight. Edited by Hedley Bull and Carsten Holbraad. (New York: Holmes and Meier, 1978. Pp. viii + 317. \$22.50.)

The original version of Martin Wight's *Power Politics* was published by the Royal Institute of International Affairs in 1946. An essay of 68 pages, it has had a deep influence on the study of international relations in the universities of Britain. The work being reviewed is based on Wight's revision and expansion of this essay during the 20 years of scholarly activity that preceded his death in 1972. Wight's unfinished manuscript was completed for publication by the editors, whose scholarship is evident in the book's intellectual coherence and integrated style. The editors' introduction is itself an excellent critique of the work. I shall gladly acknowledge their judgments as I develop my own evaluation.

Two generations have passed since E. H. Carr noted that the science of international politics was in its infancy (*The Twenty Years' Crisis*, London: Macmillan, 1939). The lively polemics that have followed between traditionalists and behaviorists have undoubtedly advanced the study of international relations by diversifying the approaches to theory. But no truly definitive works have been written nevertheless. Thus, the value of a scholar's contribution may be assessed better by the questions raised in the field than by the answers provided. For modern science insists upon the hypothetical nature of empirical knowledge. Consequently, those works are classics of international relations whose postulates raise lasting and vigorous controversy. One remembers Hans Morgen-



thau's *Politics Among Nations* (New York: Knopf, 1948), and Morton Kaplan's *System and Process in International Politics* (New York: Wiley, 1957). As Imre Lakatos points out in his "Falsification and the Methodology of Scientific Research Programs" (in Imre Lakatos and Alan Musgrave [eds.], *Criticism and the Growth of Knowledge*, Cambridge, 1976), not an isolated theory, but only a series of theories can be said to be scientific: to apply the term "scientific" to one single theory is a "category mistake" (p. 119). Martin Wight's *Power Politics* is in the mold of such classics. It is also unqualifiedly of the traditionalist school. On the other hand, it does not present a general theory of international relations based on power. As the editors note, it provides no methodological prolegomena, but addresses itself directly to the substance of international politics.

A substantial portion of the book addresses such fundamental features of international politics as dominant powers, great powers, international revolutions, vital national interests and prestige, the balance of power, war and intervention, and international anarchy. Wight's analysis of these factors is grounded in a thorough and sensitive knowledge of history, rare in contemporary literature. It combines insight with erudition, clarity of language with richness of illustration.

But is Martin Wight's *Power Politics* a curio: well done, but useless to today's concerns in the study of international politics? In agreement with the editors, I must stress that Wight makes little or no attempt to relate to the academic literature on international relations that has engulfed the field during the 20 years he labored to expand the original essay. Given his approach, it may be understandable why he chose not to come to terms with the issues on methodology raised by the social scientific school. His indifference to the economic aspects of relations among states; the role played by transnational actors other than states, like classes, political parties, or multinational corporations; or the importance of Third World countries in reshaping the international system is, on the other hand, more surprising. It detracts from the value of the book all the more because the structure of Wight's approach could have easily incorporated consideration of these aspects of contemporary international relations.

Wight did attempt to up-date his analysis in one important aspect. There are chapters on the arms race, disarmament, and arms control. These chapters are, however, less insightful and little developed conceptually, and suggest that

Wight had not fully understood the unprecedented and potentially severely disruptive impact of the military technology of the nuclear era. Lest I appear too severe in this judgment, I would emphasize that this is a fault widely shared with the authors of the conceptual literature on international politics, regardless of methodological approach. The prolific literature on deterrence, nuclear strategy and arms control, and the wide-ranging literature on international politics have not been integrated into conceptualizations that articulate the crucial roles of nuclear technology in international politics.

*Power Politics*, after all, focuses on the elements of continuity, not change, in international politics. This is the main strength of Wight's work and its greatest utility for contemporary scholars. Wight's study is a rich source of insights and verbal models providing working hypotheses worthy of test by theorists of international relations—regardless of methodological approach.

CIRO ELLIOTT ZOPPO

*University of California, Los Angeles*

**The German Problem Reconsidered: Germany and the World Order, 1870 to the Present.** By David Calleo. (Cambridge: Cambridge University Press, 1978. Pp. xi + 239. \$11.95.)

What is the German problem? Ought the German problem be reconsidered? Does the German problem need reformulation in 1980? David Calleo, in his elegant attempt to look at old material in a new and interesting way, has attempted to answer these questions by examining Germany and the world order from 1870 to the present.

Calleo's thesis is that Germany's history of aggression should be regarded primarily within the context of the evolution of the European nation-states. By treating internal German politics only superficially he implicitly rejects the thesis (espoused by Bracher and Dahrendorf, among others) that blames German aggression on the allegedly vicious nature of the German political culture. Calleo argues that Germany's behavior as an aggressor in the international system is not different from what he describes as the similarly expansive behavior of the other major nation-states. At the same time he also plays down the uniqueness of Germany's domestic political culture, and particularly objects to the generally accepted thesis that Germany was uniquely aggressive externally because she

was uniquely vicious domestically. His evidence is not convincing. Perhaps Germany was imperialist in a sea of imperialism, yet the existence of the Prussian three-class franchise and the existence of an imperialistic foreign policy were not unconnected. Unresolved domestic problems prescribed an aggressive stance abroad. Although Calleo may be right that Germany's response to modernization was not basically different from that of other countries, it went further than that of its neighbors. The restless, expansionist character of German foreign policy mirrored internal tensions.

Calleo argues for historical continuity in German politics. Although he does not specifically treat the current German controversy of the 1945 *Nullpunkt*, he spends much time discussing intellectual and geopolitical constants in German history. The omission of a discussion of internal politics again flaws the argument. The historical continuity in German politics lies primarily in the internal political and social structure. The failed revolution of 1848 and the outlawing of the Social Democratic party in 1878 contributed to the lack of trust in democratic institutions and fear of the left characteristic of the German bourgeoisie. Even today these fears surface in the polarization of parliamentary debates. Yet at the same time there are breaks in the continuity of German history, particularly after 1945. The experience with Nazism, the 1945 collapse, and the experience with Communism next door have created a new international situation. The modernization of German society, the American and European influences, the new social and economic mobility have created an atmosphere in which the older ideological discussions which figure so prominently in this book have lost much of their relevance. The German problems today are much the same as those faced by the rest of the industrialized countries of Europe.

In his revisionist attempt to play down the uniqueness of Germany, Calleo devotes only a paragraph or two to the Weimar Republic. The dichotomy between cultural flowering and political disorder, the contempt for "normal politics" that characterized Weimar led to the immoderate politics of the thirties. National Socialism can only be understood in the context of the social and political conditions of Weimar, a peculiarly German blend of innovation and tradition. But at least Calleo avoids the current vogue of making lengthy excursions in search of the intellectual origins of Nazism or of dealing in analogies between Weimar and today's America. It may well be that the Weimar Republic came too early in German

history, and that Germany is, as Hellmut Plessner once wrote, the *Verspaetete Nation*. Only today has Germany finally completed its revolution of modernity and become a modern industrial society indistinguishable from its neighbors.

At the end of his book Calleo suggests that it is not only the Germans who should study German history. But if the problem of modern industrial mass societies is to adapt old institutions to new circumstances, then Calleo's grand style of history may be less relevant today. The German problem in history was one of resolving social conflict. Thus the vast output of German history, whether constitutional, intellectual, diplomatic or political, needs to be harnessed to modern sociological and political science analysis.

The origin of the Holocaust, the World War I guilt question, inflation and reparations, Bismarck's diplomacy, the *Ostpolitik*—these are questions which in a moral and political way have affected all of us in the twentieth century. They are reviewed here provocatively from a broad geopolitical outlook. What has made Germany so fascinating has been the impact of its politics not only on the personal destiny of every Central European but also on the fundamental questions of the social sciences.

CHARLES R. FOSTER

#### *Committee on Atlantic Studies*

**Forecasting in International Relations: Theory, Methods, Problems, Prospects.** Edited by Nazli Choucri and Thomas W. Robinson. (San Francisco: W. H. Freeman and Co., 1979. Pp. xii + 468. \$29.95.)

This book gives a broad survey of forecasting in international relations in the 1970s. It discusses the philosophical problems of forecasting in detail, covers a wide range of forecasting methodologies, and deals with the problems of applying forecasting to policy making in the United States and elsewhere in the world. However, the subtitle "Theory, Methods, Problems and Prospects" should be taken literally and as a caveat, as the articles discuss these issues almost to the exclusion of "Results." The book should be required reading for anyone interested in becoming involved in forecasting or using forecasts in international relations, and will be useful to those doing forecasts in other policy areas, but contains little material of general interest.

Some form of forecasting is involved in almost all policy making but most forecasts

have been done on the basis of the training and intuition of the policy makers or their consultants. Since the record of history shows such forecasts to be anything but flawless, the methodologies of modern political science provide a plausible alternative. The ideal forecasting methodology would provide decision makers with a crystal ball which would foretell what is going to happen under any set of conditions. However, it is clear from the discussion in this book that the crystal ball is not forthcoming. Dozens of complications are noted by different authors; I would single out three as most important. First, there is the inherent unpredictability of politically important events such as deaths and natural disasters. Second, there is the complexity of social systems and the fact that the number of possible futures expands exponentially even in short time frames. Third, the policy community can react to a forecast crisis either to reduce the probability of the crisis (e.g., population growth) or at worst to exacerbate it (e.g., the "missile gap").

While forecasting uses many of the methodologies of the scientific study of social phenomena, it has had a difficult time finding a niche in that approach. As Cheryl Christensen notes, forecasts are part of but not all of the scientific approach. In general the policy community is uninterested in the theory underlying forecasts, and that neglect of theory is apparent in the book. Only one essay, by John Ruggie, deals in depth with international relations theory that might be used to construct forecasts. Generally, forecasting models appear to be constructed either of a series of plausible bivariate hypotheses, as in the systems dynamics approach, or else the theoretical approaches are implicit in the minds of the experts assembling the forecast. With few exceptions, the only theory one encounters here is probability theory; the lack of cumulativeness vis-à-vis the international relations literature is acute.

More bothersome than the lack of theory is the lack of *forecasts*, which a reader might reasonably expect in a book on the subject. Most of the examples used are ad hoc and loosely reasoned, the sort of illustrations one would present to an undergraduate class. These do nothing to engender confidence in the techniques. There are four notable exceptions: Thomas Milburn discussing the Michelson Project, Edward Azar discussing the COPDAB crisis warning model, Nazli Choucri and Marie Bousfeld discussing extensions of the Choucri and North Nations in Conflict (W. H. Freeman, 1975) model, and Robert Young and G. Robert

Franco discussing long-range projections by C.A.C.I. All present relatively unambiguous forecasts with reasonably clear, though not replicable, descriptions as to how the forecasts were made. These, however, are only 4 chapters out of 28 and employ only a fraction of the methodologies suggested. The editors state in the preface that they are "painfully cognizant" of the absence of forecasts: so is the reader.

Despite the absence of actual forecasts, about a third of the book is devoted to the discussion of forecasting methodologies, including Delphi techniques, Bayesian analysis, econometric methods, systems dynamics, computer simulation, Markov chains and other techniques. Richard Ashley on Bayesian methods, Choucri on econometrics and systems dynamics (two chapters) and Azar on forecasting using events data are particularly good.

Throughout the book a healthy concern is shown for the normative issues in forecasting, a concern conspicuously absent from the earlier "systems analysis" forecasting concerning nuclear war, economic development and other social issues in the 1950s and 1960s. The essay by A. Michael Washburn and Thomas E. Jones is particularly good on this issue. The social science profession seems to have—finally—lost its innocence concerning the possibility of making value-free "scientific" statements in a value-laden policy world. In the policy world, unlike in formal logic, the truth of the consequent of an "If . . . then . . ." statement seems to imply the truth of the antecedent and in contrast to Milton Friedman's oft-cited suggestion, we probably cannot afford to make arbitrary assumptions solely on the grounds of accurate predictions.

But lest one becomes too engrossed with the possible applications of forecasting, Chester Cooper's essay on the uses of forecasting in Washington is a chilling reminder that even if one were to overcome the technical problems of accurate forecasting, that information would probably not be used. Accurate *current* information—as in the Bay of Pigs incident and during the Vietnam War—often fails to have an impact, so one could not expect any more of forecasts. Nevertheless, the Department of Defense has funded extensive research in crisis forecasting (see the March, 1977 *International Studies Quarterly* [Vol. 21]); and the Central Intelligence Agency has started to employ econometric forecasting. The State Department, on the other hand, appears to have little interest in developing new forecasting techniques.

In conclusion, forecasting in some form has been and will be an integral part of the policy

process and is one obvious application of research methodologies developed in the social sciences. However, it does not appear that we are on the verge of any breakthroughs in the area, and forecasting may already be close to its limits of precision (which, I note, is itself a forecast). The problems presented far outstrip the methodologies and one is constantly brought back to the absence of examples, a silence which is unsettling. The book is presumably intended for internal communication within a small academic and policy community—and will doubtless serve that purpose well—but it seems to outline difficulties rather than solutions for political science generally.

PHILIP A. SCHRODT

*Northwestern University*

**Tides Among Nations.** By Karl W. Deutsch.  
(New York: Free Press, 1979. Pp. viii + 342.  
\$17.95.)

Reviewing an anthology of Karl Deutsch's work is a far from simple assignment. The scope is incredibly broad, ranging from the social psychology of national loyalty to the rise and fall of empires. It is at once as detailed as the size of the industrial work force in 1955 and as sweeping in its generalizations as to suggest the basic and recurring patterns of nation building over the past five centuries. Further, his metaphors run the gamut from the thermostat to oceanic tides and currents. Despite this conceptual and empirical diversity, Deutsch's work shows an underlying unity, and this particular volume—whose articles date from 1940 to 1974—makes that unity abundantly clear.

That coherence lies, first, in Deutsch's focus on the relationship between the global system and the nations that constitute it. Second, there is the epistemological unity by which he skillfully combines carefully devised data sets with broad theoretical models. Third, and perhaps most salient to political science today, is the normative coherence, with description, explanation, prediction, and prescription side by side, carefully differentiated but explicitly intertwined. In a social science that is as much in flux today as it was when Deutsch's work first appeared, and as rife with division as when the behavioral revolution—of which he was a major instigator—first broke upon us, this is as valuable as his theoretical breadth and conceptual creativity.

Most of the chapters in *Tides Among Nations* revolve around an intriguing dilemma. That is, our nation-centric system has historically been

a relatively war-prone one, yet there is little likelihood of restructuring the system quickly enough to make it much less war-prone in the nuclear-missile age. Put differently, nationalism has historically been a major concomitant of war, but while war must be reduced or eliminated in the near future—if we are not to suffer untold destruction—nationalism is likely to be with us for perhaps another century. This dilemma may, it seems, be broken down into one empirical generalization, one conditional prediction, and one unconditional prediction.

The generalization is that a system of sovereign territorial states is necessarily warlike, and this seems hardly controvertible. According to data from the *Correlates of War* project, we have experienced from three to thirteen international wars per decade, with a mean of six, since the Napoleonic wars, and there is nothing in the time plots to suggest either some clear periodicity or any secular trend; up or down. The conditional prediction is essentially an extrapolation of the above pattern: as long as the national or multinational territorial state is the dominant form of human organization, war will continue with deadly regularity. The unconditional prediction, however, is equally interesting, clearly central to the book's argument, but remains less than demonstrable at the moment: that nationality and nationalism will remain the dominant form of social organization in the global system for perhaps another century. Most of the articles in this anthology address that prediction, and they bring to bear an impressive amount of direct and indirect evidence, along with the rigorous theorizing and original speculation that so characterize the research and writing of this distinguished colleague.

In the first part of the volume, the evidence for the durability of the nationality preference is offered largely in terms of the lengthy historical process by which nations took shape. Not only does Deutsch find some impressive uniformities in that process; he also finds some radical transformations in a wide variety of social practices and institutions, many of which must be wholly or partly undone if we are to go beyond nationalism. More importantly, he finds that the material and psychic advantages have been greater than is often appreciated, suggesting that vested interests as well as habit stand as strong bulwarks against any immediate assault on the structural and cultural patterns associated with nationalism. Particularly noteworthy in this part of the book are Deutsch's creative use of quantitative indicators of social mobilization and his insightful analogies between coalition and competition in the economic and the

ethnic-linguistic spheres.

In part 2, we shift from an emphasis on the factors making for the growth and durability of national consciousness to the problems of identifying and measuring social distance and interaction between nations. From the practices by which governments generate acquiescence, if not active support, of major groupings, to the relative propensity toward intra-national and extra-national transactions, these four chapters nicely combine a range of important theoretical insights with some ingenious indicators of interaction, interdependence, and distance. Then in part 3, the focus is on the post-World War II processes and prospects of European integration. Drawing from the historical patterns discerned in the processes by which Switzerland, America, Italy, and Germany were integrated into relatively effective multinational states, Deutsch examines the early development and subsequent slowing-down of the West European process. Also important here is the author's balancing of the factors—material as well as symbolic—that make for disintegration and secession, along with those that presage the formation of larger political units and security communities.

Then in the fourth and final part, Deutsch returns to the relationship between national integration and international conflict. While expressing the hope that “during the next half-century, world politics may become in some aspects more similar to domestic politics, and that some counterparts to intra-national welfare policies and income taxes may have to be found and accepted” (p. 8), the author's analyses in these last three chapters hardly offer grounds for optimism. To the extent that a certain level and distribution of wealth is a prerequisite for world peace, the data suggest that neither in growth nor in redistribution are we moving rapidly enough. Similarly, and making a point closer to his basic theme, Deutsch reminds us that nationalism is “an epistemological disaster” to the extent that “urgent and relevant messages from reality are overridden by unrealistic or irrelevant messages which this ideology [nationalism] prefers” (p. 302). Moving to other requirements for keeping the peace, or at least avoiding nuclear war, he notes two that seem dangerously distant: one is the acceptance by the wealthier nations of an international income tax, and the other is the renunciation of a nation's right “to initiate or provoke warfare without an international mandate” (p. 313).

In sum, *Tides Among Nations* offers a fascinating and provocative sampling of Karl Deutsch's impressive output since his arrival on

the American academic scene. While he has been no more successful than the rest of us, here or abroad, in cracking the causes of war/conditions of peace problem, or coming up with a solution that is likely to be politically acceptable and effective, he has done more than almost any other scholar in moving us ahead on those two paths. His extraordinary capacity to bring historical facts, correlational knowledge, theoretical models, and quantitative techniques together should provide both a more solid base for the research of the decades ahead and demonstrate that creativity and precision, not to mention concern and competence, can and must inform the efforts of all serious social scientists. The likelihood of making the necessary global changes in time is not very good, but to the extent that we follow Deutsch's example, we nevertheless retain a fighting chance.

J. DAVID SINGER

*University of Michigan*

**Myth, Oil, and Politics: Introduction to the Political Economy of Petroleum.** By Charles F. Doran. (New York: Free Press, 1977. Pp. x + 226. \$12.95.)

Charles Doran's book suffers from ambivalence in purpose: its ostensible and real messages compete for attention.

To explore the role of myths in shaping energy policy, Doran devotes one chapter to each of the following myths: that oil prices are unfairly high; that Israel is the fundamental catalyst for OPEC unity; that profits of the oil industry are excessive; that divestiture of oil firms can solve the energy problem; that the International Energy Agency (IEA) reflects the solidarity of consumers; that OPEC is inherently cohesive. According to Doran, these myths serve important psychological and political functions for their adherents, such as creating certainty for individuals or enhancing loyalty to causes; but they contain substantial, albeit often subtle, elements of falsehood which go unrecognized by their believers. The persistence, influence, and irrationality of the myths, he suggests, have impaired U.S. energy policy.

Doran's observations seem to require—as he acknowledges—an analysis much like those produced for studying the role of symbols in politics. But, on this score, the book disappoints. Doran never satisfactorily explains why his list constitutes core myths. Moreover, he skirts the problem of finding tests for the

impacts of myths on political processes and outcomes. For example, he observes that the U.S. public distrusts petroleum companies, but after offering a conventional explanation for this phenomenon, he does not demonstrate whether it affects the substance of policy, or merely its timing and form.

Ultimately, Doran's failure to explore the subtleties surrounding myths is not critical because the apparent theme of the book obscures its real focus; it is a series of essays disproving hypotheses which he considers erroneous. As such, the book is a modest, though uneven, warning against simplistic diagnoses and solutions of energy problems. It contains few materials which will be new to specialists. Its appropriate audience will consist of scholars and sophisticated political actors who have not specialized in the field.

The chapters on oil prices, the role of Israel, divestiture, and OPEC are the most rewarding. They set forth maxims drawn from welfare economics, theories of monopoly and oligopoly, and political science in a sensible and succinct manner. The essay on divestiture does an especially good job of showing the strategy's unintended consequences. It ignores, however, the best case for divestiture: one arguing that firms exceed the size necessary for optimum efficiency and, therefore, that divestiture (in the manner of action against Rockefeller's Standard Oil trust) should force disposal of subsidiaries to reduce size, not abolish vertical integration.

The chapter on profits in the oil industry correctly concludes that competition increased after 1945 and profits were not extraordinary. But its reasoning is shaky. It mistakenly looks at profits on refining operations as a good test of overall profitability. Refining profits were often low during years of very large profits on production because of the companies' oligopolistic strategies. More generally, it short-changes the evidence showing how corporate maneuvers slowed the decline of oligopoly and inadvertently eased the way to cartelization by OPEC.

The chapter on the IEA has the most claim to originality, but it is the weakest. It begins with a series of misleading emphases. For example, during the effort to install a minimum safeguard price for energy in the IEA there is little evidence, despite Doran's emphasis, that anyone worried much about the issue of wind-fall profits or thought the U.S. would act unilaterally. Doran also misses important aspects of the work done on creating national review programs, on monitoring the oil market, and on the design of arrangements for research and development cooperation which empha-

sized selective participation by members.

Doran suggests that most IEA countries are so different in their energy profiles from that of the U.S. (and, to a lesser extent, Japan and West Germany) as to make the organization ineffective. Unfortunately, the test of this provocative hypothesis is mediocre. The "Q-factor analysis" of elements affecting the "foreign energy dependence of consuming countries" (p. 130), the key indicator of difference, is inadequate. Some of the factors, like net capital inflows, have only slight connections to the level of dependence. Moreover, Doran is not adequately sensitive to the perennial problem resulting from giving the factors equal weight. In addition, the variables rely on data from 1970 and 1972, thus failing to capture such subsequent crucial swings as the ones for energy imports into the U.S. and the U.K. The ultimate predictions of coalitions capture neither key divisions over specific programs nor the general interplay of the U.K., Italy, Japan, France and Canada with the rest of the IEA.

Astonishingly enough for a book on myths, its hypothesis on the dynamics of the IEA also ignores the role of ideological factors, such as preferences concerning the regulation of markets. And it does not explore one major competing explanation for the IEA's difficulties: that the U.S.'s decline in power means that the IEA lacks a truly hegemonic power, which certainly must be singular in its profile, capable of organizing cooperative ventures. Thus, Doran's hypothesis, and the prescriptions drawn from it, are suspect.

In summary, for specialists this study lacks either the nuance or the originality necessary for a book to stand out in the flood of recent work. Nonetheless, Doran has synthesized a considerable range of valuable materials in a single volume which generalists may find rewarding.

PETER F. COWHEY

*University of California, San Diego*

**Presidents, Secretaries of State, and Crises in U.S. Foreign Relations: A Model and Predictive Analysis.** By Lawrence S. Falkowski. (Boulder, Colo.: Westview Press, 1978. Pp. xii + 173. \$16.50, cloth; \$7.50, paper.)

Students and practitioners of foreign policy have long believed that the secret of successful foreign policy is an effective foreign policy decision-making process. If the process "works," so the argument goes, the desired outcome will follow. Making the system work is

often defined (among other ways) as increasing communication among bureaucratic participants, increasing the availability of information, encouraging the search for alternatives, and teaching policy makers to learn from their failures. These procedures are designed to maximize the rationality of the process and, by implication, to achieve the "best" outcome. Falkowski's effort to explain U.S. foreign policy behavior is of this genre.

Lawrence Falkowski develops a model of flexible foreign policy behavior, where "flexibility" characterizes a good or an effective foreign policy process. His model addresses three questions: Do U.S. leaders (presidents and secretaries of state) exhibit flexibility? Do different leaders exhibit different degrees of flexibility? What factors are related to a leader's flexibility or inflexibility?

Falkowski hypothesizes that differences in leaders' goal referents, goal themes, and policy themes (as reflected in their speeches, news conferences, and testimony before congressional committees) are related to differences in perception, and that variations in perceptions can be employed to explain the likelihood of flexible behavior on the part of any decision maker. Specifically, the greater the congruence between referents and the current situation, the more flexible is foreign policy behavior. And the greater the congruence between referents and goal themes in terms of a particular issue, the more flexible is foreign policy behavior. (The implicit hypothesis, of course, is that the greater the flexibility in foreign policy behavior, the "better" is the policy.)

Falkowski tests these and other hypotheses by focusing on those situations most likely to encourage flexible foreign policy behavior, namely, crises that have been deemed failures. If leaders exhibit any flexible behavior, so Falkowski hypothesizes, they should do so after they have failed in a crisis. On the basis of his analysis of the pre- and post-crisis perceptions and behaviors of presidents or secretaries of state from Truman to Nixon, Falkowski concludes that his hypotheses are supported. That is, the extent of foreign referents and the degree of congruence between referents and goal themes explain flexibility.

On the positive side, this volume demonstrates that psychological variables can be addressed more or less successfully by non-psychologists, and that elements of memory can be identified, defined, and operationalized.

On the other hand, Falkowski's model of flexible foreign policy raises questions in my mind which are not adequately addressed in this volume. First, the model is not really a

model of foreign policy *behavior*, but a model of the flexible *leader*, defined in terms of the leader's dispositions. The independent and dependent variables in the research design are "themes"—the "intellectual baggage" of foreign policy decision makers pre- and post-crisis (p. 58). And yet Falkowski defines flexibility earlier in the volume as "a change in observed behavior from a previous pattern of behavior" (p. 4). In short, there appears to be some confusion surrounding what the model is designed to explain.

A second question concerns Falkowski's decision to examine only crises that have been deemed failures. A valid test of the model requires examination of three other logically possible situations: crisis successes, non-crisis failures, and non-crisis successes. In the absence of data concerning perceptions and behaviors in these situations, conclusions derived from crisis failures are less than completely persuasive.

A third question is one that arises not only from a reading of Falkowski's study, but also from analysis of much of the "successful process" literature. Falkowski assumes that flexibility results in favorable foreign policy outcomes. But flexibility could also be characterized as inconsistency or unpredictability. Flexibility could suggest the absence of "policy" (at the behavioral level) or the presence of instability (at the intellectual baggage level). For instance, President Carter's "flexibility" on the enhanced radiation warhead issue brought charges of inconsistency from both domestic and foreign critics. The assumed linkage between successful process (particularly if defined as flexibility) and desired outcome is tenuous at best.

In summary, Falkowski's model of flexible foreign policy raises many hard research questions which remain to be addressed.

LINDA P. BRADY

*Alexandria, Virginia*

**The New Politics of Human Rights.** By James Avery Joyce. (New York: St. Martin's Press, 1978. Pp. xi + 305. \$19.95.)

**Victims of Politics: The State of Human Rights.** By Kurt Glaser and Stefan T. Possony. (New York: Columbia University Press, 1979. Pp. xxiv + 614. \$35.00.)

The reading of any national newspaper on any given day in this country is bound to turn up at least one item identifying a situation exhibiting the lack of effective implementation of international human rights principles. The



conditions of human life are affected by an unending series of man-made catastrophes. Nevertheless, today, more than at any other time in the history of mankind, there is a rising tide of hope for the protection of the individual's human rights against the state.

James Avery Joyce, in the preface, acknowledges that *The New Politics of Human Rights* is "but a broad survey aimed at the cultured reader." It is, also, "not a book for lawyers"; hence, "a legalistic style has been avoided as far as is compatible with accuracy and clarity." Furthermore, the legalistic approach is abandoned for the reason that a "serious study of human rights has become today an interdisciplinary exercise, involving history (origins), geography (frontiers), diplomacy (negotiations), sociology (group structure), psychology (motivation), ethics (standards), and many other formal disciplines, as well as constitutional and international law." The "cultured reader" will find an absence of any interdisciplinary approach, and although the title suggests a survey from a political perspective, it too is woefully lacking. In sum, the book has few pretensions and even fewer achievements.

Joyce gives the reader a synoptic view of the 1948 Universal Declaration of Human Rights, the 1949 Geneva Red Cross Conventions, and the 1977 changes in the Laws of Armed Conflict, without analysis of the political challenges leading to their final adoption. His survey of past human rights accomplishments involves slavery, humanitarian intervention, minorities, labor standards and the mandatory system. Recent examples of states being caught *in flagrante delicto* include situations in South Africa, Chile, Israel and the Soviet Union, among others. Not without merit is Joyce's treatment of the doctrine of self-determination by the third world, who are now the advocates of neo-colonialism. Another strength of the book lies in Joyce's assessment of scientific and technological advances with their concomitant encroachment and constraint upon the rights of privacy and death. On the other hand, the topics of genetic engineering and organ transplants receive only cursory inspection on an elemental level. While the examination of how the 1966 International Covenants and the Council of Europe's European Convention operate is adequate for the lay person, a political perspective would have enhanced its value. The need for a UN High Commissioner for Refugees and to a lesser extent, a British Bill of Rights, are familiar nuggets assayed without new insight. Finally, Joyce advocates that the purpose of the new legal order will be the individual.

In *Victims of Politics* Kurt Glaser and Stefan Possony have produced a definitive work with a catalog of human suffering encompassing the spectrum of human rights violations. There is an enormous amount of scholarship, sifting and refining of materials, deep thought and erudition in this volume. Where does this panorama of pain and suffering lead to? Who is responsible? The authors provide a number of sound explanations for the limitations on the implementation of human rights, explanations of human perversity and the role of ideology, and they point out problems inherent in the practical application of human rights standards.

Indifference to injustice is not a modern-day phenomenon. The authors show that "we need to revise our political theory so that it will place people first and abstract principles in a lower priority" (p. 544). They do not have the recipe for a panacea to cure this cancer permeating our planet. However, the empirical, psychological theoretical phenomena which lead to violations are amply reviewed.

The recognition and implementation of human rights principles is a universally valid moral command. Glaser and Possony agree that to succeed, a human rights policy must not be an instrument of publicity coups, nor have double or multiple standards. This policy should only focus on consistent violations rather than occasional lapses; it must be low-key, a polite exercise in persuasion, not aimed to arouse hatred or aggression. Any successful policy will contain its own guarantee of due process.

Despite the mass of facts illustrating abuses of human rights, Glaser and Possony feel that "the road toward greater realization of human rights remains open. It is long and strenuous but progress can be made step by step" (p. 555).

This book stimulates the reader. Not only have the authors whetted the appetite of those who want to learn about human rights violations and its causes, but they have provided a smorgasbord on the subject. Those seeking to dine elsewhere will find a helpful 30-page bibliography.

DANIEL C. TURACK

*Capital University Law School*

**Crisis Resolution: Presidential Decision Making in the Mayaguez and Korean Confrontations.** By Richard G. Head, Frisco W. Short, and Robert C. McFarlane. (Boulder, Colo.: Westview Press, 1979. Pp. xxv + 323. \$20.00.)

In May 1975, Cambodian armed forces



seized an American cargo vessel and its 40-man crew. In August 1976, North Korean forces killed two U.S. officers engaged in a tree-trimming detail in the tense north-south border area. How the Ford administration handled these "low-intensity crises" (p. xix) receives detailed scrutiny from three U.S. military officers associated with the National Defense University. Drawing on extensive inside information—including interviews with former President Ford and Secretary of State Kissinger—the authors show how the U.S. response was affected by such factors as "key policy makers' belief systems and the unique situational context" (p. 243), and the limits of available information.

The Mayaguez seizure came two weeks after Saigon's collapse, and Ford and Kissinger were determined to respond strongly, though U.S. forces in the area were limited, and the crew could not be precisely located. So after two days of military preparations and unanswered diplomatic messages, U.S. Marines launched a rescue effort. They met heavy Cambodian fire and 15 of them died; meanwhile, unknown to them and Washington, other Cambodian forces were releasing the crew. In the Korea case, U.S. armed forces asserted their rights by cutting down the tree where the incident had occurred. A limited apology from North Korea led to negotiations dividing the border area into north and south portions, reducing the likelihood of future incidents.

Going beyond crisis reconstruction, Head, Short, and McFarlane address broader theoretical and practical concerns. For academics, there is a cogent review of the literature on foreign policy crises, development of a conceptual framework, background on the National Security Council and Worldwide Military Command and Control systems, and a number of insightful conclusions. Head and his colleagues find that these crises gave the president particular opportunities to control policy, that bureaucratic interests were subordinated, that the formal crisis management system was used, not circumvented, and that, despite officials' sustained engagement, there was no evidence of "abnormal stress" distorting their behavior (Ch. 7). The book also provides intelligent suggestions on crisis management.

The authors' basic sympathy toward the participants, processes, and outcomes sometimes colors their conclusions. President Ford is praised for "removing politics entirely from the discussion of virtually every major issue which came before him" (p. 72). His 1976 refusal to cancel Kissinger's Rhodesia mission is the example cited; ignored are SALT and Panama, on

which Ford pulled back that same year. The Mayaguez action is labeled a "successful U.S. military operation" demonstrating "a strategic resolve worthy of a great power" (p. 148). The GAO criticism that the crew's release was unrelated to the Marine assault is quickly dismissed because this release "was not known" to U.S. officials at the time (p. 145), the U.S. had to show it "would act with firmness to protect its interests" (p. 110), and the worst-case outcome—the Cambodians' hiding the crew and humiliating the United States—was avoided. These are important considerations, and the authors are right to underscore the uncertainty within which officials worked. In turn, less certainty about the rightness of the course chosen would seem appropriate.

Head, Short and McFarlane also take formal procedures at face value, sometimes without exploring whether they were actually followed. In one background chapter, they insist repeatedly that Nixon and Kissinger operated an internally open policy process, citing as evidence mainly the system's formal description and design. Looking specifically at the Mayaguez incident, the authors begin by noting that normal NSC procedure included "attempt[ing] to determine the motives of the other party and its objectives" (p. 109). But the next 40 pages almost totally ignore this matter. Was the seizure the act of a local commander or Phnom Penh? The answer might have made a difference in how the U.S. should respond, and if this question was indeed neglected, this would seem a serious process flaw. If so, it goes unilluminated.

But despite such limitations, this book is an important addition to the literature on specific foreign policy crises. Although skewed by a sympathy that inside analyses often show, it gives us more than adequate compensation in the inside information the authors provide, and in their practical wisdom about how government works.

I. M. DESTLER

*Carnegie Endowment for International Peace*

**Quantitative Approaches to Political Intelligence: The CIA Experience.** Edited by Richards J. Heuer, Jr. (Boulder, Colo.: Westview Press, 1978. Pp. x + 181. \$16.50.)

The message of this book is that the social science research practiced by the overwhelming majority of quantitatively oriented scholars is a waste of time—at least by certain standards of judgment. The editor, Richards J. Heuer, Jr.,

and the several authors are much too circumspect to make any such explicit charge against their fellow citizens. However, in the most polite ways possible they point out that the available quantitative research was of little use when they began to try to upgrade their own forecasting and other analytical techniques.

The many reasons for the inappropriateness of the bulk of social science research are well described by explicit statement and example in the book. Some of the most damning comments are made almost in passing, as, for example, when Heuer in his introduction mentions that "the quantitatively oriented scholar can easily limit his work to those variables that can be operationalized, but the government analyst seldom enjoys that luxury" (p. 4). The implicit charge here is that quantitatively oriented scholars do, in fact, pick their research topics on the basis of those things that can most easily be measured.

The book also goes on to talk about why the models and methods used by scholars are inadequate for government use. There is no doubt that many of us will be happy that our work is so irrelevant to practical purposes, especially those practical for the CIA. For those people the book will be quite comforting. At least it will be, as long as they do not raise the question of whether the work they do interests *anyone* except another scholar.

Perhaps the most radical theme of this book (again, only implicit) is the major use of expert-generated data as a source of information on which to perform various statistical and modeling techniques. Three of the seven studies rely almost exclusively on expert-generated data. Even more interestingly, the use is not the conventional Delphi approach of eliciting forecasts of future conditions. There is much more obtaining of specific estimates of *current* conditions or the relationship between current conditions and future events. The results here are mixed. In particular some of the studies have tremendously complex and elaborate methodologies that probably do more harm than good to the data collected. One such example is the attempt by Harold E. Dahlgren to operationalize a theoretical model that will forecast domestic violence. Since I had a chance to evaluate this study before it appeared in the book, I am aware (even more than Dahlgren) that the data collected had a high degree of reliability (consensus among the respondents) and at least "face validity" (variations in the predictors did covary with incidence of violence in the countries studied). However, the elaborate model that Dahlgren presents sometimes tended to obscure this quite strong

performance of expert-generated data.

The other use of expert-generated data includes formulating Bayesian analysis estimates for Middle-East conflict and making forecasts about future events in Rhodesia.

These studies, along with those that use more conventional techniques such as regression analysis, content analysis, events data, and scaling of UN voting behavior, show the great potential of quantitative techniques. It is unfortunate that much of government work, as indicated by this book, has to begin almost from scratch with so little help from previous work by academics.

MICHAEL K. O'LEARY

*Syracuse University and  
Syracuse Research Corporation*

**Dimensions of China's Foreign Relations.** Edited by Chün-tu Hsüeh. (New York: Praeger Publishers, 1977. Pp. xiv + 293. \$20.00.)

This volume, edited by C. T. Hsueh, is a collection of ten articles which treat different aspects of contemporary Chinese foreign relations, but among which there is no common perspective or clear division of labor. The authors, most of whom are specialists on Chinese politics, do not ask similar questions, nor have they structured their analyses so as to complement the other essays in the volume. Davis Bobrow opens the collection with an introduction suggesting an analytical matrix in terms of which the articles might somehow be related to each other, but in spite of this valiant effort, Bobrow himself concludes by emphasizing questions which the authors might have asked rather than those actually posed.

Lacking a coherent organizing structure, the book can best be read, it seems to me, for the contributions made by the separate articles. The topics include: analyses of China's involvement with the two superpowers (by Steven I. Levine, Chün-tu Hsüeh and Robert C. North, and King C. Chen); studies of Chinese relations with Japan, Korea, the Palestinians, and Latin America (by A. M. Halpern, Chün-tu Hsüeh, Yitzhak Shichor, and Robert L. Worden, respectively); and, finally, an investigation of Peking's policies toward the Overseas Chinese (by Shao-chuan Leng) and a case study of the impact of the Cultural Revolution on Sino-Nepalese relations (by Roger L. Dial).

In my opinion, the articles are as uneven in quality as they are diverse in subject matter. Let me briefly comment on four of those

contributions which I found to be among the more interesting.

The first, Steven Levine's "Soviet-American Rivalry in Manchuria and the Cold War," examines the relationships among Moscow, Washington, Chiang Kai-shek's Kuomintang government, and the Chinese Communist party during the final stage of China's civil war (1945-1949) following the end of World War II. While the United States and the Soviet Union seemed to share the common objectives of encouraging the formation of a coalition government comprised of the two hostile Chinese political movements (the KMT and the CCP) and of generally refraining from direct military intervention in the Chinese civil conflict themselves, both Mao Tse-tung and Chiang Kai-shek sought cooperation with each of the two foreign powers—or, at least, attempted to win the support of one and to neutralize the other. Levine disputes the popular notion that different American policies toward the CCP at this critical juncture might have prevented the subsequent 20 years of Sino-American hostility (1950-1970), but he does show the tremendous influence of the change of American personnel in China on how events there were being reported to Washington, i.e., after Ambassador Hurley replaced officials advocating U.S. cooperation with the CCP (like John Service and John Paton Davies) with those who saw Chinese communism from more of a cold war perspective.

C. T. Hsüeh, surveying Chinese policy toward Korea over the entire period since 1949, analyzes the Sino-Korean relationship in terms of the historical links between the two countries, the principles of proletarian internationalism, and the realities of political power in the region. Hsüeh seems to suggest that in spite of China's verbal support for the reunification of the Korean peninsula and the withdrawal of American troops (as President Carter has promised), Peking might actually be willing to accept the status quo as the best of the likely alternatives. In fact, since the political outcome of any effort to unify Korea is indeed unpredictable, one wonders if all of the major powers involved in the area (the U.S., China, Japan, and the Soviet Union) might not prefer the status quo of a divided Korea—for China, especially since Peking's recent unhappy experience with a reunified Vietnam.

A third chapter of particular interest is "The Palestinians and China's Foreign Policy" by Yitzhak Shichor. Arguing that China's policy toward Israel and the Palestinians has been essentially a reflection of Peking's global policy, Shichor traces Chinese relations with the Pal-

estinians through three periods: the early years of China's alliance with the Soviet Union against the United States, the period from the mid-1960s when Peking confronted both superpowers as enemies, and, finally, beginning in 1971, the years of growing cooperation with the United States and continued confrontation with the Soviet Union. His point is that policy toward Israel and the Palestinians was shaped by the different requirements of China's global strategy during each period. Shichor makes good use of a hitherto virtually untouched goldmine of materials on Chinese foreign relations, the *Shijie zhishi nianjian* (World Knowledge Yearbook) and *Shijie zhishi shouce* (World Knowledge Handbook) series; and he has produced an impressively detailed and balanced analysis—in short, a valuable study of a topic which has received little previous scholarly attention.

The final article in this collection is Roger Dial's study of the Chinese foreign affairs bureaucracy which is conceived in terms of a general notion of organizational response to a crisis situation. Dial treats the domestic chaos of the Great Proletarian Cultural Revolution as a challenge to the established bureaucratic patterns of Chinese diplomacy, and he investigates the case of Sino-Nepalese relations, focusing on activities undertaken by Chinese Foreign Ministry professionals to protect their organization from Red Guard efforts to disrupt their normal organizational procedures. The result is a thoughtful case study which suggests lines of analysis which might fruitfully be pursued in subsequent studies of the role of the Foreign Ministry bureaucracy in Chinese foreign relations.

PETER VAN NESS

*University of Denver*

**The General Assembly of the United Nations: A Quantitative Analysis of Conflict, Inequality, and Relevance.** By Kurt Jacobsen. (New York: Columbia University Press, 1978. Pp. 209. \$15.00, paper.)

The book is based on research done at the International Peace Research Institute (Oslo), involving approximately ten years of quantitative analysis. The purpose is to "analyze some of the functions of the United Nations during the time the organization has existed" (p. 14). The book opens by treating the United Nations in terms of Almond's and Powell's framework to analyze political systems that include the categories of "capabilities," "conversion pro-

cesses" and "system maintenance and adaptation." From this point of departure, Kurt Jacobsen attempts to index "capabilities" through the scale of assessment for regular budget, "representation" through numbers serving in appointed and elected positions in the United Nations, "behavior" through presentations of proposals, participation in debates and voting on proposals, and "voting and interaction" through estimates of agreement in respect to voting behavior and as measured by co-sponsorship of proposals. Having established how he is going to index various key variables, Jacobsen then proceeds to evaluate the interest articulation and aggregation function of the United Nations in various ways. In this respect, "one's conclusion would have to be that there has been a shift-over time from the conflict model towards the consensus model" (p. 67).

Does this mean, then, that the world itself is becoming more peaceful? Jacobsen thinks not. "Our main point, however, is that one cannot and should not regard the U.N.'s relative 'peace system' separately from that of the 'peace' we are trying to create in the outside world. The observed levels of consensus and conflict would have to be evaluated in light of the levels of consensus and conflict that reveal themselves in all parts of the world, not just in the corridors in New York" (p. 67). In evaluating system maintenance, adaptation and the development of legitimacy, Jacobson finds that the United Nations has become less dominated by the East-West conflict, that the group of non-aligned nations has expanded considerably, and that, in general, the United Nations has become more "balanced." "The end result is, however, that the organization is less unbalanced in a Southern direction than it was in a Western direction in earlier periods. The extreme isolation of the minority group—the East—has been broken" (p. 127). In evaluating equality versus power, Jacobsen finds that although inequalities exist at the UN, reflecting power, they are less apparent in the UN system than in the non-UN world and that the explanation, at least in part, can be found in the one-nation, one-vote rule.

On the question of relevance, Jacobsen feels that the United Nations has become more "unrealistic" since the resolutions do not reflect, as much as they have in the past, the interests of the stronger, more developed states. He argues: "the products of activities become paper resolutions with limited consequences for the situation in the 'real' world" (p. 170). This, however, is admittedly a "Western" position, and from the viewpoint of the less-developed states "no action at all may be preferred to

action according to the wish and will of the powerful nations" (p. 170). Regarding the impact of socialization and communication, Jacobsen stresses the broadening of perspectives that are likely to develop in delegates through their interactions with others holding divergent viewpoints. That is, he feels that the United Nations may raise levels of mutual predictability which, in turn, may reduce levels of tension and conflict.

In general, the book is highly recommended to serious scholars of the United Nations. The greatest weakness is Jacobsen's tendency to ignore numerous significant contributions concerning the United Nations by Rummel, Russett, Wittkopf and others, many of which are not even listed as references, that are highly relevant to his effort. In spite of this weakness, however, Jacobsen has produced a high-quality, innovative and readable contribution to the literature on the United Nations.

JACK E. VINCENT

*Florida Atlantic University*

**U.S. Foreign Policy in Asia: An Appraisal of America's Role in Asia.** Edited by Yung-hwan Jo. (Santa Barbara, Calif.: ABC-CLIO Press, 1978. Pp. vii + 488. \$19.75.)

After the communist victory in Vietnam, America's commitment to its containment policy in Asia was subjected to anxious evaluation. Indicators were clear that America would withdraw from its forward lines on the mainland to its Pacific island bases. By 1976, this withdrawal seemed to bode ill especially for South Korea, a country that seemed fated to repeat South Vietnam's mistakes. In 1976–1977, therefore, experts were invited to three symposia hosted by the Center for Asian Studies, at Arizona State University, to reassess America's role in Asia and to examine the Korean question in relation to Vietnam. More than 30 experts on these and on other involved countries participated. This volume is a result.

Like other compilations of conference papers, this one lacks coherence. The authors of its 19 essays and contributors in six discussions communicate out of different backgrounds and values and discuss different subjects. They include critics and defenders of the disparate policies of the Soviet Union, North Korea, South Korea and America. Several scholars make important recommendations for new policies.

For their research materials, these experts relied mainly on their readings of newspapers,

periodicals, books and official reports. Since many of them are area specialists, they also drew on their intimate knowledge of relevant countries. In the main, they omitted explicit theorizing, quantitative analyses, or references to global political considerations.

Despite their differences, the experts shared the views that American power in and commitment to the defense of Asia has been reduced by Vietnam's victory, that the big four powers in Asia—U.S., USSR, China and Japan—did not seek any radically destabilizing changes in Korea, that the stability of Northeast Asia would not be secured until the two Koreas were neutralized and/or reunified, and that Vietnam, in 1976, was primarily concerned with its own economic development.

Part 1 examines the reasons for America's intervention in Vietnam and Korea. Soon S. Cho surveys familiar history and concludes that America's initial division of these countries was caused more by the ignorance, imprudence and pragmatism of U.S. policy makers anxious to enlist the support of Russian or French colonialists than by any rational plan. Robert Oliver, in the style of a personal memoir, recalls his experience as advisor to Syngman Rhee and echoes many of Cho's criticisms. Gareth Porter represents the early policies of America in Vietnam (1954–1963) as based on the vain hope that they could counter the forces of popular revolution where the French had failed. Selecting data from official reports and personal interviews, Porter depicts American policy makers as consciously designing an economic aid policy that would favor the rich. While his thesis is well argued, it plays down consideration of other relevant factors such as the softness of Saigon's bureaucracy, the stubbornness of Diem, and the Americans' lack of knowledge and control of the countryside.

Douglas Pike's succinct piece on U.S. policy toward Vietnam in 1976 reads like the outline of a government position paper; it depicts U.S. policy makers as waiting "to let the dust settle" while, hindsight suggests, the Russians "made hay."

Jane Werner bases her impressions of Hanoi's foreign policy on extracts from official statements; she hazards interesting speculations on Hanoi's future policies with mixed results. Scott McNall's essay on the U.S. rationale for a limited war in Vietnam proposes that a specific policy of limited conventional offense was aimed at demonstrating the credibility of a general policy of unlimited nuclear defense. While he argues persuasively, the rationales for U.S. intervention appear more plural than singular and varied according to time and

circumstance.

Phan Thien Chau appeals for a more accommodating U.S. policy toward Hanoi. Donald Weatherbee evaluates the impact of communist Indochina on Southeast Asia's security and describes new conditions defining a new environment affecting the foreign policies of regional actors at three levels. Weatherbee's analytic construct is unusually illuminating; he generates new insights from old data.

Part 2 examines American policy toward Korea. Gregory Henderson describes the negative effects of current repressive and militaristic policies in Korea and recommends that the four big powers cooperate to restrain their Korean clients and proceed toward stabilizing policies. In an emotional essay reflecting similar concerns, T. C. Rhee castigates South Korean policies that threaten to erode popular support by increasing domestic misery and foreign dependence. Anthony Khang rejects U.S. Asian policy and appeals for Korean unification on the communist model. Fred Carrier faithfully advocates official North Korean policies. Pyong Choon Hahm makes a contrary case for the South Korean government.

Part 3 analyzes America's foreign policies in relation to other Asian countries. Bernd Kaufman, an East German expert, ably presents the Soviet view of U.S. Asian policy. Yung H. Park's excellent article on Japan's policies toward Korea and America is one of the least prescriptive in the volume. Tracing the changes of Japanese policy toward the two Koreas in the 1970s, he perceives them as resulting from the changing pressures of domestic political party factions and business groups.

Douglas Mendel's spirited advocacy of a pro-Taiwan policy is apparently derived from his stout loyalty to that beleaguered government and from his reading of sympathetic sources. Robert Youngblood's essay on Philippine-American relations is gracefully written, coolly presented and solidly documented. Tracing the efforts of various nationalistic governments to develop independent foreign policies, he focuses on the Marcos regime and presents evidence indicating that, despite contrary claims, the New Order's economy and security remain dependent.

Robert Scalapino's paper on U.S. policy toward South Korea is a review of recent developments which plays down the problem of human rights, cheers signs of economic progress and carefully evaluates the interests of the big powers in Northeast Asia. In light of these factors and a promised reduction of U.S. forces, he prescribes a strategy for Korean demilitarization and negotiation that involves all these

participants in a plan for reunification.

Although several of these essays are seriously dated because of subsequent interstate hostilities, by new efforts at negotiations and by U.S. recognition of Peking, this volume contains so much pertinent information and so many insights on the subjects at issue that it should prove most useful to interested graduate students, to area specialists and to policy makers.

JOHN JAMES MACDOUGALL

*University of Alabama, Huntsville*

**The Leninist Response to National Dependency.** By Kenneth Jowitt. (Berkeley: University of California Institute of International Studies, Research Series No. 37, 1978. Pp. ix + 85. \$2.50, paper.)

The recent vogue of dependency theory, derived mainly from Latin American studies, cries out for a searching critique by scholars familiar with the origins and historical evolution of Leninism in Eastern Europe. This booklet by Kenneth Jowitt constitutes, one may hope, a beginning of that critique. Jowitt's purpose is to examine Leninism—the doctrine and the movement—as a response to political underdevelopment. Apart from liberal individualism, Jowitt considers Leninism to be the principal response. Like dependency theorists, he accepts the generalization that the critical aspect of political underdevelopment has been dependency upon the more advanced societies of Western Europe and their overseas progeny. His task, therefore, is to show how and why Leninism constituted a way of overcoming dependency at the international level, or, to be more exact, for replacing dependency on the Western European system by dependency on the Soviet Russian system. The new pattern, which he labels “combined substitution,” is a transformation of countries’ “domestic social order and international position *under the deliberate organizational auspices of a great power with whom they shared an ideological affinity*” (p. 79).

To show how Leninism came to constitute an alternative, Jowitt must, of course, refer to development theory. Drawing on his intimate knowledge of Romanian development, he substantiates Frantz Fanon's argument (anticipated by Romanian thinkers) that avoidance of a dependency situation requires the development of a national culture. The *modern* international order tends to replicate the *traditional* peasant social order as a strongly hierarchical status system, with “an international ‘big man’ patron who is expected to allow national ‘small boys’

to exercise control in their own bailiwicks in return for diffuse services to the patrons” (p. 26). Hence, for local elites, the international relationship, though a source of resentment, is familiar, therefore basically acceptable. Consequently, the elites adopt premature liberal institutions and styles to make themselves intelligible to Western patrons, until and unless a fully independent national consciousness arises. This interpretation appears very plausible for some international relationships and perhaps still more for internal ethnic relationships of dependency. Jowitt's interpretation is also a welcome corrective to the stress on economic and political factors which has commonly pervaded dependency theory. In fact, I can only regard Jowitt's analysis as “a sociopsychological interpretation of . . . dependency behavior,” although (p. 29) he categorically rejects this characterization.

Possibly recognition of the socio-psychological nature of his interpretation would have compelled Jowitt to recognize that his fundamental assumptions (though not his insightful treatment of Leninism) are much like Parsonian development theories, also ultimately derived from Weber. Instead, Jowitt (like all too many Eastern European specialists who turn to general theory) employs terms and concepts in idiosyncratic ways. He contrasts “status societies” with “class societies” without recognizing that, since Weber, generations of sociologists have accepted “status” and “class” as pervasive social characteristics. Nor is Jowitt's use of “impersonal” versus “trust” relationships an advance over the generally accepted “universalistic: particularistic” dichotomy. More seriously misleading is his attempt (p. 14) to develop a conflict model of changing family structures. Jowitt's belief that the nuclear family is a new phenomenon is simply wrong; it was widespread in Western Europe by the fourteenth century and may—rather than the “Protestant spirit”—constitute the key factor in the emergency of individualism there. To assert as Jowitt does (p. 14) that such a slowly evolving social form exists in a “kto-kovo” relationships with the extended family is a misuse of both the conflict concept and Lenin's polemically sharp adaptation of it.

Conversely, Jowitt is right in ascribing fundamental significance to Leninist transformation of traditional peasant extended family relationships, where such relationships are salient. However, he does not give sufficient attention to the crucial distinction for *peasants* of the *manner* in which Leninist regimes attained power. In my view, characteristics such as provision of heroic models and substitution

of party loyalty for traditional village solidarity are incomparably more significant in Communist-led guerrilla struggles, from which Jowitt (referring to Ho Chi Minh, Malaysia, etc.) derives most of his examples. It may be that in Romania comparable substitution patterns have been imposed successfully from above, and consequently contribute to a modernized social order. It is incontestable that the *initial* reaction of Romanian peasants (before massive Soviet force *compelled* disruption of the old village patterns) was rejection of *artificially* introduced guerrilla solidarity models. The artificiality of Leninist substitution patterns in other Eastern European countrysides (apart from Yugoslavia), in contrast to the considerable success of such patterns among modernized proletarian elements, appears to be even more evident, especially in Czechoslovakia and the German Democratic Republic. In other words, though Jowitt presents a seminal interpretation of the relationship of certain types of Leninist regimes to peasant transformation, his interpretation should not obscure the fundamental *Soviet* model of transforming the hostile, or at least apathetic village, by cadres based on urban fortresses.

JOHN A. ARMSTRONG

*University of Wisconsin, Madison*

**Career of Empire: America and Imperial Expansion over Land and Sea.** By George Liska. (Baltimore and London: Johns Hopkins University Press, 1978. Pp. xi + 360. \$17.95.)

*Career of Empire* is advertised as a companion volume to *Quest for Equilibrium*, George Liska's earlier effort to interpret American foreign policy in terms of equilibrium, or balance-of-power strategy. *Career of Empire* concentrates on a second motif—the impulse toward hegemony—and uses general propositions about the creation, consolidation, and dissolution of empires drawn from Roman and British history to examine America's imperial experience.

A reviewer of the first study wrote that it would repay "those who have the time and stamina to wrestle with close reasoning, complex syntax, and a high level of abstraction." The same can be said, if less euphemistically, about this essay. It is a maddening book—a provocative book written in prose frequently so turgid that the mind boggles. One typical extract will illustrate the need for "time and stamina." In discussing the consequences of delegating defense responsibilities to client-states, Liska writes: "It [i.e., the range of the

imperial power's controls] would then comprise only activities and factors apt to inhibit seriously the crystallization and continuous operation of near-autonomous regional balance-of-power systems that would have taken the place, in the devolutionary context, of a partially regionalized enactment of the global or central balance attendant on mere delegation" (pp. 278–79). My objection to verbal contortionism of this sort is not just aesthetic. Language should be a tool to clarify and illuminate, not mystify and obfuscate. In this particular case, the substance is difficult enough.

There are three essential points to Liska's argument: (1) American policy has followed an imperial course comparable, *mutatis mutandis*, to that of Rome and Britain; (2) the American Empire, however, was not acknowledged until that moment when it was explicitly rejected (erroneously, in Liska's view) on both moral and utilitarian grounds; and (3) the dissolution of the American Empire was owing primarily to internal factors. It was an act of self-abdication, neither heroic (like the Roman) nor honorable (like the British), but an act which, precisely because it was voluntary, may be reversible.

Liska believes that such a reversal—a reexpansion of the American Empire "in scope or institutional depth" (p. 336)—could (and should) result from central (Soviet) threats to American or Allied security, regional hegemonism by [Vietnam?] or fragmentation of [Iran?] middle-range powers, economic aggression by natural-resource monopolists [the Arabs?], or a renovation of the domestic political elite.

Superficially, at least, all this appears to be merely an erudite restatement of recent demands that America cast off its Vietnam-Watergate-induced posture of self-effacement and resume a vigorous pursuit of its national values and interests. And if it were only that general principle at issue, then Liska's exhortation would be unobjectionable. After all, the dangers and challenges are real.

*Career of Empire*, however, is intended to be much more than a simple protest against American passivity or impotence in international affairs. It is intended to demonstrate that the protection of American values and interests necessarily entailed *imperial* expansion, since "the flaws [of empire] were organically of a piece with the accomplishments" (p. 334), and that only by arresting the internal rot that has sapped its will-power [will-to-power?] can America reconstitute its empire and thus defend those values and interests.

The argument is complex, tortuous, and afflicted by an inordinate fondness for alliteration.



tive, tripartite classification schemes. Liska begins by identifying the three uniform bases or sources of expansion (security, sustenance, system-maintenance) and then describes the three phases of empire building (predation, preclusion, terminal expansion), each with its typical relationship to economic factors (p. 53). From the Roman and British experience, he also abstracts three phases of ruling-class transmutation (trusteeship, transactional control, trauma), the last of which is associated with imperial decline, caused by three general types of challenges (p. 92) and the loss, inevitably, of the three specific props of empire (p. 97).

The framework is inspired categorization, but it is so all-inclusive and indeterminate (internal and external causes, security and material motivations) that it adds little, in understanding the rise and fall of empires, to the explanatory or predictive power of such banal variables as capacity or a permissive international system. Its elasticity is obviously amenable to Liska's main thesis, but even with much stretching and bending it is hard put to accommodate the American eagle. For, as Liska himself is forced to concede, this empire is a very rare bird. It was hatched without the typical nurturing warmth of a security or economic crisis (pp. 109–12) and was reared by no typical parents, in the form of an imperial elite or ruling class (p. 169). It developed no typically formalized, centralized structure (p. 269) and was sustained by no typically imperial ethos or romanticism (p. 306).

Small wonder, then, that only the "domestic counterelites" were easily able to recognize this empire. After all, like Liska himself, they understood things in "systemic" and "structural" terms. In particular, they saw an American Empire—in Vietnam and elsewhere—as the ineluctable projection of monopoly capitalism at home. But who were these counterelites, responsible for social decay at home and dissolution of the empire abroad? Who were these "verbalizers of discontent" (p. 316) who cast "ideologically inspired aspersions on the conduct of both low-level warfare (My Lai) and highest-level political office (Watergate)" (p. 319)? Subversive conspirators? Traitors? Editors of the *Monthly Review*?

They were, writes Liska, "societal elites farthest removed from the productive process [my italics], such as college students and professional intellectuals and media men" (p. 319). They were "America's 'responsible' [i.e., liberal] political elites, intellectual dissenters, rationalizing interpreters, and impressionable public..." (p. 330). The phraseology and

world-view unavoidably recall other such denunciations, of "nattering nabobs of negativism" and an "effete corps of intellectual snobs." *Déjà vu*, but with one new twist: the very source of those denunciations, a "self-consciously conservative administration," is itself implicated in the crime of empire-liquidation, because it failed "to seize upon an opportunity to reach over the heads of a dispirited liberal political elite and professional middle class to the masses (including the conservative middle class) on the basis of the defense of empire in exchange for disciplining the mode of social change" (p. 349).

*Career of Empire* is not a conspiracy theory, but it does share the premise of some conspiracy theories—that America must be, at home and abroad, a seamless, imperial web. As such, it provides little guidance in establishing priorities and allocating appropriate resources—that is, in dealing with the real dangers, which Liska points out, of a world of infinite problems and finite power.

MARK HELLER

*Boston College*

**Patriot or Traitor: The Case of General Mihailovich—Proceedings and Report of the Commission of Inquiry of the Committee for a Fair Trial for Draja Mihailovich.** By David Martin. (Stanford, Calif.: Hoover Institution Press, 1978. Pp. xviii + 499. \$19.00.)

This book does not unravel the complex question of what happened in Yugoslavia during World War II; it does not pretend to. Its chief value, in my opinion, is in pointing to the inadequacies of works (e.g., Tomasevich, Auty, Barker, et al.) which have aspired to do so. The book consists of two parts: (1) a lengthy introduction, and (2) proceedings and report of the Commission of Inquiry, made up of jurists of national distinction, set up by some 70 prominent Americans who sought a fair trial for Mihailovich, after the Tito regime rejected the U.S. government request that American military personnel who had been in Yugoslavia during the war be allowed to give testimony at the trial.

The commission addressed many probing questions to some 20 Americans, some had been officially attached to Tito's Partisans and Mihailovich's Chetniks, while others were American airmen who had been forced to bail out over Yugoslavia and were rescued by the Chetniks. In some 300 pages of testimony, reproduced here, the commission, which had



been interested in evidence of alleged collaboration with the Germans, found no conflict in the testimony of these three classes of witnesses, none of whom had observed or learned of any collaboration, while several had witnessed military actions against the Germans.

While the preservation of the work of the commission in widely available form is to be applauded, I am convinced that the introduction is of far greater importance. For the most part, Martin concentrates on the period (the latter part of 1943) when the British were in the process of abandoning Mihailovich. He makes much use of the Operational Log, largely excerpts of messages to and from British liaison officers with the Mihailovich and Tito forces, maintained by the Special Operations Executive (SOE) in Cairo. In going over the Foreign Office and the prime minister's files on Yugoslavia for the same period, Martin was amazed at how little material from the log was reflected in those files. He produces significant information to indicate that the Yugoslav section of SOE Cairo was dominated by Communists, who deliberately did not pass on to London information favorable to Mihailovich. In addition, SOE prevented the transmission of certain messages from American officers with the Chetniks.

This slanting of intelligence, to which MI 6 also seems to have contributed, was compounded by the pro-Partisan broadcasts of the Yugoslav desk of the BBC, some of which attributed to the Partisans several major Chetnik actions against the Germans in the critical months of late 1943. Requests from British officers with Mihailovich for corrections were ignored, leaving Mihailovich to wonder even about the reliability of these officers, to say nothing of British policy.

The log, from which Martin reproduces (pp. 70–82) a chronology of Chetnik actions against the Germans during the period when they were being abandoned, has been publicly available to scholars since 1974, but has not been quoted from even by those who had access to it (e.g., Auty and Barker). The one-time head of the Southern European Department of the Foreign Office told Martin that had the detailed information on the Chetniks been available to the appropriate quarters in London, it certainly would have had an effect on the decision-making process in the Foreign Office.

Some will ask: would the outcome in Yugoslavia have been any different? No one can say with certainty. But Martin's work makes it evident that much more research is needed if we are to have a reasonably accurate picture of Mihailovich's movement and the policies of the

Western allies toward it.

ALEX N. DRAGNICH

*Vanderbilt University*

**International Tourism: A Political and Social Analysis.** By Harry G. Matthews. (Cambridge, Mass.: Schenkman Publishing Co., 1978. Pp. viii + 99. \$11.25.)

"The literature of tourism today is fragmented from a social science perspective because there has been little effort to integrate the work of researchers in different fields" (p. 87). The complaint still stands despite Harry Matthews' efforts at meshing a variety of explanatory perspectives in this slim volume. What Matthews does attempt—and he executes this task with commendable clarity—is to suggest the basic parameters for a political investigation of tourism.

The central argument of Matthews' tightly organized study appears to be that because of the *mass* character of international tourism, institutions are appearing that channel and organize this phenomenon in ways that are highly competitive but little understood in terms of their ultimate impact. This becomes a major political problem for both developed and developing societies because of the magnitude of international tourism. The effort to understand the possibilities and pitfalls of tourism is further complicated by the ideological prisms through which tourism is viewed. Still more dangerous is the fact that one of these ideological perspectives, that of corporate capitalism, has become the overwhelming paradigm for an analysis of tourism. "Those who question either the kind or volume of tourism seem to speak in hushed whispers" (p. 91).

Matthews supports his thesis with arresting detail, but his treatment is too brief to cover adequately many of its very intriguing themes. His pluralistic interest-group approach focuses on three levels of tourism politics: the "metropolitan" nation where the bulk of the market exists, the host country, and the spectrum of groups which attempt to influence policy regarding tourism.

Matthews pursues three central questions: who benefits from tourism, at what cost, and how adequately are benefits and costs measured? His answers are tentative but provocative. He finds that government encouragement of tourism constitutes a national subsidy to the travel industry only partially offset by such benefits as taxes and reduction of travel trade

deficits. For the host country he adds numerous caveats to such measures as gross foreign exchange earnings and the "multiplier effect," both of which purport to illustrate the importance of tourism to development. These neglect, he argues, the "import-prone" nature of tourism and the problems of accounting remittances when ownership and/or control are in foreign hands. To these costs he also adds the social costs which those marketing tourism seldom acknowledge and which even critics have difficulty estimating.

The picture which emerges may be affected by the fact that Matthews used the U.S. and Canada for his metropolitan nations and Barbados as his host country example. The Caribbean, particularly, has been the focus of many of the critiques of international tourism. However, research on tourism in Pakistan, the Philippines, and other Third-World countries also tends to support his concern that international tourism has become alarmingly homogeneous.

As Matthews illustrates, it threatens to lock the industry into a single marketing format, to depersonalize tourists and trivialize and despoil their destination while preventing the funds generated by tourism from developing the nation's potential or being a stabilizing influence in the society. The villain: an uncritical acceptance of "corporate capitalism" which has inadvertently developed maximally intrusive forms of tourism which immediately threaten tourism's developmental potential and ultimately threaten the industry itself.

The most intriguing social issue raised in the book is perhaps ultimately going to be the most important politically: does tourism need to be so intrusive? Do the fantasy aspects of tourism require the tourist to be so disruptive of the local economy and culture? For all the marketing information and all the social critiques of tourism available, this question is yet to be systematically addressed.

LINDA CLARK RICHTER

*Kansas State University*

**The Origins of the Cold War in Asia.** By Yonosuke Nagai and Akira Iriye. (New York: Columbia University Press, and Tokyo: University of Tokyo Press, 1977. Pp. ix + 448. \$25.00.)

This interesting volume is the result of a conference held in Kyoto in 1975, supported through a grant from the Japanese Ministry of Education and designed to bring together

American, Japanese, and British scholars to have another look at the development of the cold war, and to make use of recently available documents from the 1940s. It goes without saying that conference volumes are usually uneven, but in this case the unevenness itself provides food for thought. This is because the unevenness has an apparent relationship to the country of origin of each scholar. In general, the American scholars discuss the onset of the cold war within the comfortable confines of the mainstream versus revisionist framework, an American peculiarity if there ever was one; the British historian D. C. Watt provides a distinctive British view in his thoughtful introduction and his chapter on British policy, both of which occasionally take aim at American cold war revisionists yet document the serious divisions between U.S. and British policy that have provided so much grist for the revisionist mill. The Japanese scholars, with only one or two exceptions, would prefer to talk about anything *but* Japan's role in the events in the 1940s—including Poland (see Itō Takayuki's contribution), a curious inclusion in a book ostensibly about Asia. The reader concludes that there may well be a Japanese view on these events, but it won't be found between the covers of this book and instead is avoided at all costs.

The best articles in the book are those by Watt, Akira Iriye, George Kahin, Okabe Tsumi, Okonogi Masao, and Allen S. Whiting. Whiting says little about the *origins* of the cold war—a problem with many of these essays—but instead uses some of Mao Tse-tung's writings made available during the Cultural Revolution to shed light on Chinese views of the Korean War, the Quemoy crisis of 1958, nuclear weaponry and, most important, the Chinese view of the U.S. Far from seeking confrontation or possessing "worst case" expectations of American behavior, Whiting shows Mao to have held a balanced view of both the dangers and the potentialities of the Sino-American relationship, premised on Mao's belief that the U.S. was a declining power at least by the late 1950s if not earlier, leading to increasing disarray in the Western world. Toward the end of the article Whiting also sets Mao off from the "Gang of Four," showing through creative use of children's booklets that Chiang Ch'ing and others fostered a virulent anti-Americanism in such media that went far beyond Mao's own views.

Iriye's interesting article documents a set of consistent assumptions in American planning for Japan that defined the course of U.S. policy toward Japan throughout the decade of the 1940s. Through careful use of declassified documents, Iriye shows that there was no grand

reversal in U.S. policy toward East Asia, from an alliance with China to one with Japan; instead there was "an underlying pattern of continuity" stretching from 1941 to 1949. A corps of Japan specialists in the State Department spewed out reams of papers on postwar Japan from 1941-45, quickly adopting a consistent position, and eventually a consensus, that Japan would be liberalized rather than destroyed, administered under predominant American authority at war's end, such that no other power could make military use of the Japanese islands, and allowed to preserve the emperor system if it aided in the reconstruction of Japan. A reformed Japan would serve general American interests in East Asia far better than a dismantled or a revolutionized Japan. Thus, Iriye finds, "American unilateralism" defined the policy position of these experts from the beginning, and eventually defined the postwar policy toward Japan.

Okabe's essay addresses "changing Chinese Communist perceptions" of international relations in the late 1940s, finding three basic views: an image of cooperation among the Allied powers, held from the war years until mid-1946; an image of the "intermediate zone," a vast region which U.S. imperialism attempted to exploit, continuing to the end of 1947; and a third image of tight East-West bipolar confrontation ensuing thereafter. This essay, seemingly modeled on earlier studies by Iriye (e.g., *Across the Pacific*) which emphasize mutual images and perceptions, is at its best when discussing Chinese theories of the "intermediate zone" and the policy of self-reliance, both based on horizontal polarization of East and West, and vertical polarization between big powers and small nations. Although this article covers familiar ground, Okabe argues shrewdly and exploits a variety of rarely used Chinese sources.

Okonogi Masao's contribution views the Korean War as "basically a domestic conflict over the legitimacy of political power" between two Korean sides, a conflict that both drew in the big powers, and one that the big powers internationalized. Thus, Okonogi argues forcefully, "domestic and international elements were intertwined" in this war. He then proceeds to document his thesis using materials in several languages; the article is fascinating, and tantalizing in its unfortunate brevity.

Robert M. Slusser and John Lewis Gaddis also examine the Korean War and U.S. and Soviet policies toward Korea; both articles are as interesting for their flaws as for their strong points. Slusser treats a difficult and neglected topic: Stalin's goals in Korea. Unfortunately, he

is forced to argue by reference to what Stalin did *not* say about his goals, postulating "a chain of pregnant Soviet silences," "a series of meaningful silences" on Stalin's part. Whether it is a silent majority or a mute Joe Stalin, political goals are hard to document in the absence of statement. One goal that Stalin was pregnant with, Slusser says, was a warm-water port in Korea; yet no such baby emerged from the Soviet-occupied womb in northern Korea. Perhaps it was just a glimmer in Uncle Joe's eye? Gaddis, like many other contributors, takes on the revisionists; his presentation of American policy toward Korea in the late 1940s is based for the most part on documents coming out of Washington. Yet without looking at the now-available record of the American occupation in Korea (1945-48) one cannot understand how Washington's ostensible policy for Korea was transformed into something quite different by those on the scene in Seoul. From the Seoul side of things the revisionist arguments appear much more cogent. In reading Gaddis' account one wishes that Korea policy had in fact been an easy matter of Washington enunciating the policy and watching its faithful implementation, instead of continually seeing the best plans done under by Americans and their Korean allies in South Korea. Okonogi is much better on the dialectic between Washington's wishes and the reality of occupation policies.

These are the articles that held my interest. Many of the others—especially those by Nagai, LaFeber, Aruga, Ito, Nakajima, and Yamamoto—either offer little that is new or wander far (often very far) from the ostensible subject of the book. Still, the book will hold the attention of specialists on the cold war in Asia, will be useful in classroom assignments, and all in all constitutes a welcome contribution to the field.

BRUCE CUMINGS

*University of Washington*

**Armed Struggle in Palestine: A Political-Military Analysis.** By Bard E. O'Neill. (Boulder, Colo.: Westview Press, 1978. Pp. xiii + 320. \$20.00.)

The main virtue of this rather uneven study of the Palestinian "war" against Israel is the military perspective which Bard O'Neill brings to bear on a subject which is usually approached from historical or partisan positions. O'Neill clearly admires Israel's counter-insur-

gency efficiency and shows little enthusiasm for the goals or efforts of the Palestine Liberation Organization (PLO), but for the most part he does not allow these feelings to divert him from his course: a methodical assessment of the conflict in terms of a framework of important factors—popular support, organization, unity, external support, the environment, and the government's counter-insurgency role.

The military part of this "political-military" analysis is stronger than the political. O'Neill first sets forth a concise theoretical statement about insurgency in general, drawing upon the major writers, including Mao Tse-tung, Che Guevara, Samuel Huntington, Ted Robert Gurr, and others. Then, after a fairly superficial exploration of "the political context," he moves into the substance of the study by examining Israeli counter-insurgency efforts against the Palestinian *fida'iyyin*. Unfortunately, he accepts too uncritically Israel's description of the occupation as "liberal" and tends to give Israel the benefit of the doubt when evaluating the persistent charges of torture and other excesses on the West Bank and Gaza. But he does succeed in providing a sober and straightforward account of Israel's success in limiting internal organized opposition and containing externally based guerrilla and terrorist activity. O'Neill's comments about the Israeli government's use of "sound counterinsurgency principles" and his discussion of the physical environment, which is on the whole unfavorable to guerrilla warfare, are quite illuminating; and he shows clearly the folly of applying "general" theories of insurgency developed for jungles or rugged, wide-open spaces to the terrain of Palestine.

Perhaps the most hard-hitting part of the book is chapter 5, which deals with the lack of organization and cohesion within the PLO and its main guerrilla organizations. The critique is devastating and should make valuable if unpleasant reading for PLO officials. O'Neill recounts the ideological disagreements, the personal rivalries, and the complicating inter-Arab state interests that accounted for the poorly executed, badly focused and short-lived heyday of Palestinian military activity against Israel from 1968 through Black September 1970, and its decline after that debacle in Jordan to sporadic and senseless acts of violence thousands of miles from Israel. The author succeeds in revealing the enormous limitations affecting any Palestinian military option through his cool description of the failure of the PLO's armed struggle campaign in the late sixties and early seventies.

On the political side, O'Neill concludes that

the PLO also largely failed to mobilize popular support among Palestinians, especially in the occupied territories. Only in the refugee camps and among the intellectuals does he find evidence of success in this crucial requirement for effective armed struggle. He also finds that the external support provided by various Arab states to be limited at best. Here I think O'Neill has underestimated the PLO's political capabilities. Most observers of Arab affairs believe that the PLO has won the allegiance and support of Palestinians everywhere (the occupied territories included), particularly since 1974. The non-military infrastructure of the PLO—to which O'Neill devotes only a few paragraphs—is extensive and significant, with its popular organizations, professional societies, factories and financial institutions, medical services, social security program for the families of fighters, and not least its quasi-parliamentary National Council. Perhaps because the study's sources do not reveal direct access to the complex Arab political environment, it tends to underestimate the salience of the Palestinian cause both to Palestinians and other Arabs. Another reason for this misperception is the time frame of the analysis, which deals mainly with the 1967–73 period. Lacking on the one hand a sense of the long history of the conflict and on the other a close analysis of developments since 1973, O'Neill focuses on those years of almost anarchic growth of the movement which were followed so quickly by the disastrous collision with Jordan and tension with other Arab states. Thus he tends to miss the underlying continuity of support which keeps the PLO afloat despite its strategic muddleheadedness and military inefficiency. Indeed, today the PLO is richer, stronger, better organized, and more popular than it was during its explosive adolescence in 1968–70.

But the PLO's prospects for effective armed struggle remain marginal, as O'Neill correctly indicates. In light of this impotence, yet recognizing that it retains at least limited political clout, he concludes his study with the suggestion that a PLO-administered Palestine entity might put to rest the political tension in the Middle East without threatening the security of Israel. This suggestion, and the logic behind it, is provocative, and it would be even more compelling had O'Neill given due weight to the political importance of the PLO. Despite this flaw and others such as repetitiveness, reification of terms like "ideology," and insufficient Arabic sources, this study effectively highlights the Palestinian military dilemma. But it adds considerably less than one might have expected to the excellent earlier works by John Cooley

(*Green March, Black September*, 1973) and by William Quandt, Fuad Jabber, and Ann Lesch (*The Politics of Palestinian Nationalism*, 1973).

MICHAEL C. HUDSON

Georgetown University

#### Decision Making in the European Community.

By Christoph Sasse, Edouard Poulet, David Coombes, and Gérard Deprez. (New York: Praeger, 1977. Pp. xv + 352. \$29.95.)

This book belies its title. It is not an analysis of decision-making processes in the European Community, but rather a combination of three descriptive and prescriptive pieces on European Community institutions. The three sections are linked by a central purpose: "to analyze the underlying causes of the deficiencies in the policy-making process" (p. 138). In pursuit of this diagnostic task, the authors first devote a chapter or two in each of the three sections to a short and, for the most part, derivative description of the main institutions involved in European Community affairs. They then go on to offer "practical suggestions for achieving increased efficiency" (p. 87).

The opening section by Christoph Sasse on the role of national governments and parliaments in Community affairs is somewhat uneven. The Federal Republic, for example, gets a great deal more attention than the other member states. The materials in the chapter, mostly written for earlier publication in Germany, could also use some updating. In sum, there is only a little here that goes beyond the excellent monographs in the PEP/Chatham House series by Martin Niblock (*The EEC; National Parliaments in Community Decision Making*, London: PEP/Chatham House, 1971) and Helen Wallace (*National Governments and the European Communities*, London: PEP/Chatham House, 1973).

The second section, by Edouard Poulet and Gerard Deprez, departs from the public law approach of Sasse and examines the structure and functioning of the European Commission from an organizational sociology approach. Despite the recognition at the outset that individual institutions of the Community must be viewed as "constituent elements in an interacting system" (p. 133) and the "impossibility of analyzing the Community system by reference, explicit or implicit, to any given institutional model" (p. 134), Poulet and Deprez do not really pursue this line of thinking. Quite the contrary: there is almost no reference to process and environment or any

effort to develop a description of a dynamic institution at work.

David Coombes' reflective chapters on the role of national parliaments and the European Parliament, centered on what he believes to be the crisis of legitimacy in the European Community, provide interesting and constructive insights into legislative processes. Importantly, he points up the Community's failure to acquire political roots of its own, a shortcoming that in all probability will not be made up even by the introduction of direct elections to the European Parliament in June 1979. Coombes' chapters provide a more satisfying conclusion to a book that is for the most part rather short on analysis and insights.

It is disappointing that the authors of this long collection made so little of their opportunities. Not only do they fail to come to grips with the process of decision making in the European Community, but also even their descriptions of the institutional machinery are static and overly legalistic. With only lip service paid to the organic and ever-changing nature of political processes, it is little wonder that the collection has withstood the test of time rather poorly.

Since the prescription stands or falls by the quality and relevance of the description, the authors' remedies for the alleged ills of the Community system are also unconvincing. If indeed, as Sasse suggests, "The European Community is suffering from a discrepancy between achievement and expectation" (p. 113), we cannot help asking the question: whose expectation? It is very doubtful that it is public expectations, as the authors appear to indicate. This of course brings us back to an issue which the authors barely address: the sources of demands and constraints placed on Community institutions.

A similar failure to address crucial questions allows Poulet and Deprez, for example, to suggest that what is necessary for the Commission is that "the decisions taken should correspond to its proposals" (p. 131). Surely one of the most fascinating and unusual features of Community decision making is the very fact that proposals, carrot-like, stand a good way ahead of decisions. Just as Commission memoranda have fulfilled this function since the early 1960s, so also summit meeting communiqués now do the same thing.

Perhaps some of these misperceptions stem from the time-worn habit, on the part of scholars and practitioners on both sides of the Atlantic, of assuming that there is a single identifiable phenomenon called "European integration" and then, if such a phenomenon is to

be located, that some inherent good is to be found in it. Poulet and Deprez make no bones about this in their prescriptions for the Commission: "It will still need an . . . overall picture of the form of organization that seems to it to be best suited to its essential mission, namely the preparation of Community policies as part of a global and coherent view of the Community integration process" (p. 178). No one is faulting the idealists, but in this world of practical politics and economics, of small improvements and major setbacks, the holy grail of integration and supranationalism emerges as something of an anachronism.

Observers of the European Community now have at their disposal a whole library of informative and useful studies of its institutions, old and new. What is sorely lacking are sensitive and perceptive analyses of these institutions in action in the day-to-day world of European and national politics.

GLEND A. G. ROSENTHAL

*Columbia University*

**Africa Reports on the Nigerian Crisis: News, Attitudes and Background Information; A Study of Press Performance, Government Attitude to Biafra and Ethno-Political Integration.** By Henryka Schabowska and Ulf Himmelstrand. (Uppsala: Scandinavian Institute of African Studies, 1978. Pp. 161. Skr. 75.)

The lengthy and complex title of this study indicates its importance, conclusions, and deficiencies. The effort to address the previously unexamined role of the African press in one of the continent's great conflicts is welcome, particularly in the context of the present battle within UNESCO over the role of the press in the Third World. The sponsoring institute has generally offered monographs of descriptive and theoretical distinction. Unfortunately, this is not one of them.

The work comprises three parts: two chapters on the general approach to mass media in developing countries and the specific aims of this research; two chapters on the history, tradition, and structure of African press and news agencies; and two chapters on attitudes of African states to the Nigerian civil war—as seen in the press of Ghana, Tanzania, Senegal, Ivory Coast, and Guinea, selected to represent pro-West and pro-East, anglophone and francophone, and pro-federation and pro-Biafran states. Eleven publications were analyzed during four stages of the conflict: January-Septem-

ber 1967, Aburi and secession; May-October 1968, peace talks; May-September 1969, Ahiara Declaration and stalemate; and December-February 1969–1970, war's end. The hypothesis, claimed to be confirmed, is that "the views of African governments were reflected" in the press, and these in turn were shaped by "the degree of ethno-political national integration" (pp. 10, 11). These two propositions and their connection may well be valid; but the verdict is "not proven," owing to serious—and in my opinion, fatal—flaws in theoretical underpinnings, research design and techniques, and presentation.

The initial ventures into theory about mass media and the developing world are a jumble of ideas, diagrams, and models, most of which are then used unconvincingly—or not at all. Vital factors in African policy towards Biafra, such as the relationship of various governments to France and to DeGaulle, or the regional competition between Ivory Coast and Nigeria, are never mentioned. The choice of ethno-political integration as the controlling factor in policy is thus not justified; assuming that it were, other states (such as Sierra Leone) should have been considered as well.

The research itself was at best careless, in both general and Africanist terms. There are great gaps in the materials, attributed to the inability to get complete files, which the authors claim—unconvincingly—do not damage the findings (pp. 66–68). If the needed materials were not obtainable, the study should have been terminated or redesigned. Other misdirected choices or simple errors of fact further undermine even descriptive utility (for example: the use of the *Ghanaian Times*, always a state-owned paper despite the identification to the contrary, rather than the *Daily Graphic*, with by far the largest circulation in Ghana, and the overemphasis on the importance of the Catholic-oriented *Afrique Nouvelle* in the overwhelmingly Muslim society of Senegal). The ability of coders whose first language is neither English nor French, and who are (apparently) not Africanists, to fully understand the import and significance of reports and editorials in the African press can be questioned. The coding scheme itself is not presented and must be obtained by writing to the authors! (p. 68).

These shortcomings of content are matched by a didactic and ponderous style. Documentation is thin, and many footnotes (e.g., nn. 3 and 6 of chapter 2) bear no obvious relevance to the text. The results of translation from the original Polish of the principal author should have been more carefully checked. In a work of greater theoretical strength, such unevenness would

perhaps be of lesser importance; but here, particularly as the subject itself is the written word, it is unforgiveable. Given the price of this book, the student of Africa and of the media would be advised to await some other study.

MARSHALL CARTER

Ohio University

**Anti-Apartheid: Transnational Conflict and Western Policy in the Liberation of South Africa.** By George W. Shepherd, Jr. (Westport, Conn.: Greenwood Press, 1977. Pp. xii + 246. \$15.95.)

During the past two decades, the international political system has been significantly transformed by the emergence of myriad transnational actors. Most prominent among them are, of course, the multinational corporations, whose role and impact have been the object of study and controversy by both economists and political scientists. Nonetheless, another category of transnational actor has also altered the dynamics of the international political system—the non-governmental or voluntary organization that has extended its influence into the national liberation struggles and the struggle for the promotion and protection of internationally recognized human rights standards. This transnational actor—the NGO—has, however, not yet attracted the serious analysis that it warrants. For this reason, George Shepherd's *Anti-Apartheid*, with its focus on "the conflict-oriented transnational human-rights actors working against apartheid" (p. 12), is a noteworthy and important contribution to international relations theory, in addition to further illuminating the anti-apartheid struggle and the forces sustaining racism in southern Africa.

Shepherd perceives the anti-apartheid struggle as a continuation of the nineteenth-century abolitionist movement, in that apartheid is considered a form of slavery (forced labor). The main resistance to the abolitionist movement is "the Atlantic system," a concept drawn from regional and dependency theory (p. 11), whose core powers are the industrialized Western nations (the United States, the United Kingdom, Germany and France). South Africa is seen as a semi-peripheral power of the system, providing essential services of the core and helping the Atlantic capitalist powers penetrate economically the rest of the Southern African periphery. To bring about the defeat of apartheid, it is necessary to destroy this symbiotic relationship.

It is in this context that Shepherd examines the role and the effectiveness of the numerous NGOs that comprise the anti-apartheid movement, with particular emphasis on American and British organizations. In the course of his analysis, Shepherd provides a wealth of descriptive material on the origins and nature of these transnational actors—an invaluable contribution in and of itself; however, the concern of his study is the policy results of these human rights NGOs, not the organizations themselves. Specifically, he examines the extent to which NGOs contribute to the growth of transnational abolition pressures on apartheid—through education, support aid, and the strength of the liberation and majority movements. Chapter 2 details the manner in which the human rights NGOs have been able to assist the United Nations process of identifying injustice and recognizing the legitimate representatives of the powerless people; chapter 3 traces the development of the human rights movement from an initial protest period of legalistic focus on reform of law, through a period of direct action, to the current stage of direct support for the liberation struggle; chapter 4 analyzes the trends in both the U.S. and the U.K. among black NGOs, away from assimilation and co-optation and toward black consciousness and multiracial cooperation; and chapters 5 through 7 examine the campaigns for an arms embargo, assistance to liberation movements, and transnational sanctions (including the bank campaign, the sports boycott, and economic boycott) that have been waged in recent years.

Throughout his analysis, Shepherd is conscious of—and seeks to illuminate—the splits and factions within the anti-apartheid movement, particularly between the "gradualist" strategy, in the direction of domestic reforms with minimal external pressure, and the "abolitionist" strategy, in favor of disengagement and support for liberation. He, himself, is convinced—and convincingly argues—that only the latter holds hope for eroding the structure of apartheid:

One of the assumptions . . . of this study is that fundamental solutions to racial conflict emerge out of racial class changes rather than out of individual attitudinal changes. A change in attitude may facilitate change, but it cannot substitute for changes in the power position of groups (p. 204).

Under the enormous pressures for changes generated by the external linkages which have been forged by NGOs with the internal racial complex in South Africa, the South African government has made limited adaptations: the



sacrificing of "petty apartheid," the initiation of "detente" and "dialogue" with Black African states; the "homelands" policy; and some concessions to the Black Consciousness and trade-union movement, followed by even more repression. But, none of this has altered the basic stratification. Only revolution will bring down the racist structure. Moreover, states Shepherd, "The revolutionary option is real and is being facilitated by the abolitionist movement abroad. It is time that more people saw clearly the conflicts between the abolitionists and the gradualists and began to make some hard choices" (p. 226).

Shepherd's conclusions suggest that the NGOs have not only played a vital role in bringing the southern African situation to the point where "the revolutionary option is real," but that in certain respects, "they play a formative role in world politics" that is occasionally "more determinative than that of major powers" (p. 13):

Western Governments may well continue to support the apartheid superstructure. But its collapse has already begun and will continue until the persistent nongovernmental actors, along with the liberation movements, secure fundamental change (p. 229).

LAURIE S. WISEBERG

### *Human Rights Internet*

**The Politics of International Economic Relations.** By Joan Edelman Spero. (New York: St. Martin's Press, 1977. Pp. vii + 326. \$12.95, cloth; \$5.95, paper.)

The appearance of this volume is to be welcomed enthusiastically by a discipline that is trying to make sense of the growing prominence of economic issues in American foreign policy and international politics. Theoretically, it reflects a growing disciplinary tendency to break the artificial bonds imposed by "realism" on international politics with its assumptions of a single issue-system governed by an elite of national governments preoccupied by the ebb and flow of "power" narrowly defined. Spero restores the inherent connection between international economics and politics, artificially severed during the postwar dominance of international relations scholarship by realism; she argues persuasively that *in reality* the uncoupling of the two spheres was never valid. Along with several recent works—notably, Keohane and Nye, *Power and Interdependence* (1977) and Blake and Walters, *The Politics of Global Economic Relations* (1976)—this book provides a concise appreciation of what is meant by

fashionable but ill-used concepts like "interdependence" both among actors and among issues.

Spero has organized her introduction, ten chapters and conclusion around three issue-areas—a Western system, a North-South system and an East-West system. She identifies within each the key issues and their peculiarities, the key participant actors and their roles, how these issues and the systems they constitute have been organized and administered. Treated historically, each issue (i.e., "international monetary management" within the Western system or "the use of aid" within the North-South system) is presented in sufficient technical depth to avoid even the slightest hint of superficiality yet so as to strengthen inexorably but subtly the core argument that "international economic relations, in and of themselves, constitute political relations" (p. 9). The material is presented so that no single issue or system appears to function in isolation from the others. We are constantly reawakened to the links among them, the changes in those links and their interplay with conventional concerns of "peace and power." The discussion of each issue manages to provide both rich description and compelling theoretical insight, so that there is something of value in every section for professional economists interested in the relevance of their concerns to political outcomes and political scientists seeking to acquire a grasp of the intricacies of economic phenomena and to integrate them.

If Spero's facility for explaining the nuts and bolts of economic questions will delight political scientists and students, so too will her capacity for explaining the political nuts and bolts of these questions. The impact of bureaucratic contention, both within and across national frontiers, is elaborated, and, by inference, we are alerted to the unhappy consequences of what Steinbruner calls "cybernetic" decision making. In addition, the permanent bond between domestic and international politics (and economics) and its reciprocal consequences are consistently dealt with and explained.

Among the book's most important themes are those concerning the decline of American world influence since the period of virtual politico-economic hegemony immediately after World War II, the shifting nature of global cleavages since this period and the growing enfeeblement of "sovereign" actors trying to cope with international economics by self-help. The first is highlighted in the Western economic system, and in the process Spero helps to clarify the almost linear decline of American



political influence that has apparently bewildered many students and practitioners of international affairs since Vietnam and the Middle East oil embargo. The second theme is woven with the first as we are shown how the American decline and the growing challenge of Western Europe and Japan, mirrored by the status of the dollar and contention over markets, have ended America's politico-economic "magnanimity" and have produced increasing friction with friends and allies. Such growing contention and the tasks attendant upon it are elaborated in the author's descriptions of the oil question (to the point where allegations are presented concerning American-Arab collusion in the initiation of OPEC and the raising of oil prices), the Nixon "shocks" (including pressure on Japan and unilateral devaluation), and the growing competition between the U.S. and other multinationals for markets in the Soviet bloc. This last issue also serves to reveal the growing gap between the perceived interests of American business and government. Whether the subject is the role of multinationals, the problem of cartels or the dilemma of petrodollars, the theme that national autonomy is increasingly and inexorably being undermined, most dramatically in the case of the United States, is returned to again and again.

In sum, this is a volume to elate teachers seeking a text to explain the "new" phenomena yet which serves as an excellent first source for scholars who desire a broadly informative, well-written and theoretically valuable framework for reintegrating international politics and economics.

RICHARD W. MANSBACH

*Rutgers University*

**Analyzing Soviet Strategic Arms Decisions.** By Karl F. Spielmann. (Boulder, Colo.: Westview Press, 1978. Pp. xv + 184. \$16.00.)

This is not a book about Soviet strategic power, or even about Soviet decision making on strategic arms. As the title says, it is a book about *analyzing* Soviet arms decisions. Readers who finish it may know not much more about Soviet strategic power or about Soviet strategic arms decisions than when they began it. They will have learned instead that those who analyze Soviet strategic arms decisions—presumably not the readers themselves: there are not many such analysts and Spielmann assumes that they must work inside the government where the requisite classified information is—should not attempt to analyze them at a single go.

"Multiple approaches," as advocated by Graham Allison (see below), are needed. The analyst should attack the subject three times, using: a rational strategic-actor approach; a pluralistic decision-making approach; and a third approach which postulates that the top Soviet leaders in making strategic arms decisions take account of diverse factors, not just strategic considerations.

Why three *distinct* approaches instead of a combined approach? Because a combined approach would "lump a variety of assumptions together within a single analytic framework" (p. 11). The distinctiveness of discrete approaches "is likely to be compromised by merely combining them. It would be difficult to prevent the new unified theory" from being biased to favor the view that Soviet strategic decision making is rational or, alternatively, that it is determined by bureaucratic struggle. Spielmann acknowledges that in a combined approach diverse "factors may well be taken into account, but only sufficiently so to assure skeptics that the analyst had been broad-minded" (p. 11). Spielmann is so strongly persuaded himself of the difficulties of a unified approach that he hardly pauses to persuade the reader that they are indeed insuperable.

Spielmann does not promise much in the way of positive knowledge to analysts who follow his prescriptions, and still less to policy makers who employ analysts to help them understand the Soviet strategic threat so that they can decide how the United States ought to respond to it. Preoccupied as he is with the advancement of the methodology of analyzing Soviet strategic decisions, Spielmann neglects to show how the multiple approach would provide United States policy makers with better judgments about the motives and intentions that underlie Soviet strategic programs. In fact, he acknowledges that the policy maker is not interested in receiving "different answers to the same question, and it is more difficult to make an overall judgment when multiple approaches are used" (p. 10). The chief virtue claimed for the multiple approach to Soviet strategic decisions, it would appear, is that it compels analysts to confront their assumptions and take account of relevant information (p. 143)—that is to say, the method *forces* them to do what scholarship merely *requires*.

Despite the dubious fruits of multiple approaches, Spielmann is not daunted. He concludes with a call for case studies of an array of Soviet strategic arms decisions, admittedly "an enormous undertaking" (p. 142). What are the prospects? As yet, apparently no one has

completed a study of a Soviet strategic arms decision employing a multiple approach. Spielmann's mentor, Graham Allison, who employed the method in a book-length work (*Essence of Decision: Explaining the Cuban Missile Crisis* [Boston: Little, Brown, 1971]), acknowledges that he had only illustrated the use of multiple approaches and had not provided a confident account of what had happened (see Allison, pp. viii and ix). Spielmann himself, after a mere 25 pages (around one-sixth of the book) devoted to the first-generation Soviet ICBM program (SS-6), concludes that the task of analyzing the SS-6 decisions along multi-approach lines basically remains to be done (p. 134).

While the method Spielmann advocates here is formalistic and, perhaps, finally barren, he himself is superior to that method. He has published serious substantive articles on the Soviet defense establishment and in this work also makes in passing important points. This is particularly true when he discusses "the pluralistic decision-making approach," a combination of Allison's organizational and bureaucratic models. Spielmann argues persuasively that large interest groups in the USSR—e.g., the military *services*—while they might be politically effective if they were unified actors, actually are heterogeneous and subject to powerful intra-service rivalries; component groups within the services, while they may pursue their interests united and with fervor, do not have great political weight. Spielmann also rightly emphasizes that the Soviet leadership may take account of various group interests for reasons of its own, not necessarily because of group pressures. I, at any rate, am unimpressed by the sanctions that might be available to "pressure groups" in the USSR to compel the leadership to adopt decisions they otherwise would not.

MYRON RUSH

Cornell University

**Sanctions: The Case of Rhodesia.** By Harry R. Strack. (Syracuse, N.Y.: Syracuse University Press, 1978. Pp. xvii + 296. \$15.00.)

Fourteen years have passed since the white minority government of Rhodesia unilaterally declared its independence from the United Kingdom, its metropolitan power. Like the League of Nations sanctions against Italy in the Ethiopian case, the United Nations action imposing enforcement measures under Article 41 of the Charter in order to bring about the downfall of the Smith regime has provided

political scientists with an excellent case study of the effectiveness of sanctions as a tool of the collective security system and an instrument of foreign policy in general. A review of the literature and U.N. resolutions on the subject allows the conclusion that the enforcement measures have failed to bring about a solution of the Rhodesian problem acceptable to the organization that had ordered them. Harry R. Strack tends to agree with the view that the Rhodesian sanctions have not been sufficient to isolate Rhodesia from international relations so as to provoke an internal political change satisfactory to the party imposing the sanctions. Since Strack's book covers the events as of the end of 1977, one might add that the "internal settlement" of 1978 does not seem to be considered by the United Nations as a development restoring international peace and security and thereby warranting lifting the sanctions.

Strack's primary purpose is not to analyze the effect of the sanctions on Rhodesia's economy, the approach taken by most commentators, but to identify, explain, and determine how and under what circumstances the Smith regime has been able to avoid isolation from the rest of the international system, to maintain and even develop its international contacts, and to survive as a de facto state though legally not recognized even by South Africa. Strack approaches the problem in a most comprehensive way going beyond its economic aspect, although obviously the economic relations occupy about one-third of the text.

Structurally the eight chapters of the book (leaving aside the introduction) can be divided into three parts. Chapters 1 and 2 respectively deal with Rhodesia's international status before the Unilateral Declaration of Independence and the nature and purposes of the sanctions. Against the background of the theory of sanctions Strack traces the application of the enforcement measures against Rhodesia and concludes that, without necessarily being a useful device to induce compliance, sanctions may even be dysfunctional by enhancing solidarity in the target state and forcing it to diversify its economy. Strack judges the gradual escalation of the Rhodesian sanctions to be unsound since it gave the Smith regime time to make necessary adjustments. In addition, the confusion prevailing as to the purposes and termination of the sanctions had also affected the adherence to sanctions policy.

The main part of the study meticulously describes Rhodesia's international relations in political and diplomatic matters, including for-

mal and informal links; in economics, the least successful from the enforcement point of view; in communication and transportation; in tourism, labor, and migration; and in social relations, including sport (the most diligently applied sanctions!), education, broadcasting, and general entertainment. Despite obvious difficulties, such as the secrecy surrounding the methods of circumventing the sanctions, Strack, who conducted field research in the area, offers the reader many fascinating details on the Rhodesian government's methods of maintaining sufficient international contacts to ensure the survival of the regime. Strack analyzes the efficacy of the Rhodesian sanctions in all the aspects of international relations within the framework of three perspectives or levels of analysis: global, regional (Southern African), and national. Utilizing the concept of transnational relations, he proves with the example of Rhodesia how difficult it is to isolate a country from the rest of the international system in which so much of intersocietal intercourse involving all kinds of transactions occurs outside of government control. Moreover, the governments are sometimes unable or, even worse, unwilling to control these transactions and actors, such as transnational corporations, banks, trading companies, brokers, and shipping lines. Strack finds this phenomenon particularly conspicuous in the global arena. From the regional point of view Rhodesia's environment has reduced its susceptibility to isolation. Even after the decolonization of Portugal, the states of Southern Africa are so tied together by transportation, economic, and communication linkages (Malawi, Zambia, for example) that the effectiveness of the sanctions against Rhodesia must be reduced if they cannot be applied to the whole region. At the national level of analysis Strack approaches the sanctions from the perspective of the target state's policies designed to counteract the sanctions and the perspective of other national and transnational actors engaged in relations with Rhodesia. A case study of the Byrd Amendment in chapter 4 is of particular interest to the American reader.

The conclusions (chapter 8) sum up the evidence recorded in the main body of the book. Strack's general conclusion is that, leaving aside their symbolic and ideological impact, the United Nations measures have failed to isolate Rhodesia, which casts some doubt upon the efficacy of sanctions in any future application by the U.N. or some other, perhaps regional, organization. Strack believes that the fact that Smith was finally persuaded to accept the principle of majority rule is due primarily to the coup in Portugal in 1974 and decoloniza-

tion of Portuguese Africa.

Notes, a bibliography (only of English-language sources), an index, a map of the Southern African railroad network, and 15 tables illustrate and complement the thorough analysis in the text.

Strack's well-researched and comprehensive book must be regarded as the definitive study of Rhodesia's efforts to circumvent the effects of the United Nations sanctions. Moreover, it goes beyond being a mere case study by raising within its conceptual framework broader issues, such as interdependence in today's world, the role of transnational actors, and the ability of an international organization to enforce its decisions. In this sense it should interest not only students of international organization or African affairs but also international relations scholars analyzing the fundamental structure of the present international system.

BOLESŁAW A. BOCZEK

*Kent State University*

**The "Dollar Drain" and American Forces in Germany: Managing the Political Economics of Alliance.** By Gregory F. Treverton. (Athens: Ohio University Press, 1978. Pp. xvi + 226. \$12.00.)

With the obvious exception of the Soviet Union, no country in the world is more crucial to the successful pursuit of American foreign policy goals than West Germany. But after 30 years of extraordinarily intimate collaboration, the two allies find themselves at a crossroads in their relationship. On the one side, traditional elements of American dominance in the Association remain—on the U.S. commitment to West Berlin, America's broader willingness to defend West Germany from potential Soviet aggression, and, even more generally, Bonn's expectation that Washington will continue to define the larger purposes of the Western Alliance. On the other side, however, the American economic and energy dilemma places West Germany in a new position of partial—but not full—equality within the bilateral relationship. Indeed, West German leaders publicly maintain that their country is not in the position to assume "joint" leadership of the Western Alliance with the United States; even a joint entente with France is considered premature and politically unwise from the vantage point of West Germany's leaders. Thirty years of political development have nonetheless made the Federal Republic of Germany a latent political force with which the United States must realistically deal.

Gregory F. Treverton's study of German-American conflict over the Troop Offset issue in 1966-1967 sheds interesting light on what, in retrospect, now seems a crucial stage in the political emergence of West Germany. This arrangement called for the Bonn government to purchase an agreed amount of U.S. military equipment in exchange for the stationing of U.S. troops in the Federal Republic. In view of their geostrategic positions, Treverton hardly surprises the reader with the assertion that the two allies had rather different views on the offset issue.

On the American side, the Johnson administration apparently believed that Washington was in the position to exert effective pressure on the soon-to-be-deposed Erhard government. As Treverton insightfully observes, Washington's strategy toward Bonn was simultaneously shrewd and straightforward: "In effect," concludes the author, "the United States used a domain where it perceived itself to be relatively strong—military security—to exact advantage in an area where it felt relatively weak—international finance. Germans (and other Europeans) were presumed to care more about the presence of the troops than the United States..." (p. 198). A face-saving agreement was eventually reached with the successor government of Kurt-Georg Kiesinger, and in this very limited sense the Americans got their way. But over the longer term, the confrontation over troop-offset undoubtedly emboldened future German leaders to take a tougher, and more exclusively national, line in dealing with Washington. But why would the Germans even wish to argue with the U.S. over the issue of American forces in their country?

On the German side, Treverton maintains that Bonn viewed American troops in the Federal Republic from two different vantage points. True, an American military presence on German soil continued to enjoy bipartisan political support within the Federal Republic throughout the 1960s. Yet German political

leaders were also becoming annoyed with American threats to withdraw U.S. troops in the absence of a German willingness to pay for the stationing of American forces in their country. From Bonn's vantage point, this position was based on a very limited American conception of monetary interest. Had the global Soviet threat receded to the point where only the Germans had something to fear?

Within the American and German foreign policy establishments, Treverton describes and analyzes a fascinating series of bureaucratic struggles. These struggles not only ended up pitting the two national bureaucracies against each other, but different parts of respective government agencies also found themselves in conflict with each other as well. As a case study in the politics of alliance management, Treverton's work reaffirms earlier findings (principally those of Neustadt and Allison) that national governments are anything but monolithic entities.

The "Dollar Drain" has one basic flaw, despite its overall value for students of alliance politics: far too little attention is given to the broader strategic environment in which this important conflict is played out. What impact did DeGaulle's withdrawal from NATO have on the Washington-Bonn association? Did the delicate strategic balance between Moscow and Washington influence the perceptions of American and German negotiators? In fairness to Treverton, these and other considerations are mentioned in his book; but the author could have gone further in exploring how one specific case study related itself to these larger issues. Most case studies are enhanced by the addition of larger strategic factors, and Treverton's study is hardly an exception to that rule. Having said this, however, I must add that no one interested in the present course of German-American relations should ignore this book.

JOHN STARRELS

*Washington, D.C.*

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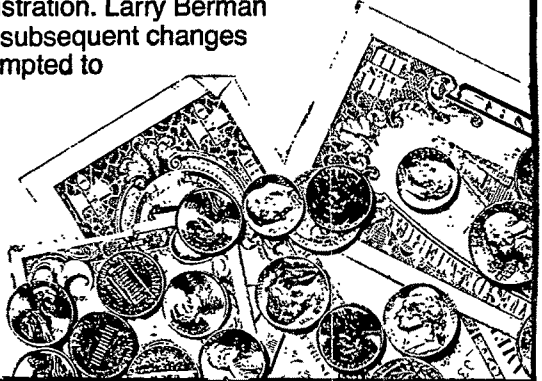
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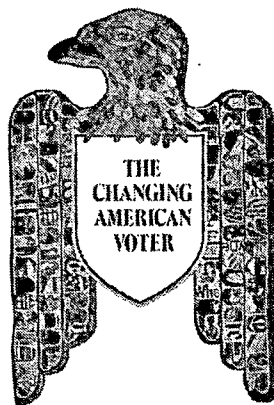
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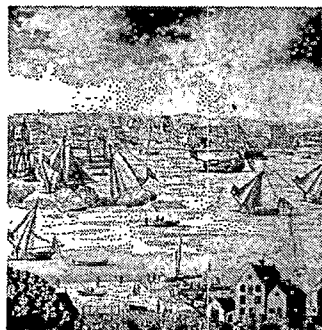


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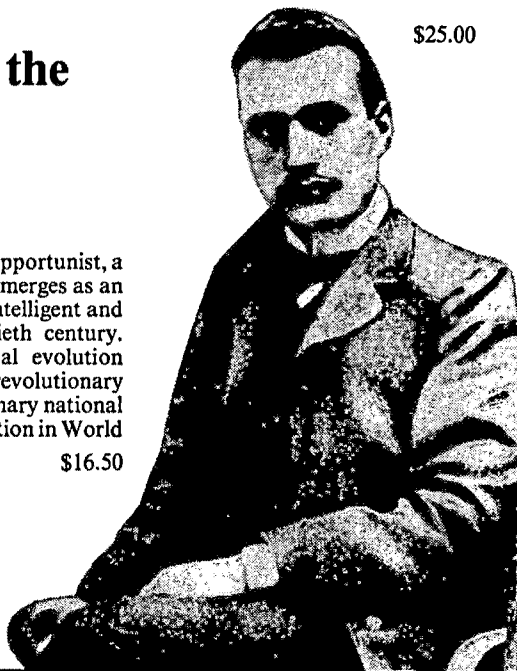
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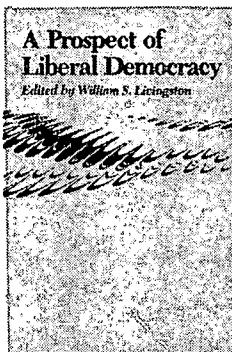
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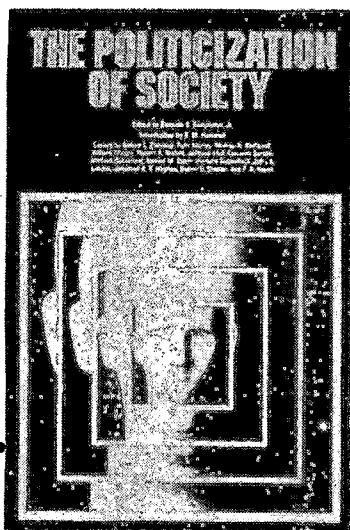
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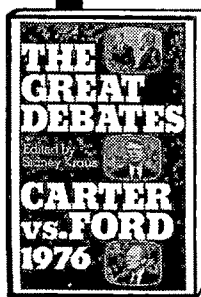


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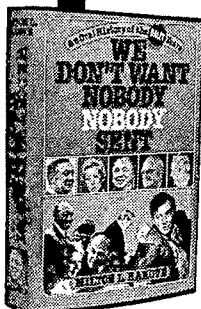


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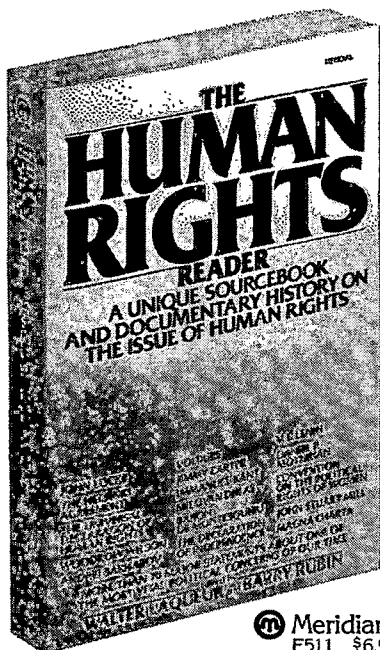
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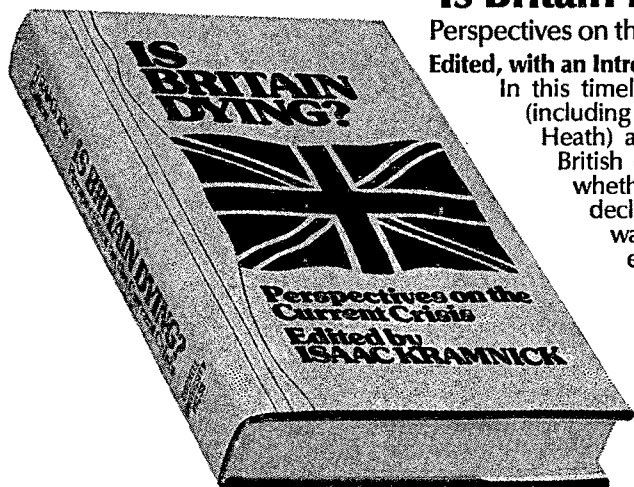
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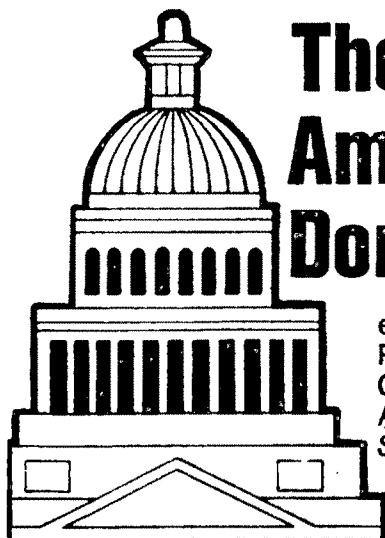
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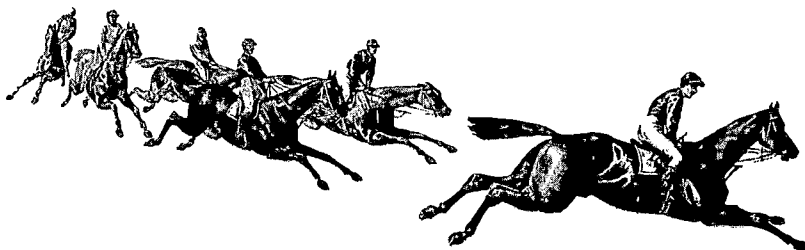
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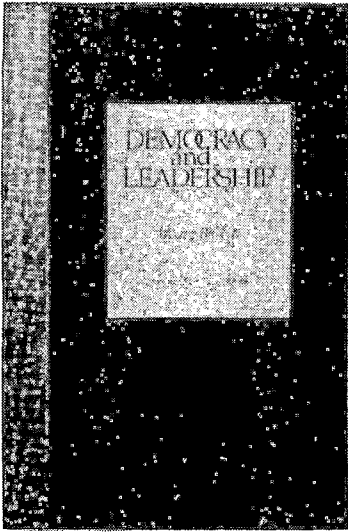
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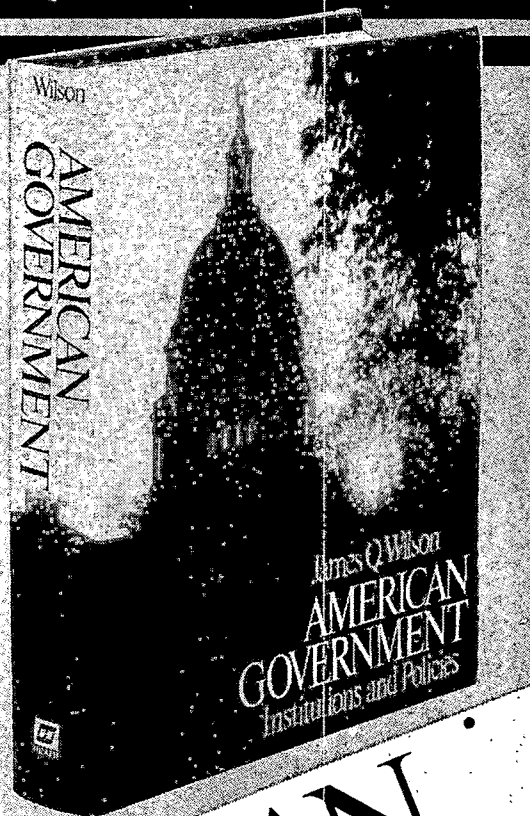
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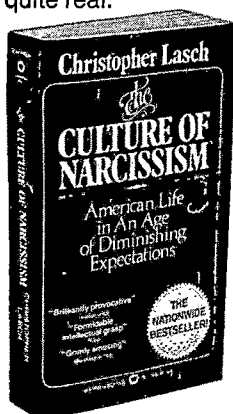
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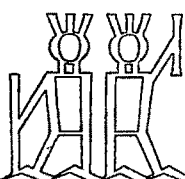
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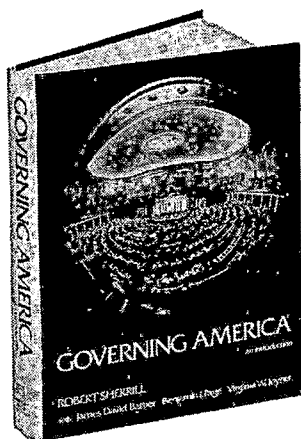
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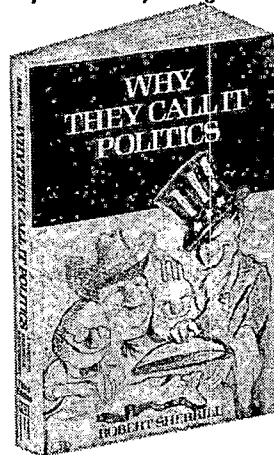
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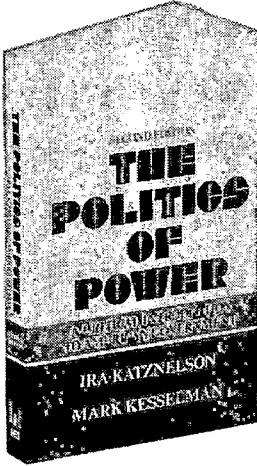


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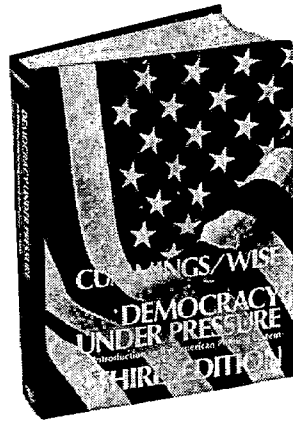
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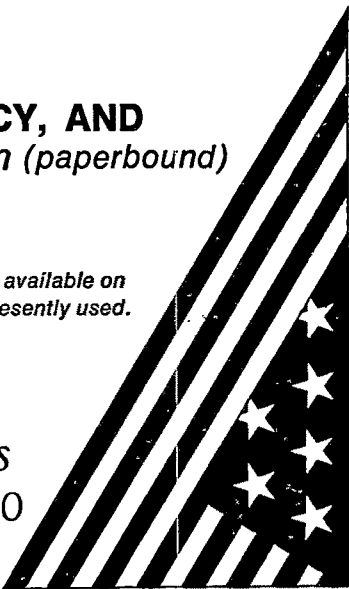
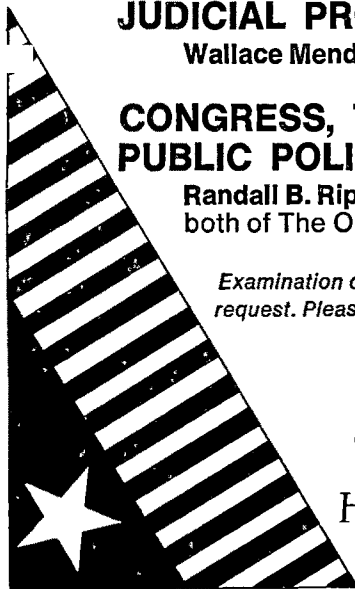
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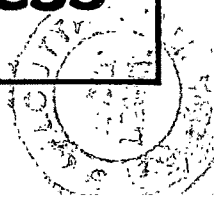
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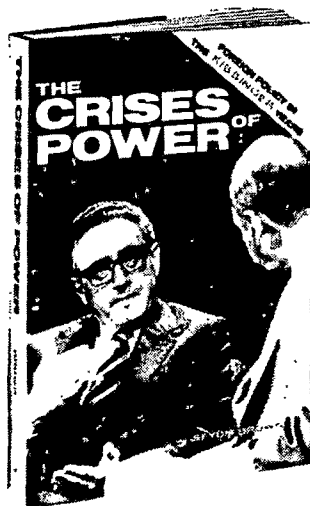


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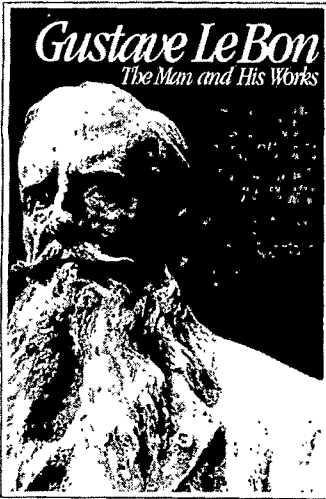
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